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THRESHOLDS FOR THE PERCEPTION OF DIFFERENCE IN FACIAL EXPRESSION AND ITS ELEMENTS

By Francis W. Irwin, University of Pennsylvania

No one can be so averse to analysis as to deny that there are degrees of generality among perceptions—for example, that one may perceive a tree and that he may also perceive the trunk of the tree, or a branch, or a leaf on that branch, or a vein in that leaf. In the same way, it is possible to perceive the expression of a face as a whole, or the peculiarities of the contours of the features—curve of lips, lines about the eyes, mouth, forehead, etc.—which are present at the same time. Now, one may say that a particular facial expression presupposes the existence of a certain disposition of these curves and lines upon the face, and no more assert that the expression *is the sum of them* than we should affirm that a tree is merely the sum of the trunk, branches, and leaves, or even of the cells into which we may analyze these parts under the microscope. We may call the expression the more general, and the curve of the lips or the creases at the corners of the eyes or the lines in the forehead the more particular, perception in this instance. But, if the perception of variations in expression be admitted to presuppose the *existence* of variations in these particulars, must it also presuppose the *perception* of them?

It is obvious that in the case, let us say, of a lump of metal the perception of the lump does not mean the perception of the indefinitely minute parts into which it can be divided; but this is not a correct analogy, since a smile is not composed of a large number of tiny smiles. Is it not possible, however, that a smile might be perceived

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when the changes in the curves and lines essential to it are imperceptible—reminding us of the visible grin of an invisible Cheshire cat? Common opinion seems ready to agree that such a thing not only is quite possible but happens continually,[1] that we often perceive things which can be analyzed into parts of a heterogeneous nature, parts which are not perceived singly; but of course our question is: *Can* they be perceived singly, whenever the more general perception is possible?

The experiment here reported deals with the difference thresholds of these two perceptions. The problem may be stated: to investigate the relationship between the thresholds for the perception of differences in facial expression and those for the perception of differences in certain elements of the expression. Sander in a review of the experimental results of the *Gestalt* psychology has indicated that the configuration in which a particular element is found may have an important bearing upon the difference threshold for this particular. He states that "die Unterschiedsschwellen abhängig sind von den jeweiligen Gestaltbedingungen, ... die Empfindlichkeit für Gestaltveränderungen im allgemeinen grösser ist als für Veränderungen isolierter Gegebenheiten." For example, "die Unterschiedsempfindlichkeit für Distanzveränderungen zweier Punkte ist erheblich gröber, wenn sie isoliert, als dann, wenn sie etwa als Pupillen eines Gesichtes dargeboten werden."[2] Similarly, Bühler reports that the difference thresholds for perception of the length of a line are higher than those for the perception of the difference between right angles in which this line forms the varying side.[3]

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[1] Cf. E. A. Singer, Jr., Studies in sensation and judgment, *Psychol. Rev.*, 4, 1897, 265. "It is well-known that where differences exist, comparatively slight in nature, between objects of considerable complexity of structure (such as human faces) one may be quite aware of a difference without being able to tell in what the difference consists." The conclusion of this experiment is that "the general judgment of difference has a lower threshold than the particular" (p. 271). The "general judgment" is defined as that judgment of difference "which cannot be explained as due to chance and to complete appreciation of the particular differences presented" (p. 270).

Cf. also Max Wertheimer's suggestion that a difference may be perceived without the ability to tell in what the difference consists. Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 4, 1923, 318.

Grace A. DeLaguna, Dualism and Gestalt psychology, *Psychol. Rev.*, 37, 1930, 200, says: "The little child . . . . . perceives and responds to a 'smiling face' before it has learned to see the curves of lip and cheek which compose it geometrically."

[2] F. Sander, Experimentelle Ergebnisse der Gestaltpsychologie, *Ber. u. d. X Kongr. f. exper. Psychol.* (Bonn), 1927, 46 f.

[3] K. Bühler, *Die Gestaltwahrnehmungen*, I, 1913.

So far as I have been able to ascertain, however, no one has experimented upon this relationship as it holds in the same figures with varying attitudes of the subject induced by different types of instructions.[4] It is toward this that the present experiment is directed. No attempt is here made to define "smiling," "frowning," or "neutral" expressions, but it will be shown that all the *S*s agreed in their choice of the direction of curvature which was toward smiling and that which was toward frowning, without any instruction upon the point. This agreement was sufficient for the purposes of the experiment.

METHOD

Apparatus. Five sets of cards, A, B, C, D, and E were used, each set consisting of 3 identical series of 7 cards. The cards were 91 mm. squares of stiff cardboard with a white unglazed surface.

Material A consisted of cards upon which were diagrammatic ('Leonardo') faces. Examples are given in Plate 1. A circle 46 mm. in diam. was in the center of each card: the nose was represented by a straight line 7 mm. long with its lower end at the center of the circle; two horizontal lines (straight or curved) 10 mm. long and 10 mm. apart on a line 8 mm. vertically above the center of the circle represented the eyes; while a horizontal line (straight or curved), 10 mm. in length and 14 mm. vertically below the center of the circle, represented the mouth. All these lines were of the same breadth, approximately 'one point face' (0.35 mm.). The variations in the 7 different cards in each of the 3 series composing this set may be described as follows:

Card 1—eyes and mouth arcs of 3 cm. radius concave upwards;
Card 2—eyes and mouth arcs of 6 cm. radius concave upwards;
Card 3—eyes and mouth arcs of 12 cm. radius concave upwards;
Card 4—eyes and mouth straight lines;
Card 5—eyes and mouth arcs of 12 cm. radius concave downwards;
Card 6—eyes and mouth arcs of 6 cm. radius concave downwards; and
Card 7—eyes and mouth arcs of 3 cm. radius concave downwards.

Thus we have a series from greatest 'upward' curvature through the straight line to greatest 'downward' curvature. This material may be referred to as the *full face.*

Material B differed from Material A only in that here the mouth alone varied, the eyes remaining straight. This material may be called the *mouth face.*

Material C consisted of cards upon which there was but a single horizontal line, in the center of the card. This line was 10 mm. long and varied in curvature just as did the varying lines of Materials A and B.

Material D consisted of the cards of Material A, inverted.

Material E consisted of the cards of Material B, inverted.

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[4] R. K. White and Carney Landis have measured the least perceptible differences of variation in a silhouette, but seem not to have attacked the problem of the threshold for differences in facial expression. See The perception of silhouettes, this JOURNAL, 42, 1930, 431-435.

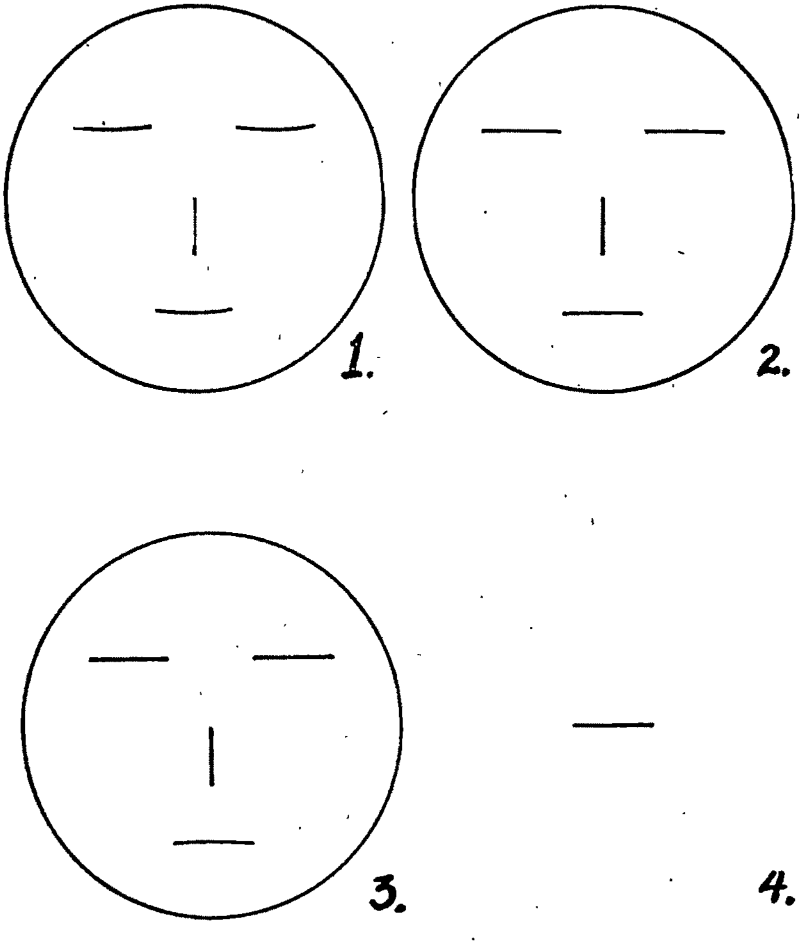

Plate 1. Materials

Key: 1 = Card 1, Material A; 2 = Card 5, Material A; 3 = Card 6, Material B; 4 = Card 4, Material C. The figures are reproduced actual size, but, when used, each figure was centered on a card 91 mm. square.

These figures were originally drawn full size by a professional draughtsman, and were then photo-engraved, with the use of a prism to prevent distortion, finally being printed upon the cardboard in black. The cutting was done by machine.

Lest dust or smudges of any kind lead to recognition of particular cards a large number were at hand, and sets were often replaced. Several times a *S* reported the belief that he could recognize a particular card; he was then told to report when next he saw the card, but in no instance was he successful in doing this.

The cards were exposed for a period of 0.18 sec. by means of a Whipple tachistoscope.[5] Care was taken to keep constant the lighting of the instrument.

The experiment was carried on from December 17, 1929, to May 21, 1930. Until February 3 the photographic dark-room of the Laboratory of Psychology at the University of Pennsylvania was used; it was then necessary to move to a room in the Hare Laboratory of Chemistry where lighting conditions were considerably different. However, no variations in the results that could be attributed to this change were noticed.

Procedure. The instructions used are presented below. It will be noted that there are two general types, (I) for 'curvature' and (II) for 'expression.' Type I necessarily varied slightly according to the material employed. Type II was the same for each material.

Instructions IA. You will be shown a series of diagrams of faces, each diagram being given but a short exposure. In some of the diagrams the eyes and mouth will be arcs turned upwards at the ends, in others, arcs turned downwards, and in others they will be straight lines. You are to make an immediate judgment for each diagram, reporting the eyes and mouth to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each diagram and to state it immediately. I shall say 'ready' a short time before the exposure of each face, and 'now' just as it is about to appear.

Instructions IB. You will be shown a series of diagrams of faces, each face being given but a short exposure. In some of the diagrams the mouth will be an arc turned upwards at the ends,[6] in others an arc turned downwards, and in others it will be a straight line. You are to make an immediate judgment for each diagram, reporting the mouth to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each diagram and to state it immediately. I shall say 'ready' a short time before the exposure of each face, and 'now' just as it is about to appear.

Instructions IC. You will be shown a series of lines, each line being given but a short exposure. Some of the lines will be arcs turned upwards at the ends, others, arcs turned downwards, and others will be straight lines. You are to make an immediate judgment for each line, reporting it to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each and line to report it immediately. I shall say 'ready' a short time before the exposure of each line, and 'now' just as it is about to appear.

Instructions ID. You will be shown a series of figures, each figure being given but a short exposure. Included in each figure will be three horizontal lines, some of which will be arcs turned upwards at the ends, others, arcs turned downwards, and others will be straight lines. You are to make an immediate judgment for each figure, reporting the lines to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each figure and to report it immediately.

It may be remarked that calling the inverted face a "figure," as in the preceding and following instructions, did not at all succeed in abolishing the idea of "face." Each of the 5 *Ss* thought it a good joke as soon as the "figure" was shown.

Instructions IE. You will be shown a series of figures, each figure being given but a short exposure. Included in each figure will be three horizontal lines. The lower two will always be straight, but the upper one, in which you are interested, will sometimes be an arc turned upwards at the ends, sometimes an arc turned downwards, and sometimes a straight line. You are to make an immediate judgment on each figure, reporting this line to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each figure and to report it immediately.

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[5] Described in G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 264.

[6] "Upwards at the ends," etc., seemed the simplest way of indicating the direction of curvature of the arcs, a rather awkward thing to make clear without danger of confusion. The *Ss* seemed to understand perfectly what was meant.

Instructions II. You will be shown a series of diagrams of faces, each face being given but a short exposure. For each face you are to make an immediate judgment, as to whether it is smiling, frowning, or neutral. Be sure to make a judgment on each diagram and to state it immediately, remembering to judge only as to whether the face is smiling, frowning, or neutral, using the terms 'smile,' 'frown,' or 'neutral' in your report. I shall say 'ready' a short time before the exposure of each face, and 'now' just as it is about to appear.

The appropriate instructions were read to the *S* at the start of each experimental period, the sentence concerning the 'ready—now' signals being omitted after several sittings.

The *S* sat at a constant distance from the tachistoscope; the front legs of the chair were 2 m. from the machine. The 'ready' signal was given about 2 sec., and the 'now' signal 1 sec., before the exposure of the card.

Two orders of presentation of the 21 cards in a set of material were employed. The first was used while obtaining the first two difference-thresholds, and thereafter the first and second were alternated for each DL, the alternation beginning with the second order. The orders were as follows (the numbers being those of the cards as given above in the description of the material). First: 3, 1, 4, 7, 6, 2, 5, 1, 3, 7, 2, 4, 6, 5, 2, 6, 4, 7, 3, 1, 5; Second: 5, 7, 3, 1, 4, 2, 6, 7, 2, 4, 5, 1, 3, 6, 1, 4, 6, 3, 7, 5, 2. The use of these two orders precluded the possibility of the learning of the order of presentation. No *S* was able to demonstrate his having learned even a part of a series, though several tried to do this at various times.

Eleven DL were obtained from each *S*. Each DL was calculated from 50 observations of each of the 7 different cards in a set of material, with the exception that in the following cases 100 judgments of each card were obtained: *A*—the first 4 DL; *C* and *H*—the first 2 DL; *Ot* and *R*—the first 3 DL.

Thus, the results include for the 5 *Ss* a total of 24,150 judgments. From 217 to 231 judgments were obtained at each experimental session. No DL was completed in a single sitting, and the exceptions noted above required three. An intermission of from 5 to 10 min. was introduced when each session was about half completed, usually after 6 series of 21 judgments had been made. An experimental period, including the intermission, usually lasted about 50 min.

Subjects. The *Ss* in this experiment were Mr. Leon Arons (*A*), Dr. Stanley F. Cooper (*C*), Dr. John F. Humes (*H*), Mr. Jay L. Otis (*Ot*), and Mr. Thomas W. Richards (*R*)[7]. With the exception of *A*, a graduate student in psychology, the *Ss* were members of the Department of Psychology at the University of Pennsylvania. *A*, *C*, and *H* were already familiar with tachistoscopic procedure; *A* and *C* had served in the preliminary experiment carried on to determine the details of the method to be followed in the main experiment, while *C* and *H* had been

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[7] The author desires to acknowledge his indebtedness to these friends for their patience, and to Dr. G. H. Clark and Messrs. W. L. VanBuskirk and F. R. Gillespie, who served in the preliminaries to this experiment; and in particular to Professor Samuel W. Fernberger for his constant direction and encouragement.

TABLE I

Showing, for Every *S*, the DL and Measures of Precision for Every One of the Various Experimental Conditions

S		1	2	3	4	5	6	7	8	9	10	11
	Condition	IA	IIB	IC	IIA	IB	ID	IIA	IIA	IE	IC	IIA
	DL	1.0	1.5	1.0	1.3	0.9	0.5	1.6	1.2	0.2	0.3	1.3
A	h_1	0.30	0.26	0.37	0.29	0.38	0.52	0.47	0.40	0.43	0.54	0.45
	h_2	0.25	0.24	0.37	0.26	0.35	0.51	0.42	0.41	0.42	0.55	0.38
	Condition	IIA	IB	IIA	IC	ID	IIB	IA	IIA	IB	IIA	IE
	DL	1.4	1.4	1.2	1.1	1.3	1.2	1.1	0.8	0.7	0.6	1.3
C	h_1	0.25	0.34	0.41	0.44	0.59	0.36	0.45	0.43	0.46	0.55	0.34
	h_2	0.30	0.35	0.41	0.48	0.55	0.39	0.54	0.41	0.47	0.71	0.45
	Condition	IB	IIA	ID	IC	IIB	IA	IIA	IE	IIA	ID	IIB
	DL	1.3	1.0	1.0	1.5	0.7	0.8	0.6	0.7	0.6	0.8	0.7
H	h_1	0.24	0.33	0.43	0.54	0.31	0.52	0.39	0.40	0.41	0.49	0.44
	h_2	0.22	0.35	0.45	0.66	0.39	0.52	0.43	0.34	0.42	0.56	0.47
	Condition	IC	IIB	IIA	ID	IB	IIA	IA	ID	IIA	IE	IA
	DL	1.0	0.7	0.4	0.4	0.8	0.6	0.5	0.7	0.7	0.8	0.6
Ot	h_1	0.37	0.36	0.62	0.87	0.45	0.64	0.62	0.91	0.94	0.51	0.69
	h_2	0.37	0.34	0.64	1.01	0.64	0.65	0.68	0.81	0.45	0.65	0.98
	Condition	IIB	IA	IIB	ID	IIA	IE	IC	IIA	IB	ID	IIA
	DL	2.7	2.2	3.3	2.3	2.6	2.6	2.4	2.3	2.6	2.6	2.3
R	h_1	0.30	0.31	0.37	0.52	0.60	0.45	1.03	0.79	0.51	1.87	0.67
	h_2	0.19	0.32	0.21	0.46	0.55	1.17	0.83	0.84	0.79	0.63	0.37

Ss in other experiments in which the tachistoscope was used. The *Ss* were examined by an oculist, who reported normal or normally corrected vision in each case. None of the *Ss* had any information as to the purpose of the experiment.

RESULTS

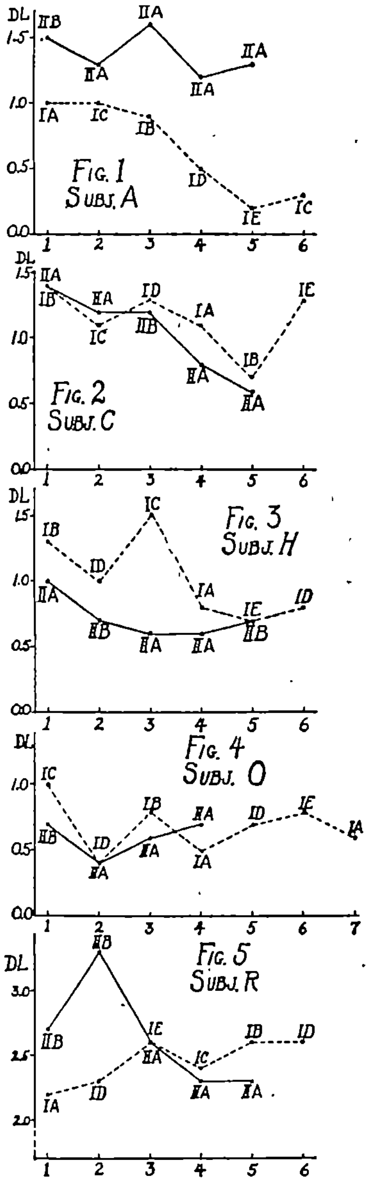
The results obtained by this method were treated in accordance with the phi-gamma hypothesis. Eleven difference-thresholds were obtained for each *S*. These DL and their coefficients of precision h_1 and h_2 are incorporated in Table I. Table II gives the DL for each *S* arranged as to the conditions (instructions and material) under which they were obtained.

TABLE II

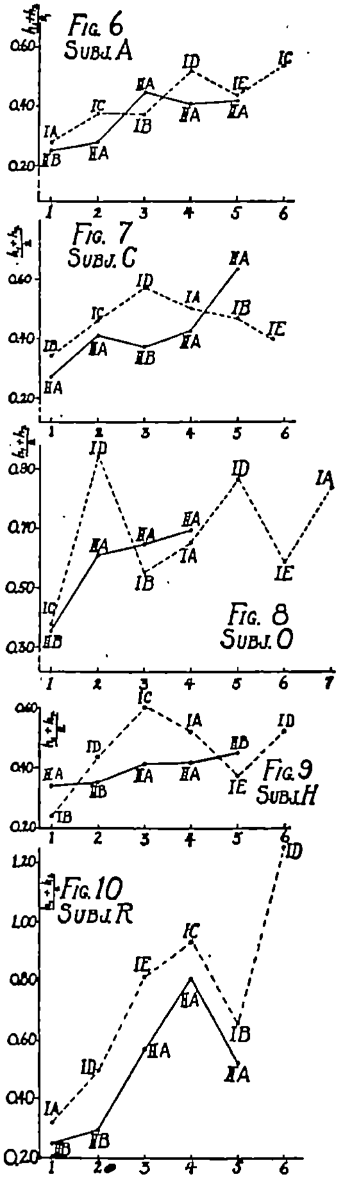
SHOWING, FOR EVERY *S*, THE DL ARRANGED ACCORDING TO THEIR EXPERIMENTAL CONDITIONS

	Conditions						
S	IA	IIA	IB	IIB	IC	ID	IE
A	1.0	1.3 1.6 1.2 1.3	0.9	1.5	1.0 0.3	0.5	0.2
C	1.1	1.4 1.2 0.8 0.6	1.4 0.7	1.2	1.1	1.3	1.3
H	0.8	1.0 0.6 0.6	1.3	0.7 0.7	1.5	1.0 0.8	0.7
Ot	0.5 0.6	0.4 0.6 0.7	0.8	0.7	1.0	0.4 0.7	0.8
R	2.2	2.6 2.3 2.3	2.6	2.7 3.3	2.4	2.3 2.6	2.6

In Figs. 1 to 5 are represented the DL for each *S*. The broken lines connect those for 'curvature' and the solid lines those for 'expression.' The ordinates represent the amount of the DL, and the abscissae give the order of succession of the various DL under each set of instructions. The notations at each point give the conditions



Figs. 1-5. DL for Every *S*



Figs. 6-10. Coefficients of Precision for Every *S*

Broken line connects values for 'curvature;' solid line connects values for 'expression.'

under which that DL was obtained. Thus in Fig. 1 we find that *A*'s first 'expression' limen, obtained with Material B and Instructions II, amounted to 1.5, while his second 'expression' limen, with Material A and Instructions II, amounted to 1.3. Figs. 6 to 10 are a similar representation of the coefficients of precision, the average of h_1 and h_2 for each DL being graphed. In calculating the DL no absolute units of curvature were used. The stimulus intervals were regarded as equal to unity; hence the magnitudes of the DL and the h are in this sense arbitrary.

TABLE III

JUDGMENTS OF MATERIAL A UNDER CONDITIONS UNRESTRICTED AS TO TIME AND DISTANCE

	Subject				
	A	*C*	*H*	*Ot*	*R*
UP	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3
DOWN	5, 6, 7	5, 6, 7	5, 6, 7	5, 6, 7	5, 6, 7
STRAIGHT	4	4	4	4	4
SMILE	1, 2	1, 2, 3	1, 2, 3	1, 2, 3	1, "perhaps" 2
FROWN	7	5, 6, 7	5, 6, 7	5, 6, 7	7
NEUTRAL	3, 4, 5, 6	4	4	4	3, 4, 5, 6

In some cases the *S*s were asked to write introspections, although no suggestion as to their content or purpose was offered. Quotations from some of these reports will be presented in the discussion; and in view of their importance it is to be remembered that they were absolutely undirected.

At the conclusion of the experiment, a complete series of the 7 different cards of Material A was laid before each *S* individually, after the cards had been shuffled. *S* was then asked to judge each card as 'up,' 'down,' or 'straight,' an indefinite amount of time and as close observation as desired being permitted. When these judgments had been made, the cards were again shuffled, laid before the *S*, and judgments of 'smiling,' 'frowning,' or 'neutral' requested. The results of this experiment are recorded in Table III.

DISCUSSION

In the first place, it is obvious that under the conditions of the experiment the middle category *neutral* is essentially more elastic than the middle category *straight*. No line which is perceived as curved can rightly be judged as *straight*; yet it is quite in harmony with the instructions, since no criterion of expression was imposed, to judge as *neutral* faces in which curvature of the lines was clearly perceived. The *neutral* category was also a much more convenient place to unload all doubtful judgments than the category *straight*. That at least 2 *S*s very probably took advantage of this elasticity is evident from Table III. *C*, *H*, and *Ot* under the conditions described judged as *smiling* each card perceived also as *up*, as *frowning* each card perceived as *down*, and as *neutral* the card perceived as *straight*. But though *A* and *R* were as accurate as the others in their judgment of curvature, they were not willing to attribute expression to all faces in which curvature was perceptible. Thus, *A* judged card 4 alone as *straight*, but cards 3, 4, 5, and 6 as *neutral*; while *R*, also judging card 4 alone as *straight*, judged cards 3, 4, 5, and 6 as *neutral* and card 2 as "perhaps" *smiling*.[8] The natural result of these tendencies, if carried into the main experiment, would be to make the DL for expression relatively greater than those for curvature in the case of these 2 *S*s. This presumption can be seen from Table II to be verified for *A*, whose DL were in general quite low; but it is doubtful as far as *R* is concerned—possibly because of the magnitude of his DL, which indicates his having had much difficulty in perceiving any differences at all except at the extremes of the stimulus series. From these considerations we may conclude that the experiment from its very conditions favored lower DL for curvature than for expression.[9]

'Learning,' as indicated by a downward tendency of the thresholds or by an upward tendency of the 'precision' might be due chiefly, first, to general adjustment of *S* to the conditions of the experiment; and, secondly, to the attainment of greater stability in his criteria of judgment. The first tendency might reasonably be expected to show itself in large measure only during the early part of the experiment, for 3 sittings of almost an hour apiece were required for each of the first 3 or 4 DL for each *S*. (Besides, *A* had observed in a period of 4 sittings, and *C* in 6 sittings, during the preliminary work.) New types of material were introduced, however, at various times, so that the difficulty of establishing criteria should have been somewhat extended, perhaps over the whole course of experimentation.

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[8] It may be noted that there was complete agreement as to the extremes and the middle, card 1 being in all cases judged "smiling," card 7 "frowning," and card 4 "neutral." This relationship held also in the main experiment, the upper threshold always being that for "smiling" and the lower that for "frowning," while the point of subjective equality varied little from 4.

[9] Cf. S. W. Fernberger, The use of equality judgments in psychophysical procedures, *Psychol. Rev.*, 37, 1930, 110 f.: "This is the real difference between the equality and the difference judgments. The difference judgments are much more 'objective' in nature. The equality judgments are more dependent on the attitude of the observer, on the form of instruction, on the observer's understanding of instructions and, as Angell pointed out, to an extent, at least, on his basic temperament."

As indicated in Figs. 1 to 5, these tendencies were variable. *A* shows a marked trend toward greater stability of criterion in the limens of curvature, and a much slighter one in the thresholds for expression. The advantage at both start and finish lies with curvature.

C shows a trend toward increased stability for expression which is emphatic; his series for curvature is more uncertain, though it is in general in the same direction except for a reversal in the case of the last DL. Here the advantage is with expression after equality at the start.

In the case of *H*, there is a more evident improvement in the criteria for curvature than in those of expression. The last 3 DL for each are practically constant.

The tendencies of *Ot* are indecisive. A sharp drop at the beginning is more or less neutralized thereafter.

R's criteria for expression show great improvement with the introduction of the *full face*, but his series for curvature has a general upward movement—the only case in the results which indicates decreasing precision of criteria.

In brief, then, in two cases, *A* and *H*, the criteria of curvature show a greater tendency toward precision than those of expression; for two other cases, *O* and *R*, the expression series shows more marked gain in stability than does the curvature series; *Ot*'s results show little improvement for either; and there is one case of a trend toward decreased stability, *R*'s curvature series.

It must already be evident that in spite of the large number of judgments upon which the DL are based, the variability is of such magnitude as to preclude statistical reliability for the differences among DL. The results must, then, be taken as suggestive rather than conclusive, certain tendencies being at least sufficiently marked to justify their being discussed.

The coefficients of precision (h_1 and h_2) in all cases show a general tendency toward improvement, as may be seen in Figs. 6-10. The curvature judgments have on the whole considerably less scatter for each *S* than have the expression judgments; but, on the other hand, for *C*, *H*, and *Ot* the precision increases more steadily for expression than for curvature, while for *A* and *R* the two increase at about the same rate. In general, the precision for curvature, while greater than that for expression, is also more variable.

Since the variability is so great, it is difficult with 5 different types of material and only 11 DL for each *S* to determine the effect of these differences of material. The *full face*, whether upright or inverted, seems to give lower limens than the *mouth face* or the *single line*; but as between the two latter there appears little difference, excepting in the case of the *mouth face* with instructions for expression, which gave in general the highest DL. From Table II it can be seen that only for *H* did the *mouth face* with instructions for expression rank higher than fourth among the 7 different conditions, and that in two cases it ranked last. Evidently the *mouth face* is not as expressive as the *full face.*

One would likely suppose *a priori* that the thresholds would be lower when there were 3 lines by which to judge instead of one, and that it would be easier to judge the expression of a face with eyes and mouth varying than that of a face with the mouth alone varying. The fact that, in spite of the variability and of the small number of DL relative to the number of conditions, the indications are in line with such apparently reasonable expectations justifies confidence in the reliability of the results.

Perhaps the most interesting comparison is that concerning the effects of the two types of instructions, those for curvature (Instructions I) and those for expression (Instructions II). Fig. 1 shows *A*'s results. The successive DL under instructions to judge curvature are connected by the broken line, and those for expression by the solid line. It can be seen that *A*'s thresholds for curvature are considerably lower than those for expression, for there is not even an overlapping of the two curves. That *A* should have given such results is quite in accord with the fact that he tended to demand an easily perceptible curvature before he would judge the face as having any but a *neutral* expression. His introspections give some cues as to his criteria. After his first observation period, under conditions IA, he wrote:

> I usually fixated the left eye. . . . . It was most infrequently that I saw the mouth. . . . . Occasionally both eyes appeared distinct—at times but one.

Under conditions IIB (his second threshold) he wrote:

> I find myself using at least three criteria: (a) distance between eyes and mouth if great usually a frown,—if very short, a smile,—in between, guess frown or smile or neutral; (b) whether mouth was curved up or down: up—a smile, down—a frown; (c) length of mouth: if very short or curved (noticeably down), a frown; medium even when slightly curved, neutral; long, if curved up, a smile, if curved down, a frown. I used these (and others I don't remember) singly, together, etc., now one, now another.

The "medium, even when slightly curved, neutral" is significant, as is the evident difficulty he had in establishing a criterion of expression. No other light upon his criteria appears in his introspections.

Ot's results (Fig. 4) are not clear-cut, though what advantage there is appears to be in favor of expression. No introspective evidence as to his criteria is available.

It will be remembered that at the end of the experiment *R* showed, as did *A*, the tendency to demand much apparent curvature before judging the face as expressive. *R*'s thresholds (Fig. 5) are by far higher than any of the other *Ss*', both for expression and for curvature; the upper and lower thresholds are near the limits of the stimulus series. His first two expression limens, with the *mouth face*, are considerably above the first two curvature limens, but, when the *full face* was introduced, the DL for expression dropped and fell below the succeeding curvature DL, though never quite reaching the low level of the first curvature DL. In an introspection upon the first DL (IIB) he stated:

> Usually I fixate the mouth, often the eye-brows, but I try to see the thing as a whole.

On the second DL (IA) he wrote:

> Since starting on the second series several things have occurred to me. I did not have a clear idea of the precise criterion for judging U, D, or S but it gradually became apparent. The first few times I judged them by the curvature of the single eyebrow, usually by the one on my left. Today I found that I was judging by looking at the eyebrow on my right, and at the right end. I tried to use the same method I had been using but could not seem to do so. Perhaps a limited distribution of attention prevents me from seeing the image as a whole and judging from that, for I cannot seem to do so in the short interval allowed.

C's results (Fig. 2) show a tendency in favor of keener sensitivity for expression than for curvature, after approximate equality at the start. Apparently enjoying the experimental session, he would often, when under instructions to judge expression, make such exclamations as, "Oh, what an ugly frown!" or "What a nice broad smile!" We have noted that *C* served as an *S* in the preliminary study when the procedure for this experiment was being worked out. At the first sitting of this preliminary work he was judging the curvature of a single line, and his introspection at the close of the period is of interest. He wrote:

> Only a short space before the end of this experimental period I first associated the idea of *mouth* with a downward curve, and the thought 'sour-stomach' came. After that the idea of *mouth* connoting *face* came and the idea that there *should be* a face surrounding the oral groove. [*C*'s italics.] Several times thereafter a mild feeling of dissatisfaction was aroused by the fact that I could not *visualize* a facial setting. The 'mouth' association—as soon as it appeared—seemed to facilitate the perception of curves.

All this was written before he had seen or known anything about the face material. Later, after instructions to attempt to forget the *mouth* association he reported that it appeared very seldom. In the main experiment he gave the following introspections:

> First DL (IIA): "At first I began using the eye-brows as my fixation point but after 4 or 5 exposures decided that mouth would be more satisfactory criterion—if 'up,' smile; 'down,' frown; 'level,' neutral. . . . . Principal criterion *mouth*."
> Fifth DL (ID): "Noticed some confusion at first with the new type of material—'figures.' The Gestalt of '*face*' was so well established that the idea 'face upside down' came into consciousness immediately and persisted until the observation period was about three-quarters thru, when I became aware that 'face' was gone and I was observing simply two horizontal lines—curved or straight—on the lower part of the card. On the first two cards I tried to get my cue from the 'mouth;' finding this unsatisfactory I adopted the 'eyes' as my criteria and continued so until the end of the day's observation."

The most definite case of lower DL for expression than for curvature was that of *H*, as can be seen from Fig. 3. His introspections are correspondingly interesting.

First DL (IB): "Instead of making my judgments solely upon the direction of curvature, I frequently made them upon the basis of the 'general impression' which the individual stimulus left upon me. If the face looked pleased, it was 'Up;' if it looked displeased or as if it wore a frown, the judgment was 'Down.' If the face appeared to have neither of these expressions, it (the judgment) was 'Straight.' [Note that this was written without *S*'s knowing that he would later be asked to judge expression.] After the intermission I tried to hold myself more rigidly to the judgment of curvature alone, and during the remainder of the period I felt that I was making a greater number of 'Straight' judgments than before. [Not in accordance with the facts—the relative number of 'straight' judgments was almost exactly the same for the periods before and after the intermission.] The degree of subjective assurance was considerably less; and I felt that while fixating the 'mouth,' the other features were getting into consciousness by indirect vision, and thus influencing my judgment, somewhat as previously, by the method of 'general impression.'"

First DL (IB)—second sitting: "I tried to make all my judgments on the basis of curvature of mouth alone, but, as before, I felt that in many cases I was influenced by the 'general impression' which the diagram made. I found it difficult sometimes to fixate the mouth alone, as that frequently caused a blurring and I could report neither 'up,' 'down,' nor 'straight.' It seemed, in some exposures, that not only was the mouth curved, but the eyes as well. This circumstance is probably a function of the whole *Gestalt* (*i.e.* pattern) itself."

Second DL (IIA): "The change in *Aufgabe* today had the effect of modifying the *Einstellung*, in that a specific judgment as to curvature of the mouth was not required, but rather a judgment of *general impression*, in terms of 'smile,' 'frown,' or 'neutral.' As other features than the mouth may contribute to these characteristic expressions, the whole facial pattern was involved in my responses. The exposures are, of course, too short to permit of anything like analysis, and, as I can not, at least not often, re-image the diagram, the response must be made on the basis of something like 'snap' judgment. While I cannot be certain that other features besides the mouth are contributing to the 'general impression,' I sometimes believe they are. However, I am sure that the curvature of the mouth, when definitely perceived, is the deciding factor in many of my judgments."

Third DL (ID): "While the reversal of the figure minimized the apperceptive tendency to interpret it as a face, yet it was still a *face*; and the criterion, which has characterized the majority of my judgments, namely, a rather indefinite 'general impression,' seemed still to operate. To me, this belief was reinforced by the fact that I experienced considerable confusion at first in reporting the judgments. If the horizontal lines were turned down on the inverted figure, I had the impression of a 'smiling face,' which previously had been reported 'up,' or 'smile.' And on several occasions, these were my reports, which I immediately asked to have changed to 'down.'"

Since these introspections were written not as the result of specific questioning, but merely under instruction to write anything *S* deemed important (*H*, indeed, wrote several of his without being asked to do so), it seems significant that the two *S*s, *H* and *C*, for whom the DL for expression tended definitely to be lower than those for curvature, should have been just as definitely the only two for whom the effect of expression in the diagrams was very striking. Further, the fact that *any* of the *S*s should have given lower DL for expression than for curvature is rather astonishing, when we remember our consideration of the *neutral* category as compared with the relative inelasticity of the *straight* category. Hence, while the inconclusiveness of the results from the statistical point of view must be admitted, the writer nevertheless believes it strongly suggested that the difference threshold for perception of facial expression in diagrams *can* be lower in the same individual than that for the perception of curvature.

From results as indecisive as those of the present experiment it would be extremely rash to draw inferences of a general nature; yet, because of the importance of the problem to the theory of perception, it is well to indicate briefly what would be implied in the outcome of such an experiment if it *were* possible to generalize. In the case that results (like *A*'s) were the rule, we should conclude that an attitude predisposing *S* to perceive the parts rather than the whole of a configuration favors the lowering of the differential limen, which very likely would be quite acceptable to the adherents of atomistic hypotheses. If, again, we found no valid differences to exist in the results (such as *Ot*'s), the descendants of Locke would be nothing dismayed. But how would this school explain lower thresholds for the perception of the configuration as a whole than for the perception of any part of that same configuration (toward which tend the results of *R*, *C*, and *H*)? What is present to assist the perception of the configuration, and not present to aid the perception of a part, unless the configuration itself?

CONCLUSIONS

(1) Thresholds for the perception of difference in facial expression and in the curvature of lines were obtained with diagrammatic material. A high degree of variability is present in the results.

(2) 'Practice effect'—the tendency toward progressive lowering of the DL—was present, though relatively slight.

(3) The 'precision' in general increased during the course of the experiment, and the increase was greater, though more variable, for curvature than for expression.

(4) The 'precision,' as a rule, was greater for curvature than for expression.

(5) The middle category of the expression judgments (*neutral*) was found to be more elastic than the middle category of the curvature judgments (*straight*).

(6) For one *S*, DL for curvature were consistently lower than those for expression. For another, the results were indecisive. For a third, the DL were lower at first for curvature but later for expression For two *S*s, the DL for expression were in general lower than those for curvature; and these two were shown by introspective evidence to have had strong tendencies toward perception of the face as a 'whole.'

(7) All *S*s agreed without instructions upon the direction of curvature present in the 'smile' and in the 'frown' respectively.

(8) The DL were lower, on the whole, for material in which three lines varied than for that in which but one line varied. This was particularly true in the DL for expression.

THE DISCRIMINATION OF SHORT INTERVALS OF TIME BY FINGER-TIP AND BY EAR

By PEARL FARWELL GRIDLEY, Northwestern University

The problem of comparing the various senses in respect to the discrimination of short intervals of time is an old one in the history of experimental psychology. Included in Czermak's program for experimentation in time perception, drawn up in 1857, is the suggestion that the shortest interval of time perceivable by the different senses be determined and that the senses be compared as to accuracy in judging the same time intervals.[1] This latter problem has never been systematically studied with any considerable number of subjects.

Most of the work in time perception has dealt with the sense of hearing alone. With one exception the early investigators were primarily or exclusively interested in this phase of the problem. Vierordt worked with vision and touch as well as hearing, but the data for each are scanty:[2] though some 2000 judgments are recorded they are spread over a large number of different time-intervals, with several different methods, dealing with the three different senses. All the early experimenters worked with few *S*s. Recent workers have used many *S*s but have confined their investigations to the sense of hearing.

The problem of comparing touch and hearing as regards time perception has acquired new interest in relation to Gault's work with the tactual discrimination of forms of speech.[3] He has demonstrated in a series of experiments with the teletactor that certain vowel, diphthongal, and consonantal qualities, as well as gross word and sentence patterns, may be distinguished through the finger tip.

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[1] H. Nichols, The psychology of time, this JOURNAL, 3, 1891, 503.

[2] K. Vierordt, *Der Zeitsinn*, 1868. Reviewed by Nichols, *op. cit.*, 506-7.

[3] See especially R. H. Gault, 'Hearing' through the sense-organs of touch and vibration, *Jour. Franklin Institute*, 204, 1927, 353; A partial analysis of the effects of tactual-visual stimulation by spoken language, *ibid.*, 209, 1930, 437-458; On the effect of simultaneous tactual-visual stimulation in relation to the interpretation of speech, *Trans. Illinois State Acad. Sci.*, 22, 1930, 630-653.

A recent study from his laboratory has shown that *S*s learn to distinguish sentences by the gross pattern[4]—pauses, accents, rhythm, and tempo—when no other criteria are present, and that when the gross patterns are the same, sentences can be distinguished, though less accurately, on the basis of vowels and consonants. It seems probable from the introspective reports of the *S*s that discrimination on the basis of consonants is at least in part discrimination on the basis of time; some consonants give a "clipped-off" effect, others a "drawn-out" effect. Gross sentence pattern involves a multiplicity of factors, one of which is time. It is possible that Gault's results, in as far as they indicate an advantage accruing to the deaf interpreter of spoken language in a situation where he simultaneously sees the face of the speaker and feels his words, may be due in part to the ability of the deaf *S* to distinguish intervals by aid of his tactual sense. These considerations bring up the old problem of time perception. How small an interval of time can be distinguished by touch? How does touch compare with hearing in this respect? It was suggested that if a technique could be developed by which the vibrations from the Seashore phonograph record for the measurement of the "Sense of Time" might be transmitted to *S* through the receiver of the teletactor a comparison might be made between the two senses, a comparison on the basis of a test already widely used in the case of hearing; and that data could be accumulated concerning the threshold for the discrimination of short intervals of time by touch. Such is the purpose of the present investigation.[5]

METHOD

The problem required that *S* should make judgments concerning the relative length of short intervals of time laid off by auditory and by tactual stimuli. It was essential that the time-intervals be accurate and be the same for the two conditions of stimulation, touch and hearing, and that such factors as intensity, pitch, and duration of the stimuli be the same and, as nearly as possible, constant throughout the experiment. To meet these requirements it seemed best that the source of energy producing the stimuli, and its transmission to *S*, should be the same for touch and hearing.

Source of stimuli, their nature, and order. The Seashore phonograph record for the measurement of the "Sense of Time" was used to produce the stimuli. This is one of the series "Measures of Musical Talent," Columbia record 53002 D,

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[4] S. F. R. Rose, The importance of pattern in learning sentences over the teletactor, unpublished Master's Thesis, Northwestern Univ., 1929.

[5] The author is indebted for the suggestion to Professor W. R. Roberts of Redlands University.

No. 3 A and No. 3 B. It was run on an electrically driven phonograph, and the vibrations were transmitted through an amplifier and telephone receiver and presented to *S* in an adjoining room as 'clicks' in the hearing condition, as 'impulses' against the finger-tip in the touch condition. Medium-tone steel needles were used. In the case of auditory stimulation *S* hears a series of clicks which come in groups of three. He is told: "You will hear three clicks marking off two intervals of time. If the second interval (that is, the time between the second and third clicks) is longer than the first interval, record L; if it is shorter, record S."[6] In the touch condition he feels these same stimuli as 'impulses' against the thumb.

The apparatus which produced the clicks from which the phonograph record was made is described briefly by Seashore[7] and more fully by Ross.[8] In the phonograph record *S* is given 100 trials, 20 for each of 5 increments. Each trial is a comparison between the standard 1-sec. interval and an interval which exceeds the standard by 0.02, 0.05, 0.09, 0.14, or 0.20 sec. In the series from which the record was made chance determined in each trial whether the standard or the longer interval should be sounded first. The key shows the order of presentation. The degree of difficulty, given in terms of 0.01 sec., is indicated at the head of each column. In the first block of 10 trials the increment 1.20 sec. is presented for comparison with the standard (Col. A reading down); following a short pause, a second block of 10 trials is given with the same increment (Col. B). In the same way two blocks of 10 trials each are given with the increment 1.14, followed by one block of 10 trials with the increment 1.09. This completes the A-side of the disk (Cols. A to E). The B-side is similar, with two blocks of trials for increment 1.02, two blocks for increment 1.05, and one block for increment 1.09 (Cols. F to J).

Key[9]

	A	B	C	D	E	F	G	H	I	J
	20	20	14	14	9	2	2	5	5	9
(1)	L	L	L	S	L	L	S	L	S	S
(2)	S	S	S	L	S	L	L	S	L	L
(3)	L	L	S	S	S	S	S	L	L	L
(4)	S	L	L	S	L	L	L	L	S	S
(5)	S	L	L	L	L	L	S	S	S	S
(6)	S	S	S	L	S	S	S	L	L	S
(7)	L	L	S	L	S	S	L	S	S	L
(8)	L	L	L	S	S	L	S	S	L	S
(9)	S	S	S	L	L	S	L	L	L	L
(10)	S	S	L	L	L	L	L	L	L	L

To test the accuracy with which the phonograph reproduces the original sounds Seashore presents photographs of the path made by the prong of a vibrating tuning fork upon the rotating phonograph disk. The test was made upon a spring type phonograph, wound by hand. The speed of rotation of the disk was found to be highly accurate; an error as large as 0.001 sec. in a single revolution was scarcely ever found.[10]

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[6] C. E. Seashore, *Manual of Instructions and Interpretations for Measures of Musical Talent*, 1919, 13.

[7] Seashore, *The Psychology of Musical Talent*, 1919, 108-9.

[8] F. B. Ross, The measurement of the time-sense as an element in the sense of rhythm, *Psychol. Monog.*, 16, 1914, 166-172.

[9] Seashore, *Manual of Instructions*, 9.

Transmission of stimuli. The phonograph used in the present experiment was a combination of two instruments. The original plan was to use a model No. 180 Columbia Viva-tonal Electric Radiograph, run by a synchronous motor. It is adjusted to run at the rate of 78 r.p.m., and the speed of the motor cannot be changed. The average phonograph record is best run at this speed. All the Seashore records except that for the sense of time are made for this standard rate. The time record, however, must be run at 60 to 65 r.p.m. This made the synchronous motor useless for our experiment. A Brunswick phonograph, Model No. T 160699, was substituted, run by a universal motor with speed regulator, driven by the city current, 110 A.C., 60 cycle. In combination with this the pick-up arm from the Columbia machine was used as described below.

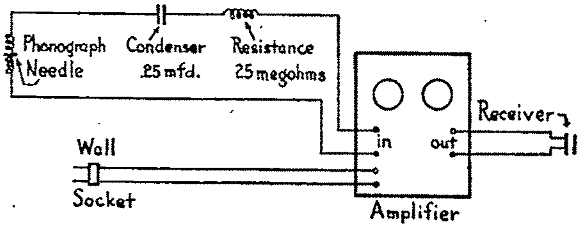

FIG. 1. WIRING DIAGRAM OF AMPLIFYING APPARATUS

The motor was set to run at 62 r.p.m. The constancy of its speed was tested by two methods: first by a Veeder Counter; and secondly by observing the number of times in a 1-min. period that a bit of paper, pasted on the record, passed a given point. Time was taken with a stop-watch and the full minute interval was used in each case. The second method was used as a check on the first, because of the possibility that excess pressure of the hand on the Veeder Counter would retard the speed. Readings were taken at both the beginning and the end of the record, that is, near the outer and near the inner limits of the disk. The mean of 26 readings was 61.5 r.p.m. with an S.D. of 0.31. This rough test was considered sufficient proof of the accuracy of the motor in view of the assurance given by Seashore's work concerning the high degree of accuracy of a good phonograph motor.

It had been hoped to take additional readings at the end of the experiment as a further check, but through a misunderstanding the mechanician in adjusting the apparatus changed the speed slightly. This occurred after the experiment was completed but before the second group of readings was taken. These readings, though useless as a check on the constancy of speed throughout the experiment, serve as an added check on constancy within a short period. Of the 30 readings taken at this time the mean was 63.21 revolutions per min. with an S.D. of 0.301.

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[10] Seashore, *Psychology of Musical Talent*, frontispiece and 32-33.

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A Western Electric two-tube amplifier, model 34-A, plugged in at a second socket to the wall current, was in circuit with the pick-up arm on the phonograph and a telephone receiver (cf. Fig. 1). The latter was in an adjoining room where *S* sat. In this way, vibrations from the phonograph needle in the grooves of the revolving record were picked up by means of the magnet in the pick-up arm and transmitted through the amplifier to the receiver. The amplifier may be set to give 4 successive degrees of amplification. In the description of the experimental procedure which follows these are referred to as amplifications 1, 2, 3, and 4, respectively. The receiver, which has a natural frequency of 800 d.v., is similar to an ordinary telephone receiver. It was made by the Bell Telephone Laboratory of New York, as a part of the Teletactor, for use by Professor Gault in the Vibro-Tactile Laboratory.

In circuit between the amplifier and the pick-up arm was a filter, consisting of a resistance coil of 25 megohms and a condenser of 0.25 mfd. This partially eliminated the vibrations caused by the scratch of the needle on the revolving record. These vibrations, scarcely audible to a listener with ear close to the needle, were very noticeable at the receiver end of the system when a high degree of amplification was used. Various types of condensers were tried in an effort to eliminate the scratch without reducing the energy of the clicks transmitted through the receiver, but with only partial success. The frequency of the scratch was so close to that of the clicks that it was difficult to cut out one without the other. The effect of inserting condensers in the circuit between the amplifier and the receiver was also tried, but with no better success. Aside from the scratch of the needle a second source of 'background' vibrations was found. When the switch in the current between the amplifier and the pick-up arm was shut off and the needle was not in contact with the record, vibrations from the revolving record were heard in the receiver. The probable explanation is an induced current produced at the point of contact of the electrical speed-regulator against the motor. Vibrations from these two sources, the scratch of the needle and rotation of the disk, were present throughout the experiment. They were faint when amplifications 1 and 2 were used, but quite pronounced with amplification 3. In the auditory condition they were heard as a background noise; in the tactual condition they were felt as a faint trembling of the diaphragm. In all cases, however, the auditory 'clicks' and tactual 'impulses' stood out clearly against this background.

Presentation of stimuli. (*a*) *Hearing condition.* In the hearing condition the receiver was suspended from a standard directly in front of the seated *S*. The height was adjusted so that it was on a level with *S*'s ears and about 3 ft. in front of him. *S* was seated in a chair with a writing arm.

(*b*) *Touch condition.* In the touch condition *S* held the receiver in his left hand, with thumb resting lightly on the diaphragm. He thrust his arm up to the elbow into a soundproof box.[11] This was of wood, 4 ft. square, completely filled with sawdust. Part way up the side was an opening large enough to accommodate *S*'s arm in a position comfortable to a seated person. A cloth sleeve attached to the opening was inserted into the box for a distance of 2 ft. Into this sleeve *S* thrust his arm, holding the receiver. A cloth was tucked about the arm to fill any extra space and close the opening. The box in large measure deadened the sound from the receiver, but some *S*s could still faintly detect the clicks.

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[11] The box is that constructed by Gault for use with the teletactor.

Earlier investigators in the laboratory have tried various means of controlling this factor.[12] The only one which proved successful was the use of a loud distracting noise. In the present experiment the same distraction was used. It consisted of two pairs of electrical buzzers, one pair suspended near the right ear, the other near the left ear of the *S*. The buzzers were all on one circuit and operated simultaneously. They produced a loud, continuous noise. Preliminary tests showed that, when the buzzers were operating, *S*, with arm and receiver thrust into the box with thumb removed from the diaphragm, was unable to detect the stimuli at all. The buzzers, while they proved successful in eliminating sound from the receiver, created another problem by providing a strong distraction in the touch condition which was not balanced in the hearing condition. The possibility of introducing a tactual distraction in the hearing condition was considered, but, since there seemed to be no possible means of equating two distractions presented to two different senses, it was not adopted. It was decided to recognize the distraction as a possible source of error in the touch condition and to make no effort to balance it in the hearing condition. A few of the *S*s reported that the distraction was definitely disturbing and made concentration difficult. A few reported that after the first few moments in the preliminary practice period they ceased to be conscious of the buzzer at all. Many reported that, while aware of it throughout, they were of the opinion that it did not affect their judgments.

Intensity of stimuli. The problem of controlling the intensity of the stimuli under the two conditions, touch and hearing, presented serious difficulties. Chief among these was the fact that in the touch condition this was partly under the control of *S*, whereas in the hearing condition it was not. The pressure of *S*'s thumb upon the diaphragm of the receiver dampened the vibrations. If considerable pressure was exerted, the distinct 'impulses,' corresponding to the 'clicks' of the hearing condition, were so deadened and blurred as to be indistinguishable from the faint 'background' vibration. In the preliminary practice period each *S* was warned of this fact and was instructed to allow his thumb to rest gently upon the diaphragm without exerting any pressure. *E* demonstrated the angle at which *S* was to hold the receiver so that the weight of the hand would not press the thumb against the diaphragm. *S* then thrust his arm with the receiver into the soundproof box and shifted it about slightly until a position was found where it could rest comfortably without tension and without pressure upon the diaphragm. In spite of these precautions a number of *S*s reported that in the effort to make accurate discriminations they unconsciously tensed the arm and pressed down upon the diaphragm. They were unaware of this reaction until they relaxed suddenly at the rest period or at the end of the sitting.

Even if *S* did not exert muscular pressure upon the diaphragm, the mere contact of the thumb against it dampened the vibration; that is, the amplitude and consequently the intensity of vibration was less when the thumb was in contact with the diaphragm than when the latter vibrated freely. A rough measure of this difference was obtained by comparing the loudness of the clicks when the thumb was placed on the diaphragm (outside the soundproof box) with the loudness when the diaphragm was allowed to vibrate freely, as in the hearing condition. In preliminary work with four *S*s it was found that the loudness produced by setting the amplifier at 1 in the hearing condition appeared to be approximately equal to that produced by setting the amplifier at 2 in the touch condition; *i.e.* contact of the thumb upon the diaphragm without muscular pressure reduced the loudness an amount equal to the change produced by reducing the amplification from 2 to 1 when the diaphragm was vibrating freely.

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[12] M. A. Weichbrodt, Tactual and visual discrimination of consonantal endings, M.A. Thesis, Northwestern Univ., 1930, 15.

The four preliminary *S*s agreed that with amplification 1 for hearing and 2 for touch the stimuli were always well above the threshold and easily sensed without strain. With a higher amplification for hearing an unpleasant roaring sound was produced; with a lower amplification for touch some of the stimuli were lost. This arrangement was, therefore, judged best, both because it appeared to approach optimal conditions for both touch and hearing and because it seemed the nearest approach obtainable to an equating of the physical intensity of the stimuli for the two conditions.

Further experimentation, however, showed that this rough measure of intensity was rendered totally unreliable by the fact, described above, that *S* might at any moment reduce the intensity of the stimuli even to a point where they fell below the threshold and were confused with the 'background' vibrations. To complicate the matter still further, some of the *S*s complained that these degrees of amplification for hearing and touch respectively were not sufficient; that the stimuli were barely discernible and often fell below the threshold when the attention wavered. Other *S*s, on the other hand, complained that when amplification was increased the stimuli became actually painful: in the hearing condition the sound was unpleasantly loud and harsh; in the touch condition the sensation was like an electric current running up the arm, distinctly painful.

In view of these difficulties it was decided to make no effort to present stimuli of equal intensity to all *S*s under both conditions, but rather to allow each *S* to choose in the preliminary practice period that degree of amplification which appeared optimal to him for touch and hearing respectively. The majority chose amplification 1 for hearing and 2 for touch; some chose 1 for hearing and 3 for touch; a few chose 2 for hearing and 3 for touch. It was argued that, since the purpose of the investigation was to measure time perception and not sensory acuity, the best measure could be obtained by presenting each *S* with conditions optimal to him individually. This advantage coupled with the great difficulties of controlling intensity was the deciding factor in a choice of the method to be used.

In the preliminary practice period *S* was allowed to hear and to feel the different degrees of amplification and was told to choose that one in which the stimuli appeared most distinct and stood out most clearly from the background vibrations. He was warned concerning the effect of pressing upon the diaphragm with the thumb, and was told that if during the experiment the stimuli appeared to grow more faint he was to regulate the degree of contact by moving his thumb very slightly during a period between stimuli.

Subjects. Thirty *S*s served in the experiment. Of these, 23 were undergraduate students in psychology at Northwestern University, 4 were graduate students, 1 was a professor of psychology, 1 a secretary, and 1 a social service worker.

Of the 30 only 3 had had previous experience with the teletactor. Two of these were graduate students (*S*s 7 and 17) who had served in a preceding experiment on the reception of speech through the finger-tip: the third, a professor of psychology (*S* 13), had repeatedly served as subject in work with the teletactor. Of the 30 a number had at one time or another in their school experience been tested for musical ability by the Seashore records in the usual way.

Experimental procedure. Each *S* served for two sittings of approximately 40 min. each in April and May of 1930. The interval between sittings varied from 1 to 38 days. At each sitting, every *S* made 100 judgments by touch and 100 judgments by hearing; that is, both sides of the phonograph record were presented under both conditions. Thus, at the end of 2 sittings each *S* had made 200 judgments by touch and 200 by hearing. This gave for the group 6000 judgments by ear and 6000 through the finger-tip. To balance between the two conditions any practice effect within the period of experimentation, the ABBA order was used, as follows. The *S*s were divided into 2 groups of 15 each. In the first sitting Group I made 100 judgments by touch, followed by 100 judgments by hearing, while Group II reversed this procedure, judging first by hearing, and then by touch. In the second sitting Group I started with hearing and Group II with touch.

At the beginning of the first sitting, preceding the main series, every *S* was given a preliminary practice period. Using the middle portion of the A-side of the phonograph record, *S* was allowed to hear the 'clicks' and feel the 'impulses' as he held the receiver in his hand. *E* counted with him "one, two, three" as the clicks occurred for two or three trials, and *S* made his judgments orally. The stimuli were continued until the nature of the task was thoroughly understood. The best method of holding the receiver was demonstrated. Then *S* thrust his hand with the receiver into the soundproof box, the buzzer was turned on, and he made a few judgments by touch. In cases where *S* seemed disturbed by the distraction he was allowed to sit for several minutes with the buzzer operating until he became more accustomed to the noise. As mentioned above he was allowed to hear and to feel the different degrees of amplification and choose which he preferred for each of the two conditions. The method of recording was explained, and attention was drawn to the longer interval occurring after every 10 trials. When the task was thoroughly understood and *S* at ease, the main series was begun. The time occupied by the preliminary practice was approximately 10 min.

During the main series *E*, engaged in the operation of the phonograph, was not in the same room with *S*. The A-side of the disk was run through without pause. Then *E* returned to the experimental room, glanced at the record sheet to make sure the task had been understood, and talked with *S* during a rest period of 2–3 min.. During this rest period in the touch condition *S* withdrew his arm from the box to prevent numbness. The B-side of the disk was run through under the same condition. Following another rest period the second condition was presented in the same way. This too was broken by a rest period in the middle. The second sitting was like the first except that the preliminary instruction and practice were shorter, just sufficient to recall the procedure to *S*.

TABLE I

Record of Scores

	Hearing				Touch			
	A	B	C	D	E	F	G	H
S	Sit. I	Sit. II	Total	Av.	Sit. I	Sit. II	Total	Av.
1.	82	85	167	83.5	82	75	157	78.5
2.	71	75	146	73.0	68	78	146	73.0
3.	80	71	151	75.5	69	73	142	71.0
4.	85	90	175	87.5	75	85	160	80.0
5.	76	70	146	73.0	69	74	143	71.5
6.	78	85	163	81.5	66	74	140	70.0
7.	87	79	166	83.0	71	82	153	76.5
8.	69	75	144	72.0	75	79	154	77.0
9.	75	81	156	78.0	74	70	144	72.0
10.	66	57	123	61.5	59	60	119	59.5
11.	79	80	159	79.5	75	81	156	78.0
12.	80	73	153	76.5	74	78	152	76.0
13.	90	89	179	89.5	72	65	137	68.5
14.	80	81	161	80.5	72	78	150	75.0
15.	78	77	155	77.5	67	78	145	72.5
16.	75	79	154	77.0	60	66	126	63.0
17.	80	85	165	82.5	80	83	163	81.5
18.	85	81	166	83.0	79	83	162	81.0
19.	60	67	127	63.5	64	55	119	59.5
20.	77	74	151	75.5	65	71	136	68.0
21.	70	85	155	77.5	71	72	143	71.5
22.	78	86	164	82.0	78	80	158	79.0
23.	83	83	166	83.0	71	83	154	77.0
24.	75	74	149	74.5	69	71	140	70.0
25.	87	82	169	84.5	78	73	151	75.5
26.	63	74	137	68.5	66	76	142	71.0
27.	86	84	170	85.0	83	75	158	79.0
28.	76	81	157	78.5	70	74	144	72.0
29.	70	80	150	75.0	70	71	141	70.5
30.	70	75	145	72.5	69	66	135	67.5

Sum = 2334.5
M_h = 77.82
P.E. of M_h = .779

Sum = 2185.0
M_t = 72.83
P.E. of M_t = .694

Diff. between means = 77.82 − 72.83 = 4.99
$P.E._d$ = 1.04
M_t = 93.5% of M_h

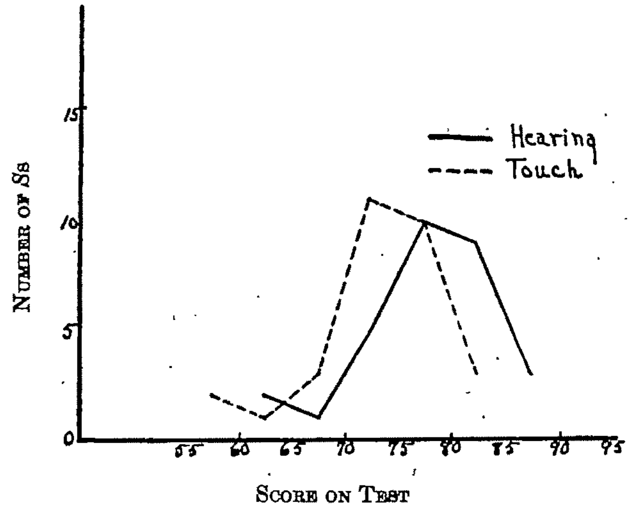

FIG. 2. DISTRIBUTION OF SCORES
Average of Sittings 1 and 2 using all increments

RESULTS

Table I presents the scores of each *S* and the total group scores. The columns to the left give figures for the hearing condition; those to the right give figures for the touch condition. For each *S* the score is given for Sittings I and II, respectively, expressed in percent right or in number of correct judgments out of a possible 100; the total score for both sittings expressed in number of correct judgments out of a possible 200, and the average score of the two sittings. Since there were 100 trials per sitting, this last item may also be read as the total score expressed in percent right. At the bottom of the table the average or mean score for the group is given. The group mean for hearing, *i.e.* the average of the items in Column D, is indicated by the letter M_h; the group mean for touch, *i.e.* the average of the items in Column H, is indicated by the letter M_t. The P.E. of the group mean has been computed for hearing and touch respectively, and also for the difference between these means. The relationship between M_h and M_t is expressed both by number of correct judgments and by percentage.

Fig. 2 shows the distribution of scores for the group for both hearing and touch. The base line gives the scores, which for each *S* are the average scores of Sittings I and II, using all 5 increments. It is plotted from the items in Columns D and H of Table I.

TABLE II

RECORD OF SCORES FOR FIVE DEGREES OF DIFFICULTY

	Hearing										Touch									
	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T
Sitting	I	II	I	II	I	II	I	II	I	II	I	II	I	II	I	II	I	II	I	II
Increment	20		14		9		5		2		20		14		9		5		2	
Subject																				
1.	20	19	18	20	16	18	17	18	11	10	19	20	20	19	20	12	15	16	8	8
2.	19	20	15	17	15	17	7	12	15	9	16	20	16	17	13	17	11	13	12	11
3.	19	20	16	16	17	15	14	7	14	13	17	17	17	19	10	13	13	14	12	10
4.	20	20	18	20	18	20	19	18	10	12	20	20	17	18	17	19	8	15	13	13
5.	20	19	14	15	16	14	12	11	14	11	16	16	15	18	16	13	13	13	9	14
6.	19	20	17	20	16	18	16	16	10	11	18	20	19	18	15	14	6	12	8	10
7.	20	20	20	19	18	17	15	14	14	9	18	18	15	19	17	17	10	15	11	13
8.	18	18	19	18	12	15	12	15	8	9	19	20	17	16	17	16	12	16	10	11
9.	20	19	18	18	12	16	14	18	11	10	18	19	15	15	14	16	16	11	11	9
10.	20	14	12	13	11	11	12	10	11	9	17	17	14	11	9	12	12	11	7	9
11.	20	20	19	20	17	16	13	13	10	11	17	20	19	18	18	19	11	11	10	13
12.	20	20	20	17	17	14	12	13	11	9	19	19	18	20	13	16	14	13	10	10
13.	20	20	19	19	20	19	14	13	17	18	20	18	16	14	15	14	12	11	9	8
14.	20	20	19	20	17	18	12	14	12	9	18	19	20	20	12	16	14	10	8	13
15.	17	19	16	19	16	14	13	13	16	12	18	18	17	19	11	13	10	16	11	12

TABLE II (Continued)

16.	19	20	18	19	14	15	13	13	11	12	10	13	13	16	11	14	14	12	12	11
17.	20	19	19	20	16	18	14	15	11	13	19	20	18	19	14	19	17	15	12	10
18.	19	20	16	17	19	16	17	14	14	14	19	19	17	17	17	16	17	18	9	13
19.	14	17	15	9	14	13	8	13	9	15	17	15	16	14	7	8	12	11	12	7
20.	20	20	17	20	16	14	14	10	10	10	17	16	16	17	8	17	12	10	12	11
21.	17	20	20	20	12	15	11	16	10	14	20	18	13	18	14	10	12	13	12	13
22.	20	20	19	20	18	19	10	14	11	13	19	20	19	20	17	18	14	13	9	9
23.	20	19	19	19	15	19	15	17	14	9	16	20	14	20	15	16	11	14	15	13
24.	20	17	18	18	11	15	12	15	14	9	15	18	18	15	14	15	13	11	9	12
25.	20	20	20	19	17	15	14	14	16	14	20	20	17	19	15	13	13	12	13	9
26.	16	19	14	15	8	13	14	17	11	10	11	18	14	19	17	15	11	13	13	11
27.	20	20	19	19	16	19	15	16	16	10	20	19	20	18	13	16	17	12	13	10
28.	16	20	16	19	16	16	17	15	11	11	16	20	18	17	15	15	13	10	8	12
29.	19	19	17	19	9	14	15	17	10	11	17	17	16	17	13	18	14	14	10	5
30.	18	18	15	18	12	12	12	12	13	15	15	12	17	14	12	12	14	16	11	12
Sum	570	576	522	542	451	475	403	423	365	342	521	546	501	521	419	449	381	391	319	322
Sum I & II	1146		1064		926		826		707		1067		1022		868		772		641	
Av.	38.20		35.47		30.87		27.53		23.57		35.57		34.07		28.93		25.73		21.37	
% rt.	95.50		88.68		77.12		68.83		58.93		88.93		85.18		72.33		64.33		53.43	

Table II presents the scores according to the various increments or degrees of difficulty, in the case of both hearing and touch. The figures directly below the letters designating the columns (A, B, C, etc.) give the sitting, either first or second. The row below this shows the increment in hundredths of a second, from the least degree of difficulty, that of twenty hundredths, to the greatest, that of two hundredths. The vertical column at the extreme left gives the numbers of the *S*s from 1 to 30.

The table should be read as follows: In the hearing condition *S* 1 in Sitting I out of 20 trials with increment 20, made 20 correct judgments, a perfect score (Column A). In Sitting II with the same increment she made 19 correct judgments (Column B). With increment 2, however, she made only 11 correct judgments out of 20 trials (Column I), and in Sitting II only 10 (Column J). This last is exactly what would be expected by chance, if all the judgments were mere guesses, and thus shows total lack of discrimination with this degree of difficulty. In the touch condition with increments 20 and 14 (Columns K, L, M, N) *S* 1 made as good a score as she did in the hearing condition. In the case of increment 9 Sitting II (Column P) is markedly lower than Sitting I (Column O), and the two together average somewhat lower than the score obtained with the same increment under the hearing condition. Increments 5 and 2 are also lower for touch than for hearing. With touch the record for increment 2 is even lower than that to be expected by pure chance.

The horizontal row directly below the record of *S* 30 gives the sum of the various columns, *i.e.* the number of correct judgments made by the group for the various increments in Sittings I and II. Below this is given the total number of correct judgments made by the group when the two sittings are combined for each of the several increments. Thus, in the hearing condition with increment 20 the group made 1146 correct judgments out of a possible 200. Below this appear the averages. For example, in the hearing condition with increment 20 the *S*s on the average judged correctly in 38.2 of the 40 trials. The last row gives this fact in terms of percent right; 38.2 correct judgments is 9 .5% right of the total number of trials given with increment 20.

Fig. 3 is plotted from the figures given in the bottom row of Table II, showing the average score in percent right for each increment for all *S*s. The abscissa gives the several increments in hundredths of a second, thus showing the different degrees of difficulty from the easiest at the extreme left to the hardest at the extreme right. The ordinate shows the score in percent right. The horizontal line indicates the conventional threshold of 75% correct judgments.

Table III shows the practice effect between Sittings I and II as shown by group scores. The score for the group, expressed in number of correct judgments, is given for each of the two sittings for the several increments. These figures are the same as those given in

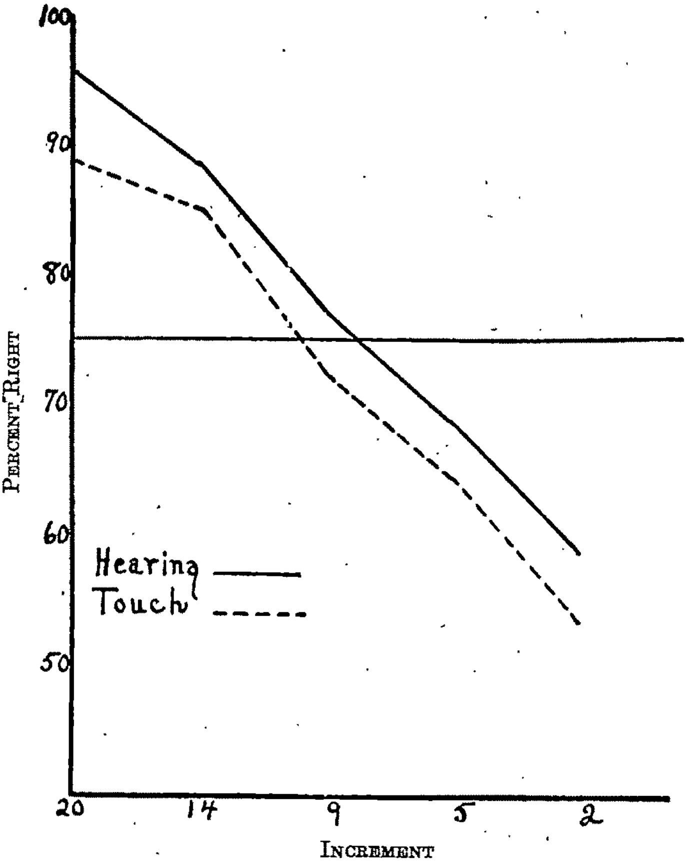

Fig. 3. Average Score of Percent Right at Every Increment

Table II, in the horizontal row labeled "Sum" directly below the record of S 30. The scores are also given in percent right, and the percent of increase of the second sitting over the first has been computed in each case. The total score, when all increments are considered together, expressed both in number of correct judgments and in percent right, is given for each sitting, and the percent of increase of the total score of the second sitting over the first. In the same way the last horizontal row gives the group score when only the first three increments are used, expressed in number of correct judgments and in percentage.

TABLE III

PRACTICE EFFECT BETWEEN SITTINGS I AND II SHOWN BY GROUP SCORES

	Hearing				
	Sitting I		Sitting II		Increase of II over I
Increment	Score	% right	Score	% right	
20	570	95 %	576	96 %	1.05%
14	522	87 %	542	90.3%	3.83%
9	451	75.2%	475	79.2%	5.32%
5	403	67.2%	423	70.5%	4.96%
2	365	60.8%	342	57 %	−6.30%
Total	2311	77.03%	2358	78.6%	2.03%
First 3	1543	85.72%	1593	88.5%	3.24%

	Touch				
	Sitting I		Sitting II		Increase of II over I
Increment	Score	% right	Score	% right	
20	521	86.8%	546	91 %	4.79%
14	501	83.5%	521	86.8%	3.99%
9	419	69.8%	449	74.8%	7.16%
5	381	63.5%	391	65.2%	2.62%
2	319	53.2%	322	53.7%	.94%
Total	2141	71.4%	2229	74.3%	4.11%
First 3	1441	80.05%	1516	84.22%	5.20%

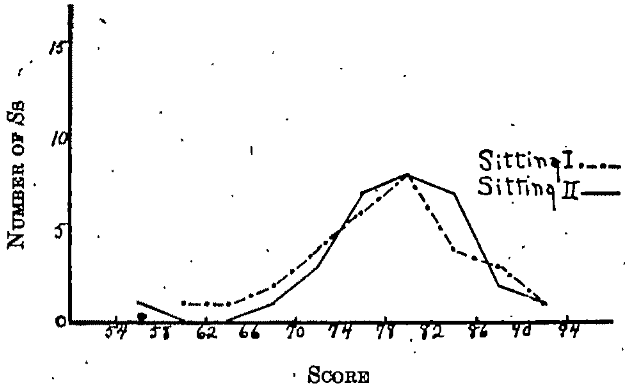

FIG. 4. DISTRIBUTION OF SCORES IN HEARING CONDITION FOR SITTINGS I AND II.

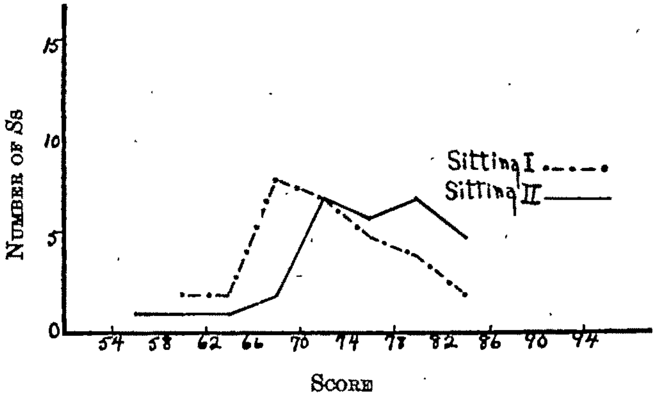

FIG. 5. DISTRIBUTION OF SCORES IN TOUCH CONDITION FOR SITTINGS I AND II

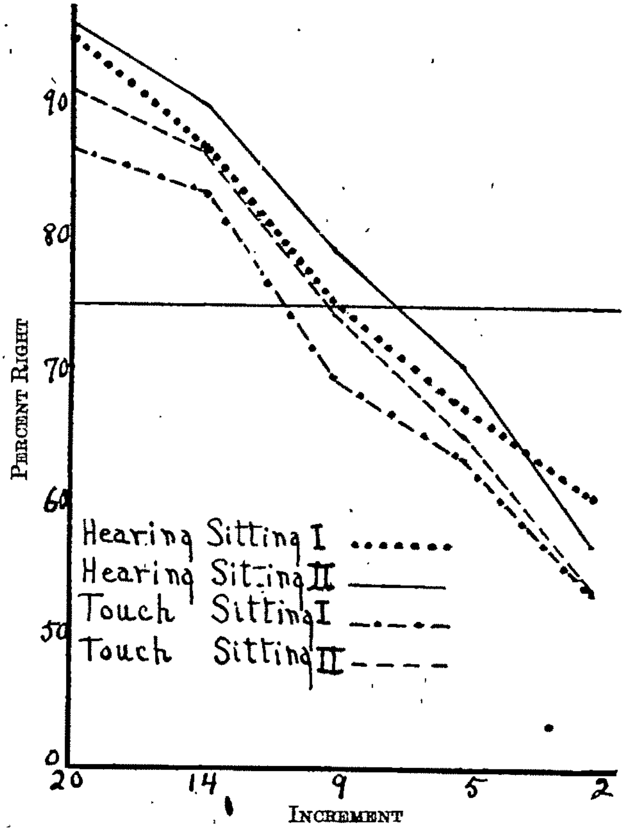

FIG. 6. AVERAGE SCORE OF PERCENT RIGHT AT EVERY INCREMENT FOR EACH SITTING AND FOR EACH CONDITION

Figs. 4 and 5 compare the distributions of scores in the two sittings. Fig. 4 gives for the hearing condition the frequency curves of Sitting I and Sitting II. The base-line gives the score in number of correct judgments out of a possible 100. The vertical axis gives the number of *S*s. The curves are plotted from the items in Columns A and B in Table I. Fig. 5 gives the corresponding data for the touch condition. It is plotted from the items in Columns E and F in Table I.

TABLE IV

FREQUENCY TABLE SHOWING THE DISTRIBUTION OF SCORES FOR THE SEVERAL DEGREES OF DIFFICULTY

		Sitting I					Sitting II				
	Increment Score	20	14	9	5	2	20	14	9	5	2
Hearing	20	17	4	1			17	9	1		
	19	6	8	1	1		8	9	4		
	18	2	5	3			2	4	4	3	1
	17	2	3	5	3	1	2	3	2	3	
	16	2	4	8	1	3		1	4	3	
	15		3	2	4	1		2	6	4	2
	14	1	2	2	7	6	1		5	5	3
	13				3	1		1	2	6	3
	12		1	4	7	1			1	2	3
	11			2	1	9			1	1	5
	10				1	6				2	5
	9			1		1		1			8
	8			1	1	1					
	7				1					1	
Touch	20	5	3	1			11	4			
	19	6	3				5	7	3		
	18	5	4	1			6	6	2	1	
	17	6	7	6	3		3	5	3		
	16	4	5	1	1		2	2	7	4	
	15	2	3	5	1	1	1	2	3	3	
	14		3	4	6			3	3	3	1
	13		2	4	5	4	1		4	6	7
	12			2	6	7	1		3	4	4
	11	1		2	4	4		1		6	5
	10	1		1	2	4			1	3	5
	9			1		5					4
	8			1	1	4			1		2
	7			1		1					1
	6				1						
	5										1

Fig. 6 also shows the practice effect between sittings. It is plotted from the items given in Table III. The average score, in percent right, for the group is given, for each increment, for each of the two sittings, for hearing and for touch.

Table IV presents the data in the form of a frequency table, showing the distribution of scores for the several degrees of difficulty for each of the two sittings. The figures at the top of the vertical columns indicate the increments from 20 to 2. The column at the extreme left gives the score in number of correct judgments, from a perfect score of 20 at the top to the poorest score made, that of 5 in the case of touch, at the bottom. The horizontal line indicates the conventional threshold of 75% correct judgments, which in this case is 15 of the 20 trials. The table should be read as follows: In the hearing condition, Sitting I, with increment 20, 17 *S*s made a perfect score, *i.e.* out of the 20 trials given with that increment each of these *S*s made 20 correct judgments. In the touch condition, Sitting I, with the same increment, only 5 *S*s made a perfect score. In the hearing condition, Sitting I, with increment 2, 5 *S*s had scores above the threshold, while in the touch condition there was one.

Table V gives the amount of the practice effect between sittings in the case of each S, shown by increase in score-points of the second sitting over the first. When the score of the first sitting exceeds that of the second the difference in score-points is indicated by a negative number. Thus in the case of *S* 1 the second sitting exceeded the first by 3 points in the hearing condition, but in the touch condition the score of the second sitting was 7 points below that of the first.

Table V gives also a comparison between hearing and touch in terms of the number of score-points by which hearing exceeds touch. This is given in the case of each *S* for each sitting and for the average scores of the two sittings. In those cases in which touch is superior to hearing the difference in score-points is indicated by a negative number. Thus in the case of *S* 2 a comparison of the hearing and touch scores made at the first sitting shows that hearing exceeded touch by 3 score-points, but at the second sitting touch was superior to hearing by 3 score-points.

Fig. 7 shows by means of a scatter diagram the amount of correlation between hearing and touch. The scores used are the average of the two sittings, expressed in percent right.

TABLE V

PRACTICE EFFECT BETWEEN SITTINGS AND SUPERIORITY OF HEARING OVER TOUCH SHOWN IN SCORE-POINTS FOR EACH *S*.

S.	Points Increase Sit. II over I		Points Hearing Exceeds Touch		
	Hearing	Touch	Sit. I	Sit. II	Av.
1	3	− 7	0	10	5
2	4	10	3	− 3	0
3	− 9	4	11	− 2	4·5
4	5	10	10	5	7·5
5	− 6	5	7	− 4	1·5
6	7	8	12	11	11·5
7	− 8	11	16	− 3	6·5
8	6	4	− 6	− 4	− 5
9	6	− 4	1	11	6
10	− 9	1	7	− 3	2
11	1	6	4	− 1	1·5
12	− 7	4	6	− 5	·5
13	− 1	− 7	18	24	21
14	1	6	8	3	5·5
15	− 1	11	11	− 1	5
16	4	6	15	13	14
17	5	3	0	2	1
18	− 4	4	6	− 2	2
19	7	− 9	− 4	12	4
20	− 3	6	12	3	7·5
21	15	1	− 1	13	6
22	8	2	0	6	3
23	0	12	12	0	6
24	− 1	2	6	3	4·5
25	− 5	− 5	9	9	9
26	11	10	− 3	− 2	− 2·5
27	− 2	− 8	3	9	6
28	5	4	6	7	6·5
29	10	1	0	9	4·5
30	5	− 3	1	9	5

Summary:

Hearing

Sit. I exceeds Sit. II	12 *Ss.*
" " "	56 points
Sit. II " Sit. I	17 *Ss.*
" " "	103 points
Sit. I equals Sit. II	1 *S*

Touch

Sit. I exceeds Sit. II	7 *Ss*
" " "	43 points
Sit. II " Sit. I	23 *Ss*
" " "	131 points
Sit. I equals Sit. II	0 *Ss*

Summary:

Average scores

H exceeds T	27 *Ss.*
" " "	164·5 score points
T " H	2 *Ss*
" " "	7·5 score points
H equals T	1 *S*

Sitting I

H exceeds T	22 *Ss*
" " "	184 score points
T " H	4 *Ss*
" " "	14 score points
H equals T	4 *Ss*

Sitting II

H exceeds T	18 *Ss*
" " "	159 score points
T " H	11 *Ss*
" " "	30 score points
H equals T	1 *S*

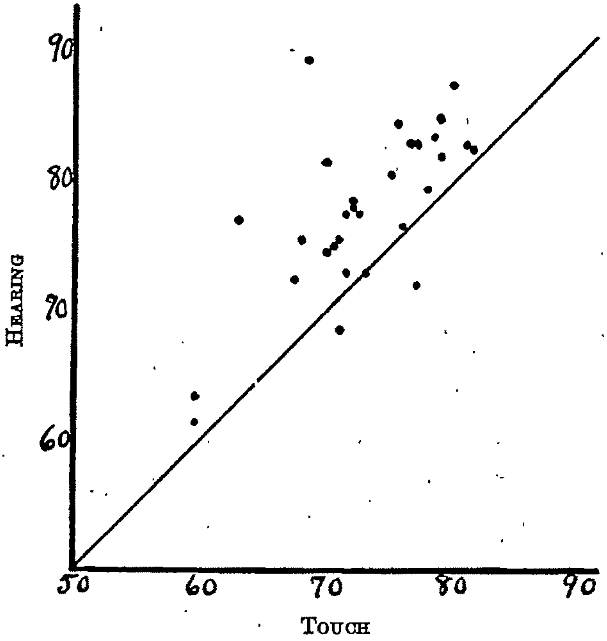

FIG. 7. SCATTERGRAM SHOWING AMOUNT OF CORRELATION BETWEEN TOUCH AND HEARING
(Average Scores of Percent Right, Sittings I and II)

DISCUSSION OF RESULTS

How does touch compare with hearing when average group scores are taken as the basis of comparison? When the average from the two sittings is used as the score for each *S* and group scores are computed on this basis, it is clear that the group showed a greater accuracy in the discrimination of short intervals of time by ear than in the discrimination of the same intervals of time through the finger tip. The group mean for hearing is 77.82 correct judgments out of a possible 100, with a P.E. of 0.779 (see Table I), while for touch the group mean is 72.83 ± 0.694. The fact that the difference between the means is slightly more than 4 times as great as its probable error, 4.99 ± 1.04, indicates that this difference is a significant one. Fig. 2 shows this superiority of hearing over touch when total scores are considered. Fig. 3 shows that when scores for the 5 increments or 5 degrees of difficulty are considered separately hearing exceeds touch in every instance. This superiority varies from 3.5% in the case of increment 14 to 6.57% in the case of increment 20 (Table II).

When scores for the two sittings are considered separately, practice effect is found to be an important factor, a factor which raises doubt as to the validity of the tentative conclusion concerning the superiority of hearing over touch. Table III and Figs. 4, 5, and 6 reveal the increase of score of the second sitting over the first. For both hearing and touch the group scores for Sitting II are higher than those for Sitting I, not only when total scores are considered, but also in the case of the scores for each of the 5 increments with one exception. The exception is found in the hearing condition with increment 2, where the first sitting exceeds the second (Table III and Fig. 6). Thus, when the group as a whole is considered, a measure of the "approximate physiological limit"[13] was not obtained in the first sitting.

Of greater interest in its bearing upon the comparison between the two senses is the fact that the practice effect is greater for touch than for hearing. In the case of touch the score of the second sitting shows an increase of 4.11% over that of the first (Table III); in the case of hearing there is an increase of 2.03%. These figures are based upon total scores. When increments 5 and 2 are eliminated from the score, on the argument that with both touch and hearing these are well below the threshold (see Fig. 6), the practice effect for touch holds nearly the same relation to that for hearing as it does when total scores are considered. The average percent of increase of Sitting II over Sitting I in the hearing condition when the first 3 increments are used to make up the score is 3.24%; in the case of touch it is 5.20% (Table III).

When individual scores are studied instead of group averages it is found that in the hearing condition 17 of the 30 *S*s improved in the second sitting; 12 showed a lowering of score and 1 remained the same (see Table V). In the touch condition 23 *S*s improved in the second sitting and 7 showed a drop in score. This suggests that the greater practice effect of the group score for touch as compared to hearing is the result, not of a few scores of large variation, but of the fact that a larger number of *S*s failed to reach the "approximate physiological limit" in the first sitting than was the case in the hearing condition.

In the first sitting 22 of the 30 *S*s made better scores by hearing than by touch (see Table V). These 22 *S*s as a group, made in the hearing condition a score which exceeded by 184 correct judgments the score which they made in the touch condition. Four *S*s made in the touch condition a score which exceeded by 14 correct judgments the score which they made in the hearing condition. The 4 remaining *S*s scored equally under the two conditions. In the second sitting 18 of the 30 *S*s showed superiority in hearing. These 18 *S*s, as a group, made in the hearing condition a score which exceeded by 159 correct judgments the score which they made by touch. Eleven *S*s made in the touch condition a score which exceeded by 30 correct judgments the score which they made by hearing, and one *S* scored equally under the two conditions. Thus, in the second sitting the superiority of hearing over touch is less both in number of score points and in number of subjects than is the case in the first sitting.

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[13] Seashore, *The Psychology of Musical Talent*, 51.

Seashore states that ordinarily 200 judgments from an *S*, that is, the score when both sides of the phonograph record are presented twice, will give a reasonable reliability,[14] or, in other words, will give the "approximate physiological limit." His statement, of course, is based upon judgments made by *S*s tested by hearing in the usual way. In the present experiment the presentation of stimuli in the hearing condition through the vibrating diaphragm of the receiver is perhaps less favorable than the usual method of presentation directly from the phonograph. Such factors as the 'background' noise and the greater novelty of the situation may tend to raise the threshold, at least during the early part of the experiment. If such is the case it is likely that the "approximate physiological limit" will not be reached within the 200 judgments. However, when the scores of the 30 *S*s, in the hearing condition, using the average of the two sittings, are compared with the Seashore norms it is found that the distribution is but slightly lower than that of the norms. Seven *S*s rank below the 25th percentile, 10 between the 25th and 50th, 5 between the 50th and 75th, and 8 above the 75th.

In the case of touch it is even more likely that 200 judgments are insufficient. The situation is more novel, the set-up more complicated. *S* starts the experiment with a greater background of experience in judging time intervals by ear than in judging by finger-tip. The curious vibratory sensation experienced when the thumb is in contact with the diaphragm, the sound-proof box, the distracting electric buzzer, may all tend to raise the threshold to a greater degree than do the less distracting factors of the hearing condition. The greater practice effect found for touch may be explained on these grounds. It is conceivable that the superiority of hearing over touch may be explained on the same grounds.

In view of these facts one must conclude that while within the limits of this experiment the discrimination of short intervals of time by hearing is more accurate than by touch, the evidence is insufficient to state that the difference is a significant one. Further experimentation is needed to determine whether or not with sufficient practice the ability to discriminate short intervals of time by touch may more nearly approach in accuracy the ability to discriminate the same by hearing, or even equal or exceed it.

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[14] Seashore, *Manual*, etc., 10.

How small an interval of time can be distinguished by touch? How does touch compare with hearing in this respect? Fig. 3 shows that when average group scores are the basis of comparison, increment 9 is below the conventional 75% threshold in the touch condition, but above the threshold in the hearing condition. In the touch condition, when the *S* must discriminate between the standard 1-sec. interval and an interval of 1.09 sec., the group as a whole judged correctly in 72.33% of the trials; in the hearing condition in 77.12% of the trials (Table II). For both touch and hearing increments 5 and 2 are well below the threshold. When the two sittings are considered separately it is found that the group score for increment 9 in the second sitting of the touch condition is only 0.4 of 1% below the group score for the same increment in the first sitting of the hearing condition, (Table III and Fig. 6). The former is barely below the threshold, the latter barely above it. It is probable that with added practice the score for touch for increment 9 would rise above the threshold.

Table IV gives further information concerning the thresholds. In the hearing condition with increment 9, in the first sitting 20 *Ss* scored above the threshold; in the second sitting there were 21, a gain of only one *S*. In the touch condition with the same increment 14 *Ss* scored above the threshold in the first sitting and 18 in the second, a gain of 4 *Ss*. In the case of increment 5 in the hearing condition, 4 more *Ss* scored above the threshold in the second sitting than in the first; in the touch condition there was a gain of 3 *Ss*. In the case of increment 2 under both the hearing and touch conditions fewer *Ss* scored above the threshold in the second sitting than in the first. From these figures the two conditions may be compared on the basis of individual scores rather than group averages. They show that for both touch and hearing in the case of increment 2 there is almost total lack of discrimination, and that the degree of practice given within the limits of this experiment does not improve the score; that in the case of increment 5 discrimination improves with practice and the very slight difference between the two conditions is in favor of hearing; that in the case of increment 9 touch gains more with practice than does hearing and in the second sitting very nearly equals the record of hearing in the first sitting. These facts bear out the comparison made on the basis of group scores as shown in Fig. 6.

Does the *S* who scores high in hearing also score high in touch? The scattergram, Fig. 7, shows the degree of correlation between the two conditions, using the average scores for the two sittings. Because there were only 30 *S*s the *r* was not computed. There were no *S*s who scored markedly high in touch and low in hearing. In 4 cases the hearing score exceeded by 10 points or more the score made by touch; the difference was greatest in the case of *S* 13, who ranked highest in the group in hearing with a score of 89.5 and only sixth in touch with a score of 68.5. It is of interest that *S* 13 is the only one of the group who had served repeatedly in work with the teletactor prior to the present experiment. His record is perhaps an argument against the suggestion, made above, that the greater novelty of the touch condition might explain the inferiority of touch when compared with hearing.

SUPPLEMENTARY EXPERIMENT

The problem of controlling the factor of intensity for the two conditions, touch and hearing, has been discussed above. Reasons have been given for the choice of method, that of allowing each *S* to select in the preliminary practice period that degree of amplification which seemed optimal to him for touch and hearing respectively. All *S*s chose a higher amplification for touch than for hearing. As explained above, this does not necessarily mean a more intense stimulus, for the reduction of the amplitude of vibration, by the pressure of the thumb upon the diaphragm, lowers the intensity in the touch condition. The criticism may be raised, however, that the added amplification for touch may have more than compensated for the deadening effect of the thumb. In this case touch would have had an advantage in the comparison. This might conceivably explain why the superiority of hearing over touch in the judgment of intervals of time was as small as the results show. With this question in mind a supplementary experiment was undertaken, whose purpose was to discover the effect of changes of intensity of the stimulus upon the ability to discriminate short intervals of time through the finger tip.

Procedure. Three *S*s, graduate students in psychology, who had not served in the previous experiment, were presented with stimuli from the phonograph record under 3 different degrees of amplification, Amplifications 1, 2, and 3. The apparatus and general procedure was the same as in the previous experiment. The amplification used was not known to *S*. It was raised or lowered by *E* according to a prearranged order after every 5 or 10 trials.

In the previous experiment there had been some indication of serial learning: a few *S*s reported that in the second sitting they could remember the order of occurrence of the 'Longers' and 'Shorters' in some parts of the record, at the beginning of a new column especially. To prevent this possibility in the supplementary experiment the stimuli were presented in a mixed order. To present all stimuli under all three degrees of amplification it was necessary to repeat three times. This would have given an even greater opportunity for serial learning than that present in the earlier work, if the same order had been used throughout. By means of the switch between the phonograph and the amplifier any part of the record could be cut out or presented to the subject at will. The record was run from beginning to end without pause; *E* with ear close to the needle was able to count each group of clicks as it appeared and cut out any parts desired. The omitted parts were presented to *S* at a later time.

The entire A-side of the record was used and the last 10 trials of the B-side. This included all trials with increments 20, 14 and 9. The key to the order of presentation is as follows:

I	II	III	IV	V
A a 2		A a 1	A a 3	
A b 1	A b 3			A b 2
B a 3	B a 1	B a 2		
B b 2			B b 1	B b 3
C a 3	C a 2			C a 1
C b 1		C b 3	C b 2	
D a 2	D a 3	D a 1		
D b 3			D b 2	D b 1
E a 1			E a 2	E a 3
E b 3	E b 1	E b 2		
J a 2	J a 1	J a 3		
J b 1			J b 3	J b 2

The Roman numerals at the top of the vertical columns indicate the successive presentations of the entire record. The capital letters indicate the vertical columns in the key to the records given in the preceding experiment. The small letters indicate the first or second half of a column. The Arabic numerals indicate the degree of amplification. Thus, in the first presentation of the record (Column I) *S* was given all trials in the natural order of the record without cutting out any parts. The first 5 trials of Column A were given under Amplification 2; this was followed by the second 5 trials of the same column with Amplification 1. In the second presentation of the record (Column II), the first 5 trials of Column A were omitted; the first stimuli received by *S* were those of the second half of Column A, given under Amplification 3. This was followed by the first 5 trials of Column B; the second 5 of this column were omitted, etc. By this method 60 trials, 20 for each of 3 increments, were given for each of 3 different degrees of amplification, in a mixed order which made serial learning highly improbable. The *Ss* recorded their judgments in continuous order, not knowing in which column they belonged. Each *S* served for two sittings. The first sitting included 2 presentations of the record, with 60 trials on the first and 30 on the second (Columns I and II in the key). The second sitting included 3 presentations of the record with 30 trials on each (Columns III, IV, and V).

Presentation of data. The records of the three *Ss* are summarized below.

	Amplification 1		Amplification 2		Amplification 3	
S	Errors	% error	Errors	% Error	Errors	% error
31.	10	16.6	10	16.6	16	26.6
32.	22	36.6	20	33.3	14	23.3
33.	12	20	9	15	4	6.6

Interpretation of data. The records of *S*s 32 and 33 suggest that increase in amplification brings an increase in accuracy of judgment. In both cases the lowest intensity gave the lowest score, and the highest intensity gave the best score. The record of *S* 31, however, shows the reverse. The poorest score was made with the highest intensity, while Amplifications 1 and 2 gave equally good results. It is of interest that the objective performance of each *S* corresponded to his subjective estimate. *S* 31 reported that in Amplification 3 the 'background' vibrations confused the issue and made judgment more difficult than in the case of the lower intensities. *S* 33 felt sure his judgments were most accurate with the highest intensity. *S* 32 reported that while the 'background' vibrations were very marked in Amplification 3 she felt that her judgments were more accurate than with the lowest intensity; she was unable to decide whether Amplification 2 or 3 appeared optimal.

One cannot base sweeping conclusions upon the records of three *S*s, but the data at hand would seem to justify the method adopted in the preceding experiment, that of allowing each *S* to choose the degree of amplification which appeared best for him individually. The influence of intensity appears to be a matter of individual differences. In this case the use of a higher amplification for touch, assuming that this involves a higher intensity of stimulation, and that the higher amplification more than compensated for the deadening effect of the thumb, would not mean for all *S*s an advantage for touch in comparison with hearing.

THE CORRELATION OF INTELLIGENCE SCORES AND CHRONOLOGICAL AGE FROM EARLY TO LATE MATURITY

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The high correlation between intelligence age and chronological age during childhood and youth is a well-demonstrated fact. Intelligence scores or mental age norms based on the results of adequately validated tests are available for each year, often for each month of each year from the period of school entrance up to 'adulthood.' Adult levels are further differentiated into 'average' and 'superior' or even into IQ or corrected IQ ratios as in the Stanford-Binet test; into 'intelligence ratings' A, B, C, D, with their plus and minus subdivisions, as in the Army Alpha tests; into 'brightness indices' or derived IQ, as in the Otis group tests; or into percentiles of adult performance, as in the Otis tests, the National Tests, and the Thorndike Intelligence Examination for High School Graduates. Studies of intelligence have for the most part been made on the more available people: school children, college students, or comparatively young adult groups, such as institutional groups or the draft army and its sub-groups. Studies of occupational groups, drawn generally from younger adult populations, sometimes include intelligence ratings, but only very rarely is it possible to discover the age ranges or age means for these groups. Comparatively little has been reported concerning the intelligence of older adults. In test standardization a plateau of ability 'adult intelligence' is generally assumed extending onward from the high point reached somewhere between the ages of 13 and 20 years.

A number of writers have discussed the probable course of the intelligence score curve in adulthood.[1] Investigations have been published with data based

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[1] E. L. Thorndike et al., *Adult Learning*, 1928, chap. xii; R. Pintner, *Intelligence Testing*, 1931, 86; F. A. Moss, *Applications of Psychology*, 158-170, 1929; H. L. Hollingworth, *Mental Growth and Decline*, 1927, 306-322; *The Psychology of Functional Neuroses*, 1920, chap. xii; C. Spearman, *The Abilities of Man*, 1927, 371-375.

on special groups which showed (1) a slight decline in score by the fiftieth year,[2] (2) a large decline at or after three score years and ten.[3] But by reason of the select character of these groups the investigators have hesitated on the basis of their findings to point a general intelligence decline of the total population from early maturity onward. Examination of 'normal adults' by the Stanford-Binet or the Point Scale technics has led examiners to question whether the often surprisingly low scores,[4] especially in the later decades, are representative of the adult maximum mental age of the individuals in question, *i.e.* whether in many cases where specific 'mental deterioration' is by no means to be diagnosed, individuals score lower on intelligence tests in later life than they once did.

The studies of the intelligence scores of adults who, by chance or on the basis of careful selection, more or less represent the normal population give certain indications as to the probable form of the intelligence curve from the age of 20 to 50 years,[5] and it is doubtless on the basis of such results as these that hypothetical curves of intelligence have been drawn.[6] Studies of atypical groups (prisoners, psychotics, general hospital and old people's home inmates) giving intelligence scores for these special populations in advanced age include the 7th, 8th, and 9th decades. The studies of the normal groups indicate either that up to the age of 50 years intelligence declines slightly from a maximum in the early 'twenties' or at least that it does not rise after such a maximum. The studies of the atypical groups indicate that among these less favored individuals a steady and considerable decline is registered from the age of 50 to 90 years, but the scores at all ages for the populations from which these individuals are drawn indicate that the individuals in question were always mentally inferior. For example, the average mental age of both Mursell's and Foster and Taylor's subjects in the 'fifties' is below 11 yrs. 6 mos.

The coefficients of correlation between score and age that we have found in the literature are for the most part limited to the normal adult ages 15 to 50.

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[2] The most extensive of these are perhaps the following: R. M. Yerkes, Psychological examining in the United States Army, *Memoirs Nat. Acad. Sci.*, 15, 1921, chap. XIV; M. V. Cobb and R. M. Yerkes, Intellectual and educational status of the medical profession as represented in the United States Army, *Bull. Nct. Res. Council*, 1, 1919-1921, 457-532; Alonzo E. Grace, The mental ability of adults as related to interests, needs, and activities, *Eightieth Annual Report Cleveland Board of Educ., Div. Adult Educ.*, 1930.

[3] J. C. Foster and G. A. Taylor, The applicability of mental tests to persons over fifty years of age, *J. Appl. Psychol.* 4, 1920, 39-58; M. F. Beeson, Intelligence at senescence, *ibid.*, 4, 1920, 219-234; G. R. Mursell, Decrease in intelligence with increase in age among inmates of penal institutions, *J. Juv. Res.*, 13, 1929, 197-203; Louis Wender, The applicability of Binet-Simon tests in psychoses of the senium, *N. Y. Med. J.*, 101, 1915, 448-454.

[4] S. L. Pressey and L. W. Cole, Are the present psychological scales reliable for the examination of adults? *J. Abnor. Psychol.*, 13, 1918-19, 302 ff. R. M. Yerkes and J. C. Foster, *A Point Scale for Measuring Mental Ability*, 1923, 81 ff.

[5] Important data have been gathered by Harold E. Jones (presented by Thorndike et al., *op. cit.*, and in an unpublished M. A. thesis by H. H. Hsiao, The performance on the Army Alpha as a function of age, *Columbia Univ.*, 1927. These give a score curve for a carefully selected representative New England population, ages 12 to 55. But the question has been raised by the investigators as to the consistently representative character of the groups in the later decades studied. (See Hsiao's discussion.) The other principal sources are enumerated in note 2.

[6] Pintner, *op. cit.*, 86; Moss, *op. cit.*, 159.

They range from −0.011 to −0.35. The coefficients (not reported) for Mursell's prisoners and for Foster and Taylor's general hospital patients, covering as they do a far wider range, would probably be larger than any of these. From the slope of the two curves we estimate them to be between −0.40 and −0.50. But, as the authors point out, neither of these populations can be thought of as representative of the general population and both are made up of individuals mentally inferior at their best. The extent to which the trend exhibited by these atypical groups, as well as by the normal populations referred to, is characteristic of other groups, especially representative normal populations, will in part appear in our further discussion.

Plan of present study. In the following pages, tables and curves are presented and described which indicate the trend from childhood to old age of intelligence scores in samplings from the populations of two American towns. In order to find the relation between age and intelligence score throughout the life span, and especially in maturity, the intelligence was measured of persons from every decade and semi-decade beginning with the 'teen' age and continuing to senescence.

An effort was made to secure a sampling of the literate citizens in each of the two towns that would (1) present sufficiently large numbers at each age decade and semi-decade for statistical reliability and that would (2) be homogeneous in general mental character from age to age. Conclusive evidence regarding the relation of age to intelligence test score will, of course, not be available until scores have been obtained year after year from childhood until old age from the same members of large representative populations. In the meantime, if as far as possible the older, the middle aged and the younger members of the same social groups can be tested in sufficient numbers and selected in sufficient numbers at each point on the age scale a substitute comparison of some value can be made. The present study is an attempt in this direction. To what extent the trend in the populations examined is characteristic of the expected trend in a representative 'general population' will be indicated as far as possible by an analysis of the characteristics of the particular groups investigated.

Subjects. The results presented are based on the scores of 823 *Ss* from two cities, A and B. In City A (a manufacturing, industrial, and banking town, population about 12,000, situated in an agricultural community, the capitol of its county) usable intelligence tests score were obtained from 122 adults (36 male, 86 female) ages 50 to 90. The distributions by age in semi-decades are given in Table I. In City B (a residential and banking town, population about 15,000, also situated in an agricultural region, the seat of several private schools and near a university) usable scores were obtained from 134 children and youths (48 male and 86 female), ages 7 to 19 inclusive, and from 567 young adults and adults (249 male and 318 female), ages 20 to 94 inclusive (cf. Table I.) The method of securing the *Ss* is described elsewhere.[7]

Test instrument. The test used was a specially printed form in large type (12-pt. Antique) of the first 60 items in the Otis Self-Administering Test of Intelligence, Higher Examination Form A given as a 15-min. test.[8] We have found that scores on Form A with a 15-min. limit correlate +0.86±.02 with scores on the same form used as a 30 min. test in the limited range of superior ability represented by 121 university students. Fifteen-minute scores of the same students correlate +0.90±.01 with their 20-min. scores on the same test. Fifteen-minute scores of 394 eighth grade pupils correlate +0.971±.003 with their 20-min. scores. The scores for split halves of the 15-min. test correlate +0.96±.005 for the university students. The 15-min. scores of 55 university students correlate +0.71±.05 with their scores on the Thorndike Intelligence Examination for High School Students.[9]

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[7] W. R. Miles, Correlation of reaction and coördination speed with age in adults, this JOURNAL, 43, 1931, 380, note 13.

[8] C. C. Miles, The Otis S-A Higher Examination as a fifteen-minute intelligence test, *Personnel J.*, 10, 1931, (in press).

TABLE I

DISTRIBUTION BY SEMI-DECADE AGE PERIODS OF 823 PERSONS SCORED ON THE OTIS S-A 15-MIN. TEST

Age Periods	City A	City B	Total
5-9	—	20	20
10-14	—	63	63
15-19	—	51	51
20-24	—	40	40
25-29	—	38	38
30-34	—	45	45
35-39	—	45	45
40-44	—	47	47
45-49	—	43	43
50-54	17	59	76
55-59	25	58	83
60-64	24	50	74
65-69	25	52	77
70-74	20	44	64
75-79	5	27	32
80-84	5	13	18
85-89	1	5	6
90-94	—	1	1
Total	122	701	823

Each test was given individually and the time measured by stopwatch.[10] The group of 823 persons whose scores have been found usable worked for exactly 15 min. each. Of these, 4 persons finished the test, having tried all 60 items.

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[9] The Thorndike test was chosen as a means of testing the validity of scores based on a 15-min. test because it is "clearly a test of superior intellect and (is) probably the most discriminative which we now possess." Cf. H. E. Garrett, The relation of tests of memory and learning to each other and to general intelligence in a highly selected adult group, *J. Educ. Psychol.*, 19, 1928, 601-613.

[10] In all cases where overtime performance occurred (22 cases, av. age 54.3 yr., av. pro rata 15-min. score 28), or where the subject for any reason whatever was unable (defective vision, 3 cases) or unwilling (cause not stated, 3 cases) to begin or to continue at work for the full 15 min. the test blank was discarded and the results are not included in this study. The results for 5 other individuals had to be omitted because the age or sex had not been noted. A total of 33 cases were thus discarded out of 856 entered on our lists.

The score used is the number of correct responses. We have prepared a table for transmuting 15-min. scores into 20-min. scores[11]. (Otis has already provided a 20 to 30 min. transmutation table), and on the basis of the 30-min. scores we have made use of the corresponding mental ages, percentiles, intelligence quotients, and Army Alpha scores given by Otis. An arrangement of these equivalents with interpolated values added is given as Table II.

TABLE II

OBTAINED 15 MIN. SCORES ON OTIS' S-A HIGHER EXAMINATION WITH ESTIMATED EQUIVALENTS FOR OTIS 20- AND 30-MIN. SCORES, OTIS MENTAL AGES AND PERCENTILES, AND ALPHA SCORES

Otis S-A 15-min. score	Otis S-A 20-min. score	Otis S-A 30-min. score	Otis Mental Age	Otis IQ	Otis Percentile	Alpha score
6	6	7	8-11	65	0.2	7
8	8	10	9-7	68	0.4	10
10	10	13	10-2	71	0.7	15
12	13	17	10-10	75	2	20
14	16	20	11-5	78	4	25
16	18	23	12-0	81	5	30
18	20	26	12-6	84	8	36
20	22	29	13-0	87	12	46
22	25	32	13-7	90	20	56
24	27	35	14-2	93	28	64
26	29	38	14-8	96	36	72
28	32	41	15-2	99	45	80
30	34	44	15-8	102	56	88
32	36	47	16-2	105	68	98
34	39	50	16-6	108	76	108
36	41	53	16-11	110	81	116
38	44	56	17-3	113	87	125
40	46	59	17-8	116	91	136
42	49	62	18-0	119	95	148
44	52	64	18-3	122	97	160

Procedure. The Otis 15-min. test was administered as part of a program of psychological experiments designed to investigate the mental abilities and capacities of normal persons in youth and maturity. The fact that this particular test was intended to measure 'intelligence' was not stressed. In our special printing (4-page booklet) of the first 60 items of the Otis S-A, Form A, in large type the word 'intelligence' was omitted from the title.[12]

Every *S* was tested separately. Seated comfortably at a table, with good light, and in a quiet room *S* was given a copy of the test and several sharp pencils. He read the first page which begins as follows:

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[11] C. C. Miles, *op. cit.*

[12] By kind permission of the World Book Co.

> *Read this page. Do what it tells you to do. Do not open this paper*, or turn it over, until you are told to do so. Fill these blanks, giving the date, your name, and age. Write plainly.

Then follow spaces for name, age, the date of testing. (The ages were later verified by comparison with birthdays given in another connection.) It had already been explained in every case that all information, as far as it referred to individuals, was strictly confidential.[13] The study was announced as an investigation of the relation between age and aptitudes, and as the services of the *S*s were paid for on the basis of age, it was understood previous to registering for the appointment that this item was to be furnished. Practically no difficulty was encountered in conducting the examination.

Below the blank spaces appeared this paragraph:

> Questions of different kinds are to be read and answered. Four samples are given. Here is a sample question already answered correctly. Notice how the question is answered.

Detailed instructions make up the remainder of the printed matter on the face of the booklet. These include samples of all the types of items in the test with clear and complete specifications for and practice in filling the blanks in each case. The concluding paragraph reads:

> There is a total of 60 questions. You are not expected to be able to answer all of them, but do the best you can. You will work for 15 minutes after the signal to begin. Try to get as many right as possible. Be careful not to go so fast that you make mistakes. Do not spend too much time on any one question. If you can't get it go on to the next question. Don't ask questions until you have finished. *Write answers plainly, make all letters like printed capitals. Do not turn this page until you are told to begin.*

As soon as the examiner observed that the subject had finished reading this page, she said quietly and pleasantly: "Turn the page and answer the questions beginning with number one." Nothing was said about speed or accuracy, although the instructions are explicit in regard to the desirability of both these points. It seems indicative of the friendly rapport which was noteworthy throughout the entire investigation that only 3 persons out of 856 refused to perform the task in question.

The timing was done inconspicuously and without the knowledge of the *S*. At the close of just 15 min. from turning the page to item 1 the examiner returned and said: "That will be enough on this exercise, now I want you to come into the other room with me to see something new." She took note of the exact point (except in the 22 cases discarded on this account) which the *S* had reached at 15-min. as there were often individuals, especially among the older ages, who wished to continue on the item then under consideration until it was either solved to their satisfaction or given up. Some complimentary or friendly remark was usually added as *S* was guided into another room for the next part of the experimental program.

We lay stress on the matter of procedure in this study because of the difficulty which we and others have encountered with adult *S*s in administering 'intelligence' tests.[14] The willing coöperation secured in the present investigation was due to many factors, of which we wish especially to note here the coöperative interest of the club and other organization chairmen and the church officials. These people secured the aid of their members in "a coöperative study" and at all points in the investigation the *S*s gave evidence of an attitude of willing aid in a joint community inquiry.

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[13] This confidence has been carefully guarded.

RESULTS

The mean scores for decades and semi-decades in the populations of the two cities appear in Tables III–VI inclusive. Fig. 1 gives the unsmoothed intelligence score curves based on the means for the 5- and 10-yr. periods. The scores from City B extend from the earliest age testable by this type of examination throughout the life span. Those from City A extend only through later maturity, from 50 to 90 yrs. of age.

For discussion we have divided the curve into three somewhat arbitrarily selected periods: (1) The period of growth, from 7 or 8 to 18 or 19 yr.; (2) the period of early maturity, from 20 to 49 yr.; and (3) the period of later maturity, from 50 to 89 yr. It is not always possible to conduct the discussion strictly within the limits of this classification, but we have done so as far as was practicable. The results for these periods are presented in sequence.

Childhood and youth. The period of growth extends here to the age of 18 yr. The correlation between age and score from the earliest age tested until the maximum mean score is reached for this population (ages 7 to 17 yr. inclusive) is +0.80 ±.023. This coefficient indicates the usual amount of agreement between score and age expected in groups of this size for the period in question and the score scatter shows no special selection of more or less superior individuals in one age as compared to another.[15] The degree of brightness in the younger group and the homogeneity of mental talent for the early period is found in a comparison of the IQ at successive age levels, and in a comparison of the average scores (using 30-min. equivalents) with Otis' norms for the ages in question. For this comparison we have combined the scores for the ages up to 19 yr. into 2-yr. periods (see Table VII) and have discarded those periods

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[14] The experiences of a number of other investigators on this point have been summed up by H. S. Conrad, The measurement of adult intelligence and the requisites of a general intelligence test, *J. Soc. Psychol.*, 2, 1931, 72 f.

[15] Scores on examination A (Army) correlated +0.70 with the chronological ages of 1,162 California school pupils, aged 8 to 16 yr. Scores correlated +0.83 with school grade and +0.82 to +0.87 with Stanford-Binet mental age. (Cf. Yerkes, *op. cit.*, 333-334.)

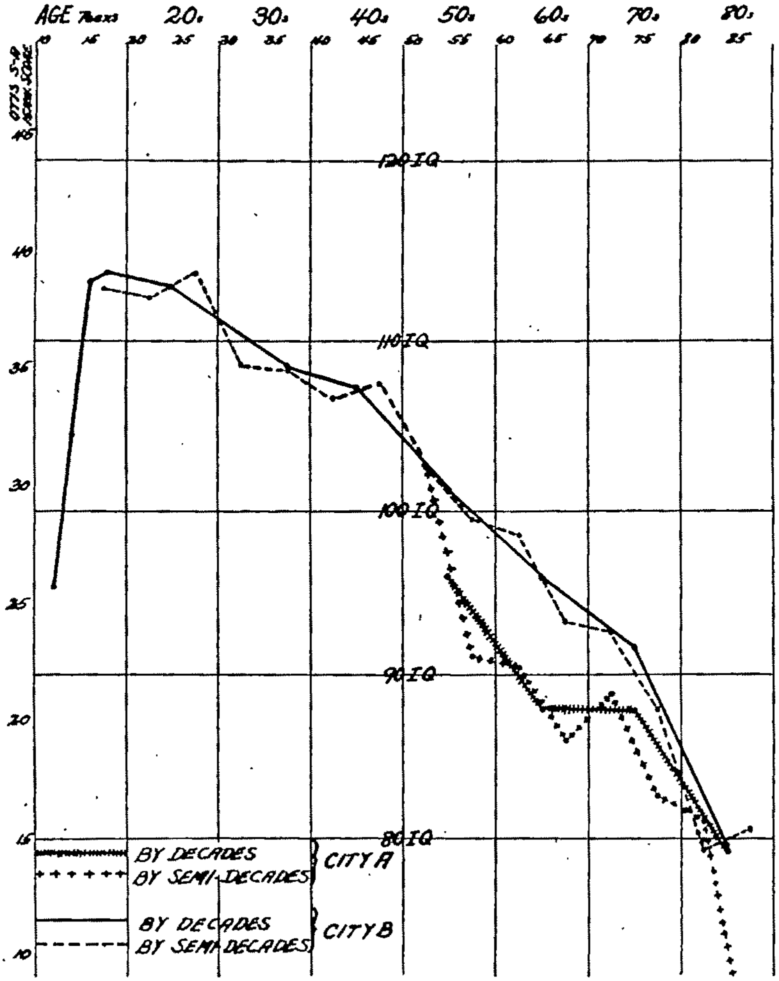

FIG. 1. THE MEAN INTELLIGENCE SCORES PLOTTED BY DECADES AND SEMI-DECADES FOR THE POPULATIONS OVER 12 YEARS OF AGE STUDIED IN TWO CITIES WITH RESULTS FOR BOTH SEXES COMBINED.

Initial rise during the period of growth shows Pearson r, +0.80. Decline from early to late maturity, r, −0.30 and −0.39.

(below age 11) which contained less than 15 cases. This neglects 26 cases ages 7 to 10 yr. inclusive. In the combined age groups of 11 and 12 yr., 13 and 14 yr., etc. are included children ranging in age from 11 yr. 0 mo. to 12 yr. 11 mo., or 13 yr. 0 mo. to 14 yr. 11 mo. We have therefore assumed that the average ages in the respective groups are 12 years 0 months, 14 years 0 months, etc., and have compared the mean scores transmuted into 30-min. scores with the Otis norms for these ages.

TABLE III

Otis S-A Intelligence Scores, by Semi-Decades, of 124 Adults, Both Sexes, from City A

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Semi-decades	Means	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
18	50-54	30.94	8.92	2.76	45.5	15·10	104	60	92
25	55-59	22.86	10.16	0.28	33.3	13·10	91	23	60
24	60-64	22.18	6.82	1.32	32.0	13·7	90	20	56
25	65-69	19.10	9.40	−0.74	27.5	12·10	86	12	43
20	70-74	21.30	10.24	1.02	31.0	13·5	89	18	56
5	75-79	16.90	8.08	0.20	24.4	12·2	82	6	33
6	80-84	15.82	10.28		23.0	12·0	81	5	30
1	85-89	5.50			6.0	8·1	64	0.2	6

TABLE IV

Otis S-A Intelligence Scores by Decades and by 20-yr. Periods of 124 Adults, Both Sexes, from City A

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
43	50-59	26.2	10.6	2.62	38.1	14·8	96	37	73
49	60-69	20.6	9.8	.84	29.9	13·3	88	16	54
25	70-79	20.4	9.7	1.55	29.7	13·2	87	15	53
7	80-89	14.5	10.0		21.0	11·6	79	4	27
92	50-69	23.3	10.5	1.98	33.2	14·0	91	25	60
32	70-89	19.1	10.2		28.0	12·10	86	11	44

The mental age equivalents for the three age levels, 12, 14, and 16 yr., are respectively 14·8, 16·2, and 17·4 yr. They show gains of something over a year for each 2-yr. period. The peak of ability is reached in the fourth 2-yr. group (see Table VII). This group

TABLE V

OTIS S-A INTELLIGENCE SCORES, BY SEMI-DECADES, OF 617 ADULTS, BOTH SEXES, FROM CITY B

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Semi-decades	Means	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
51	15-19	38.50	8.04	0.25	57.0	17'5	114	88	126
40	20-24	38.10	7.04	−0.66	56.0	17'3	113	87	125
40	25-29	39.22	8.20	1.94	56.9	17'4	115	89	131
43	30-34	35.26	10.32	0.09	50.8	16'8	109	77	110
44	35-39	35.06	8.44	0.59	50.0	16'7	108	76	108
48	40-44	33.82	11.36	−0.29	48.8	16'5	107	73	104
42	45-49	34.50	11.04	1.80	50.5	16'6	109	77	109
63	50-54	30.98	11.64	1.41	45.5	15'11	104	62	92
56	55-59	28.74	9.04	0.41	42.0	15'4	100	50	83
50	60-64	27.94	11.16	1.87	41.0	15'1	99	45	80
53	65-69	24.22	9.64	0.21	35.3	14'3	95	29	65
42	70-74	23.78	10.48	1.42	34.7	14'1	93	27	63
26	75-79	20.46	8.56	1.98	29.8	13'0	88	15	54
13	80-84	14.50	9.40	−.016	20.8	11'6	79	4	26
5	85-89	15.30	9.60		22.0	11'10	80	5	28
1	90-94	15.50	0		22.2	11'11	80	5	28

TABLE VI

OTIS S-A INTELLIGENCE SCORES, BY DECADES, AND BY 20-YR. PERIODS OF 616 ADULTS, BOTH SEXES, FROM CITY B

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
51	15-19	38.5	8.0	−.11	55.9	17'3	114	88	126
80	20-29	38.7	7.9	2.53	56.0	17'3	114	88	126
87	30-39	35.1	9.6	.17	50.6	16'8	109	77	108
90	40-49	34.2	9.2	3.27	50.0	16'6	108	76	108
119	50-59	29.9	9.7	2.86	44.0	15'8	102	56	88
103	60-69	26.1	10.6	2.05	38.0	14'8	96	37	72
68	70-79	23.1	8.7	3.54	33.5	13'11	92	24	61
18	80-89	14.7	9.3		21.0	11'7	79	4	26
131	15-29	38.6	7.9	3.84	55.9	17'3	114	88	126
177	30-49	34.7	10.4	6.51	50.0	16'6	108	76	108
222	50-69	28.1	10.4	5.48	41.0	15'2	99	46	80
86	70-89	20.9	10.2		30.5	13'4	88	17	55

(18 yr.) shows a gain of only one month (17·5) over the 16-yr. level. The high mental ages of all four groups (as compared to their chronological ages) show that the populations tested are above average. How much above is indicated, or at least suggested, by the equivalent Otis IQ of 115, 115, 117, 115.[16]

During the period of growth we are then dealing with a superior and homogeneous population whose Otis IQ approximates 115 and whose mental development follows the usual upward trend in score expressed in the coefficient +0.80 ±.023.

TABLE VII

OTIS S-A INTELLIGENCE SCORES OF 97 'TEEN' AGE REPRESENTATIVES, BOTH SEXES, OF CITY B

			Otis 15-min. S-A Score			Equivalent Measures				
No. Cases	Age Group	Av. Age	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
22	11-12	12	25.8	8.7	−2.96	37.8	14·8	115	89	74
35	13-14	14	32.3	7.0	−2.69	47.4	16·2	115	89	99
23	15-16	16	38.9	10.4	−0.11	56.4	17·4	117	89	129
17	17-18	18	39.2	7.7		56.6	17·5	115	89	130

Early maturity. This period we count as extending from the third to the fifth decade. The curve of intelligence score averages maintains almost the same level throughout the twenties. It drops very slightly in the 'thirties', and but little more in the 'forties.' The average for the 'twenties' has an equivalent mental age of 17 yr. 3 mo. (Otis IQ 114). The population is, therefore, approximately equal in mental level to the 17-18 yr. old group and in brightness to the other groups representing the period of growth. Homogeneity of mental talent seems therefore to be demonstrated up to this point as far as this population is concerned.

The averages for the decades from the third to the fifth are presented in Table VI; those for the semi-decades in Table V. These tables present beside the means and the σ for the groups in question, (1) the significance of the differences between successive periods and (2) the score equivalents period by period in terms of Otis 30-min. score, mental age, IQ, percentile, and Army Alpha score.

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[16] P. Cattell has shown that "at all the Stanford-Binet IQ levels the Otis IQ (Form A) tends to be about six points lower than that of the Binet; P. Cattell, IQs and the Otis measure of brightness, *J. Educ. Res.*, 22, 1930, 31-35. If a correction is made in these terms on our figures the resulting IQ level would be 120 to 123 indicating even more pronounced superiority than the original Otis equivalents.

From the values in Table VI it may be seen that the mean score and mental age for the 'twenties' is not lower than that for the previous semi-decade. The equivalent mental ages are the same, 17 yr. 3 mo, and the Otis IQ of 114 registered by both is approximately the same as the average for the members of the population under 20 yr. of age. The drop from the third to the fourth decade mean is represented by a decline of 7 mental age months, from 17 yr. 3 mo. to 16 yr. 8 mo. The difference between the two means is two and a half times its σ and therefore of possibly true significance. Between the 'thirties' and the 'forties' the drop is only one mental age month and the decline can not be regarded as in the least significant. From the 'forties' to the 'fifties', however, a drop of 11 mental age months is registered and the difference between the two means is more than three times its σ and, therefore, very probably significant statistically.

The semi-decade indices (Table V) show with somewhat greater detail the trend traced in the previous paragraph. It appears from these figures that the score curve forms a plateau from the late 'teens' through both early and late 'twenties,' that a considerable, but not statistically significant, drop occurs between the late 'twenties' and the early 'thirties' (8 mental age months) and also between the late 'forties' and the early 'fifties' (7 mental age months). Otherwise a gentle decline is usually present from one 5-yr. period to the next. Registered in mental age months the loss indicated from the high point of 17 yr. 4 mo. in the late 'twenties' to 15 yr. 11 mo. in the early 'fifties' is 17, making an average of 3.4 mental age months in each 5-yr. period. From Table VIII, it may be seen that although the successive semi-decade losses in intelligence scoring ability in early maturity are not significant statistically for groups of the present size those between alternate decade levels are definitely so, and the difference between the third and the sixth decade means is almost 7 times its σ.

The Otis IQ in the decade periods between 20 and 50 yr. of age are as follows: 114, 109, 107. If the population groups represented at the successive 10-yr. periods are homogeneous it appears from these IQ that within the age period in question a drop of about 7 IQ points has taken place. The Otis IQ for the 'fifties' shows a further drop to 102, five IQ points below the 40-yr. level.

The coefficient of correlation between life age and score during this period gives a further measure of the decline in scoring ability. The coefficients for various parts of the age span and for the total range from the late teens to the 'nineties' appear in Table IX. To show the relation of score to age in early maturity and for comparison with other indices of this sort published by other writers (or coefficients which we have been able to compute from their published data) we have computed three coefficients, one for the period from 15 to 55 yr. of age (males and females combined) $r = -0.283 \pm .031$; and two for the period from 30 to 55 yr. of age as follows: males only, $r = -0.108 \pm .065$, both sexes, $r = -0.191 \pm .040$. These are given with the results of other investigators in Table X. The coefficients between score and age for the period of early maturity (age 20 to 55 or 60 yr.) range from -0.01 (Yerkes) to -0.283 (our data). A single correlation (Bingham) on the range from 20 to 63 yr. is -0.35. The question of selection suggested by these coefficients and analyzed by several of the investigators will be discussed later in the present report.

TABLE VIII

CORRECTNESS OF THE DIFFERENCES BETWEEN MEAN DECADE INTELLIGENCE SCORES

(Both Sexes)

	Decade means compared	D/σ diff.	Decade means compared	D/σ diff.
City A	50–59 70–79	2.29	50–59 80–89	5.89
City B	15–19 30–39	2.22	30–39 50–59	3.79
	15–19 40–49	2.92	30–39 60–69	6.19
	15–19 50–59	5.94	40–49 60–69	5.76
	15–19 60–69	8.13	40–49 70–79	7.80
	20–29 40–49	3.42	40–49 80–89	8.34
	20–29 50–59	6.92	50–59 70–79	4.96
	20–29 60–69	9.26	50–59 80–89	6.61

TABLE IX

Correlations between Age and Intelligence Score in the Age Range 15 to 95 Years

	City A			City B		
Age range	Sex	No. cases	Product-moment r	Sex	No. cases	Product-moment r
16-55				M & F	368	−.283 ± .031
30-55				M	106	−.108 ± .065
30-55				M & F	249	−.191 ± .040
15-95				M & F	620	−.528 ± .021
20-95				M	250	−.465 ± .032
20-95				F	319	−.532 ± .026
20-95				M & F	569	−.504 ± .021
30-95				M	207	−.371 ± .040
30-95				F	282	−.520 ± .028
30-95				M & F	489	−.455 ± .023
40-95				M	166	−.381 ± .044
40-95				F	234	−.489 ± .033
40-95				M & F	400	−.447 ± .026
50-95	M	36	−.302 ± .103	M	128	−.392 ± .051
50-95	F	86	−.367 ± .064	F	183	−.392 ± .042
50-95	M & F	122	−.343 ± .055	M & F	311	−.385 ± .032

City A and City B combined

Age range	Sex	No. cases	Product-moment r
50-95	M	166	−.387 ± .045
50-95	F	267	−.370 ± .036
50-95	M & F	433	−.372 ± .028

From the peak, mental age 17 yr. 5 mo., of the intelligence score curve reached at approximately the age of 18 yr. the trend is first almost level, then it gradually declines to a point at the age of 50 yr. some 15 or 16 mental age months lower (mental age 16 years 1 month). The decline in Otis IQ points is from 115 to 104.5 and the correlation coefficient indicating the relation of score to age is −0.283 for males and females combined.

Later maturity. This we have considered as covering from the sixth to the ninth decade. From the age of 50 yr. onward scores for two populations, Cities A and B, are available. It is obvious from the intelligence score curves for these two cities (Fig. 1) that A and B represent different levels of intelligence. It is therefore necessary to consider them separately and not as a single larger

TABLE X

CORRELATIONS BETWEEN AGE AND INTELLIGENCE SCORE IN THE AGE RANGE 15–65 YR.

Investigator[17]	Date	Age range	No. cases	Product-moment r	Test	Subjects
	1921	21-60	146	−.011±.056		Medical officers, Camp Devens
	1921	21-60	308	−.120±.038		Infantry officers Camp Devens
Yerkes	1921	21-55	5404	−.192±.009		Medical officers, Camp Greenleaf
	1921	21-30	2475	−.063±.013		Medical officers, Camp Greenleaf
	1921	30-60	3267	−.192±.011		Medical officers, Camp Greenleaf
Filer & O'Rourke	1922-3	19-53	92	+.067±.071	Civil Service clerical test	Civil Service clerks
Bingham & Davis	1924	20-63	73	−.350±.07	15-min. spiral omnibus based on Army Alpha	Business men: executives and salesmen
Garrett	1928	Av. 19.4 yr. S.D. 2 yr.	158	−.110±.053	Thorndike exam. for H. S. grads.	College students, Sophs, Jrs., Srs., in Elem. Psych. course Columbia
Jones	1928	15-54	1130	−.110±.023	Army Alpha	Typical rural New England community
Burgard	1929	18-50+(1 at 65, 1 at 75)	60	−.151±.085*	Individual test Binet and Performance Type	Specially selected cases. Wide educational range
Conrad & Jones	1929	17-54	48	−.063±.097	Army Alpha	Typical rural New England community
	1929	17-54	58	−.239±.084	Army Alpha	
Grace	1930	15-59	599	−.100±.028*	Otis S-A	Adult school pupils, Cleveland
	1930	15-59	2400	−.190±.013	Thorndike test of word knowledge	
Miles & Miles	1931	30-55	106	−.108±.065	Otis S-A	Males, City B
	1931	30-55	249	−.191±.04	15-min.	Both sexes, City B
	1931	16-55	368	−.283±.03		

*Our computation from the tables of data.

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[17] R. A. Yerkes, Psychological examining in the United States army, *Mem. Nat. Acad. of Sci.*, 15, 1921, 817-17; H. A. Filer and L. J. O'Rourke, Progression in civil service tests, *J. Person. Res.*, 1, 1922-23, 484-520; W. V. Bingham and W. T. Davis, Intelligence Test Scores and business success, *J. Appl. Psych.*, 8, 1924, 1-22; H. E. Garrett, The relation of tests of memory and learning to each other and to general intelligence in a highly selected adult

population except where trend during the age period of 50 to 90 yr. is being considered. However, it is equally clear that the score curves for both follow approximately the same downward course. The average population of one city registers at successive decades mean scores regularly divergent from those of the other and this divergence is always in the same direction, *i.e.* City A is always lower than City B. The significance of the differences is as follows for the 'fifties,' 'sixties,' 'seventies,' and 'eighties': 2.01, 3.18, 1.21, 0.55. For the combined period 50–69 yr. the ratio of difference to σ diff. is 3.75 and for the total period, 50–90 yr., it is 3.48.

The decade and semi-decade scores for City B, forming the curve which we have followed up to this point, (Tables V and VI, and Fig. 1) show about the same rate of decline from the 'fifties' onward as was indicated from the 'forties' to the 'fifties.' From the 'forties' to the 'seventies' the decade scores describe a straight line, from the 'seventies' to the 'eighties' the curve is steeper. The loss in scoring power from the 5th to the 6th decade was 11 mental age months. From the 6th to the 7th decade it was 12 mo., from the 7th to the 8th there is a drop of 9 mo., and from the 8th to the 9th, 28 mo. The differences are respectively, 3.2, 2.9, 2.0, and 3.5 times their σ and, therefore, possibly or probably of statistical significance. The semi-decade drops are always less than twice the σ diff.; they range in amount, for the successive groups having more than 10 members, from 2 to 18 mental age months and average 8.5 mental age months. The total mental age loss from the age of 50 yr. (M.A. 16–1) to the age of 85 yr. (M.A. 11–7) is 54 M.A. months or 4.5 M.A. years. The Otis IQ for the 6th to 9th decades are as follows: 102, 96, 92, 79. From the age of 50 yr. (IQ 104.5) to the age of 85 yr. (IQ 79) the loss is 25.5 IQ points. The relation between scoring ability and age in later maturity (City B) is indicated further by the correlation coefficients (Table IX); for the period from 50 to 95 yr. of age, $r = -0.385 \pm .032$ (both sexes combined) and -0.392 for either sex separately.

In City A the trend is similar but the curve is, as already noted, at a lower level. The decade populations are smaller than those for

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group, *J. Educ. Psychol.*, 19, 1928, 601-613; H. E. Jones in E. L. Thorndike's *Adult Learning*, 1928, 158; J. Burgard, Testreihe zur Untersuchung der Allgemeinintelligenz Erwachsener, *Arch. f. d. ges. Psychol.*, 68, 1929, 325-394; H. S. Conrad and H. E. Jones, Psychological studies of motion pictures: III. Fidelity of report as a measure of intelligence, *Univ. Calif. Publ. Psychol.*, 3, 1929, 245-276; A. E. Grace, The mental abilities of adults as related to adult interests, needs, activities, *Adult Educ. Bull., Cleveland Board of Education*, 94 1930, (series 4, no. 2) 99-101.

City B and the differences between levels are therefore correspondingly less significant statistically. The decade drops are respectively 2.6, 0.8, and 1.6 times the σ diff. Those for the semi-decades are with one exception (early to late 'fifties' 2.8) never even possibly of statistical significance. But gradual though they are, they total for the period from the beginning of the 6th to the middle of the 9th decade a loss of 52 mental age months or 4 1/3 mental age years, which is exactly the amount of loss suffered in City B in the same age period. The difference between the mean score for the 'fifties' and the mean score for the 'eighties' is 5.9 σ diff. The degree of correlation between score and age in City A (Table IX) from 50 to 95 yr. of age is −0.343 ± .055, for the sexes combined; −0.302 ± .103, for the males alone; or −0.367 ± .064, for the females alone.

From the means for the populations in the two cities the decline in scoring power is seen to persist and even to increase from the beginning to the end of the period of late maturity. Differences, insignificant as the mean scores for successive 5-yr. periods are compared, become highly significant as they accumulate and the loss for the total period is found to be 7.48 σ diff. for the two cities combined. The correlation between intelligence score and age for the combined group, age 50 to 95 yr., is −0.372 ± .028.

Summary. From a maximum score at the age of 18 yr. the curve of intelligence maintains almost a plateau through the 'twenties,' begins then to fall, and persists in its downward course, at first slowly, then more rapidly until the end of the life span. The drop in intelligence from the age of 18 to the age of 85 yr. (City B) measured in 15-min. scores on the Otis S-A test is 24 points, 62% of the original high score; in mental age months it is 70 (5 yr. 10 mo.), in Otis IQ points it is 36. The correlation between age and score from the age of 20 to the age of 95 yr. is approximately −0.50 ± .02, and the difference between the average score in the 'twenties' (City B) and that in the 'eighties' (City B) is 9.9 σ diff.

COROLLARY RESULTS

(*1*) *Brightness groups.* Besides comparison by central tendency such as that described above, a comparison of the frequency of various degrees of ability in the several age decade classes is another method of indicating the trend of intelligence scores throughout the life span in populations A and B. For this comparison we have employed the well-known Army classifications, transmuting our Otis 15-min. scores into equivalent Alpha scores for the purpose (see Table II for transmutation values). The percentage of cases in each brightness group is given

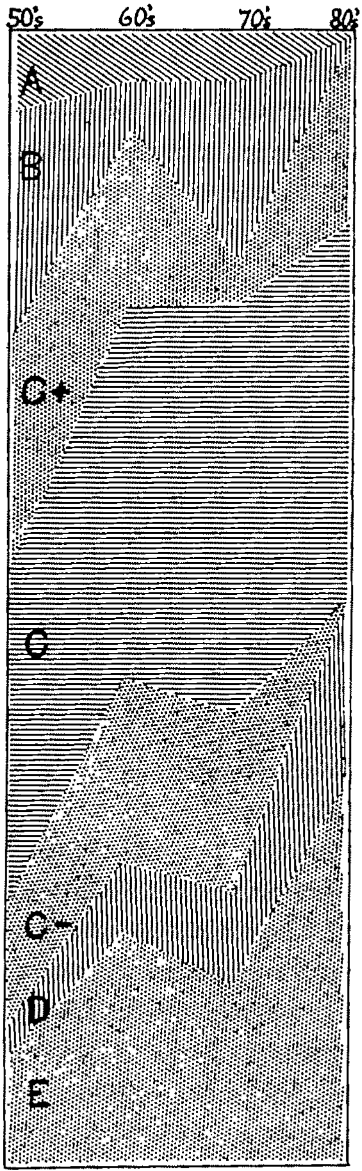

FIG. 2. PERCENTAGE DISTRIBUTIONS FOR 7 BRIGHTNESS CLASSES (ARMY) BY DECADES FOR CITY A
(122 cases)

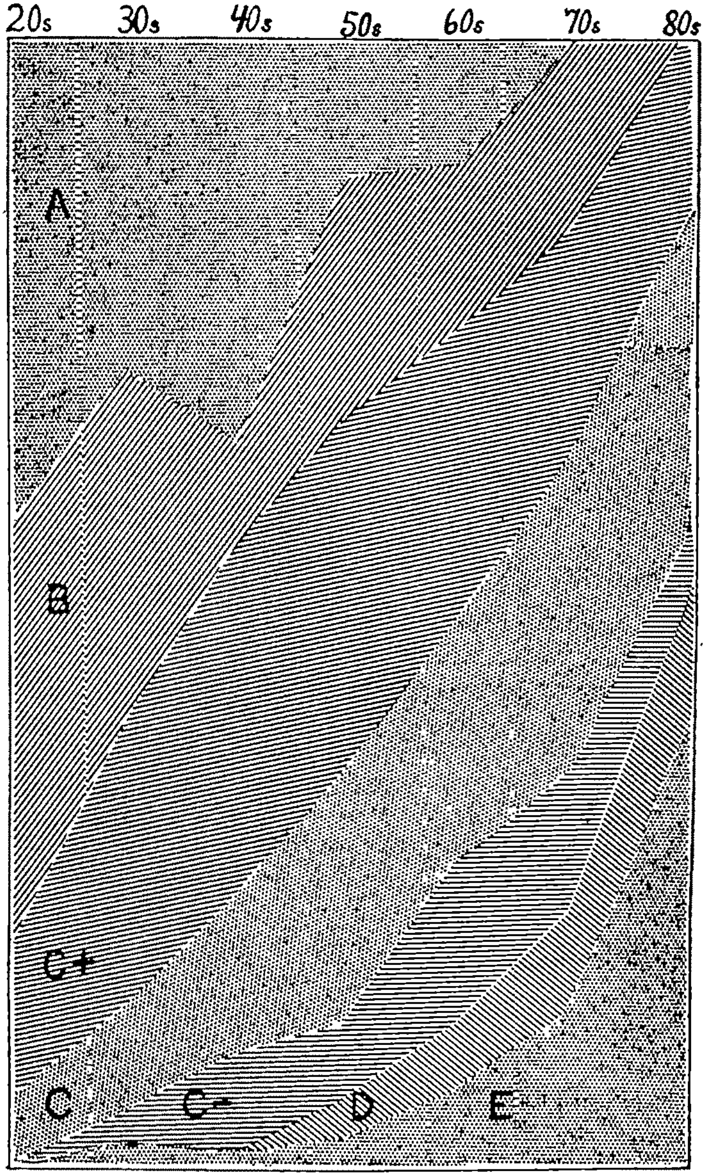

FIG. 3. PERCENTAGE DISTRIBUTIONS OF THE 7 BRIGHTNESS CLASSES (ARMY) BY DECADES FOR CITY B
(567 cases)

by decades in Tables XI and XII for City A and for City B. Table XIII gives the percentages for the sexes separately in duo-decade groupings. Figs. 2 and 3 give the decade results in graphic form. In the case of City A the comparison covers four decades from the ages of 50–90 yr. The graph for City B gives the classification from the ages of 15–90 yr., thus covering 7.5 decades.

TABLE XI

PERCENTAGE OF ADULTS, BOTH SEXES, AT EACH AGE DECADE IN 7 BRIGHTNESS CLASSES

(City A, 122 Cases)

Class	50s	60s	70s	80s
A	7.1	4.1	4.0	—
B	19.1	4.1	16.0	—
C+	21.4	16.3	4.0	16.7
C	28.6	32.6	36.0	33.3
C−	11.9	16.3	16.0	—
D	2.4	6.1	8.0	16.7
E	9.5	20.4	16.0	33.3
Total	100.0	99.9	100.0	100.0
No. Cases	42	49	25	6

TABLE XII

PERCENTAGE OF ADULTS, BOTH SEXES, AT EACH AGE DECADE IN 7 BRIGHTNESS CLASSES

(City B, 567 Cases)

Class	20s	30s	40s	50s	60s	70s	80s	90s
A	42.3	28.9	35.6	12.0	10.8	—	—	—
B	37.2	31.1	8.9	21.4	14.7	16.9	—	—
C+	14.1	25.6	31.1	30.8	25.5	19.7	16.7	—
C	6.4	10.0	14.4	23.1	21.6	28.2	27.8	—
C−	—	2.2	8.9	6.9	14.7	12.7	5.5	100.0
D	—	—	—	3.4	5.9	8.5	11.1	—
E	—	2.2	1.1	2.6	6.9	14.1	38.9	—
Total	100.0	100.0	100.0	100.2	100.1	100.1	100.0	100.0
No. Cases	78	90	90	117	102	71	18	1

The persistently lowered percentage of representatives in the higher classifications A and B with increasing age and the disappearance of these classes in both populations in the ninth decade is noteworthy. Similarly the appearance of the groups C−, D, and E, in a population (City B) where they were non-existent in the earliest adulthood, to include altogether 35% of the population in the eighth and 55% in the ninth decade, is a striking indication of the slowing down of the abilities of older people as far as intelligence test performance of the type used here is concerned.

At this point the question may well be raised: are the successive decade groups for the period of maturity homogeneous? In other words is the decrement registered (1) by the regular drop from decade mean to decade mean, or (2) by the diminishing percentages in the upper brightness classifications and the increasing percentages in the lower levels, actually a function of increasing age or are the lower scores of the older groups due to differences of some other sort. If, for example, one sex regularly scores lower than the other and if the older age groups are disproportionate as compared to the younger with reference to their male and female constituents (not true of our populations) the decrement would be spuriously attributed to age. Or if the younger groups contain actually a larger proportion of the more able persons of the population as compared to the older another confusing element would be introduced. This latter is quite unlikely, for actually the opposite is very probably true. Length of life seems positively related to general mental ability and resistance to interference from the "weight of years" may be proportional to mental ability. The data gathered by one of the writers in a study of the intelligence of eminent men show a positive relation between longevity and IQ estimated from extensive biographical material.[13] The average IQ estimate for eminent men who lived to be 80 years of age or more was not less than 5 IQ points higher than the average IQ estimate of men of shorter lives. The correlation between IQ and longevity for the group as a whole is not very large but it is of practical significance when it is recalled that the intelligence range under consideration here was fairly narrow. Therefore, the decade decrements which we have found for the sample populations tested with the Otis S-A examination are theoretically less than would be shown if the same identical population could have been tested at succeeding 10-yr. intervals.

There may be other more obscure elements tending to produce heterogeneity in the populations from decade to decade but the present data offers no means for isolating or examining them. The question of sex differences and of the constitution of the groups in terms of educational status is, however, directly amenable to investigation.

(*2*) *Sex differences.* The score curve for the ages from 50–90 yr. in City A is based on the tests of 37 males and 87 females, the curve for the ages from 15–90 yr. in City B is derived from tests of 272 males and 344 females. The decade means for the sexes separately with equivalent scores, Otis mental age, IQ, percentile, and Army Alpha score appear in Tables XIV to XVII inclusive. In Table XVIII the statistical significance of the differences between the means for the sexes appear decade by decade for each of the cities. It is quite clear from these tabulated results that the scores for the males tend to average higher than those for the females, but they are not always higher. In the 12 comparisons 9 are higher and of these only 3 show D/σ diff. greater than 2; the highest is 2.97 which is just 'probably significant.' When the decades are combined into larger age periods, the 5 differences turn out 4 in favor of the men and 1 (age range 30–49, City B, −0.70) in favor of the women. There are 2 (3.58 and 2.61) that seem to indicate possibly or probably true superiority in the scoring ability of the men in question over the women from the corresponding age groups. As the trends are not regular (see Fig. 4) we are inclined to regard the difference exhibited between the sexes as possibly due to errors in sampling. But it will be noticed that in both cities the curve for the females drops more rapidly than that for the males from the 'fifties' to the 'sixties' and there is something of a proportional recovery after the seventh decade.

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[13] C. M. Cox, The early mental traits of three hundred geniuses, *Genetic Studies of Genius*, II, 1925, Table 12 A, facing p. 60.

TABLE XIII

Percentage of Adults by 20-yr. Periods in 7 Brightness Classes
(618 Cases, Sexes Separate and Combined, City B)

	15-29			30-49			50-69			70-89			90-109		
Class	M	F	Both	M	F	Both	M	F	Both	M	F	Both	M	F	Both
A	55.6	35.4	45.0	30.0	34.0	32.2	15.7	8.5	11.4	—	—	—			
B	30.1	30.8	30.2	22.5	18.0	20.0	19.1	17.7	18.3	25.0	5.7	13.5			
C+	11.1	27.7	19.4	26.2	30.0	28.3	30.3	26.9	28.3	16.7	20.8	19.0			
C	3.2	4.6	4.7	11.3	13.0	12.2	19.1	24.6	22.4	22.2	32.1	28.1			
C−	—	1.5	0.7	6.3	5.0	5.6	11.2	10.0	10.5	8.3	13.2	11.2		100.0	100.0
D	—	—	—	—	—	—	1.1	6.9	4.6	8.3	9.4	8.9			
E	—	—	—	3.8	—	1.7	3.4	5.4	4.6	19.4	18.9	19.0			
Total	100.0	100.0	100.0	100.1	100.0	100.0	99.9	100.0	100.1	99.9	100.1	99.7	0	100.0	100.0
No. Cases	63	65	129	80	100	180	89	130	219	36	53	89	0	1	1

TABLE XIV

OTIS S-A INTELLIGENCE SCORES, BY DECADES AND 20-YR. PERIODS

(37 Adults, Males, from City A)

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
12	50s	27.2	3.2	1.48	39.0	14·10	97	39	76
13	60s	23.1	9.4	.37	34.0	14·0	92	25	62
8	70s	21.5	9.9	.87	31.0	13·5	89	17	56
4	80s	16.5	9.2		24.0	12·2	82	6	32
25	50-69	25.1	10.0	1.51	36.0	14·4	94	30	67
12	70-89	19.8	10.0		28.2	12·10	86	12	46

TABLE XV

OTIS S-A INTELLIGENCE SCORES, BY DECADES AND 20-YR. PERIODS

(87 Adults, Females, from City A)

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
31	50s	25.9	10.6	2.45	37.0	14·6	95	33	70
36	60s	19.8	9.6	−.03	28.3	12·10	86	12	46
17	70s	19.9	9.7	1.24	28.4	12·10	86	13	47
3	80s	11.8	10.5		17.0	10·10	75	2	19
67	50-69	22.6	10.6	1.48	33.0	13·10	91	22	60
20	70-89	18.7	10.2		26.5	12·6	85	9	40

TABLE XVI

OTIS S-A INTELLIGENCE SCORES, BY DECADES AND 20-YR. PERIODS

(272 Adults, Males, from City B)

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
21	15-19	41.7	8.7	.30	59.7	17·9	118	93	143
43	20-29	41.1	8.2	3.51	59.	17·8	117	93	140
41	30-39	34.2	9.7	.14	49.2	16·5	107	73	105
38	40-49	33.8	12.6	.96	48.8	16·4	107	72	103
50	50-59	31.3	10.6	1.08	46	16·0	104	64	93
43	60-69	28.9	10.9	1.59	42.4	15·5	100	51	84
29	70-79	24.7	11.3	1.45	36	14·4	94	30	67
7	80-89	9.9	7.2		14	10·4	72	1	16
64	15-29	40.6	10.0	3.92	58.6	17·8	117	93	136
79	30-49	34.0	11.4	2.34	49	16·5	107	72	104
93	50-69	30.3	10.6	3.43	44.5	15·9	103	60	89
36	70-89	21.9	13.2		32.0	13·7	90	20	56

The difference between the mean scores of all of the males and all of the females of City A is 0.84 in favor of the men; it is 0.41 σ diff. The difference between the means for the sexes (ages 15 to 90 yr.) in City B is 2.76; this is 2.91 σ diff. but here it should be noted that the older (lower score) groups contain more women than men, while the younger (higher score) groups are fairly equal.

TABLE XVII

OTIS S-A INTELLIGENCE SCORES, BY DECADES AND 20-YR. PERIODS

(344 Adults, Females, from City B)

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
30	15-19	36.2	6.7		52.1	16'9	110	80	114
37	20-29	36.2	6.3	0	52.1	16'9	110	80	114
46	30-39	35.9	9.2	.16	51.8	16'9	110	80	112
52	40-49	34.4	10.4	.79	49.8	16'6	108	75	106
69	50-59	28.9	8.9	3.06	42.5	15'5	101	52	84
60	60-69	23.9	9.8	2.85	34.8	14'2	93	28	63
39	70-79	20.9	8.1	1.60	30.5	13'4	89	17	55
11	80-89	17.8	9.1	1.04	25.7	12'5	84	8	36
67	15-29	36.2	6.6		52.3	16'9	110	81	115
98	30-49	35.2	10.0	.78	50.8	16'8	109	77	109
129	50-69	26.6	9.9	4.26	39.0	14'10	97	38	76
50	70-89	20.3	8.5	6.31	29.5	13'1	88	15	52

For the present study a more important fact than the rather slight, but possibly characteristic sex difference, is that when the sexes are regarded separately, from decade to decade throughout maturity, each shows a considerable score decrement. This quite definite tendency has already been indicated by the coefficients of correlation for the general groups as presented in Table IX. Without very large populations the differences could scarcely be made out from year to year. When we compare one 10-yr. period with the next they are not usually large enough to be beyond statistical doubt especially in our smaller groups of data from City A. But for 20-yr. periods the changes are sufficiently large to be quite convincing.

The graphic presentation of the results for the sexes separately in the two cities, Fig. 4, shows again that the downward trend of the intelligence scores as age increases is definitely a characteristic of both sexes. A weighted score that allowed for the differences in the relative sizes of the populations for the sexes from one decade to another would therefore show the same general trend as that indicated for our populations as they stand. No matter how or from which side we picture them the facts indicate progressive decrement in the intelligence scoring ability of the human being with increasing age.

TABLE XVIII

SEX DIFFERENCES IN MEAN INTELLIGENCE SCORES OF ADULTS CLASSIFIED BY DECADE AND DUO-DECADE AGE PERIODS

(Otis 15-min. Score)

		City A				City B				
Age Periods	Sex	No. Cases	Mean	σ_m	D/σ diff.	Sex	No. Cases	Mean	σ_m	D/σ diff.
15-19	M					M	21	41.7	1.9	2.45
	F					F	30	36.2	1.2	
20-29	M					M	43	41.1	1.2	2.97
	F					F	37	36.2	1.0	
30-39	M					M	41	34.2	1.5	−0.87
	F					F	46	35.9	1.4	
40-49	M					M	38	33.8	2.0	−0.24
	F					F	52	34.4	1.4	
50-59	M	12	27.2	.93	0.61	M	50	31.3	1.5	1.33
	F	31	25.9	1.90		F	69	28.9	1.1	
60-69	M	13	23.1	2.61	1.08	M	43	28.9	1.7	2.33
	F	36	19.8	1.61		F	60	23.9	1.4	
70-79	M	8	21.5	3.50	0.38	M	29	24.7	2.1	1.53
	F	17	19.9	2.36		F	39	20.9	1.3	
80-89	M	4	16.5	4.60	0.62	M	7	9.9	2.7	−2.03
	F	3	11.8	6.08		F	11	17.8	2.8	
15-29	M					M	64	41.1	1.06	3.58
	F					F	67	36.3	.81	
30-49	M					M	79	34.0	1.26	−0.70
	F					F	98	35.1	1.0	
50-69	M	25	25.1	1.99		M	93	30.3	1.10	2.61
	F	67	22.6	1.29	1.06	F	129	26.6	.87	
70-89	M	12	19.8	2.89		M	66	21.9	2.19	0.64
	F	20	18.7	2.29	0.30	F	50	20.3	1.20	

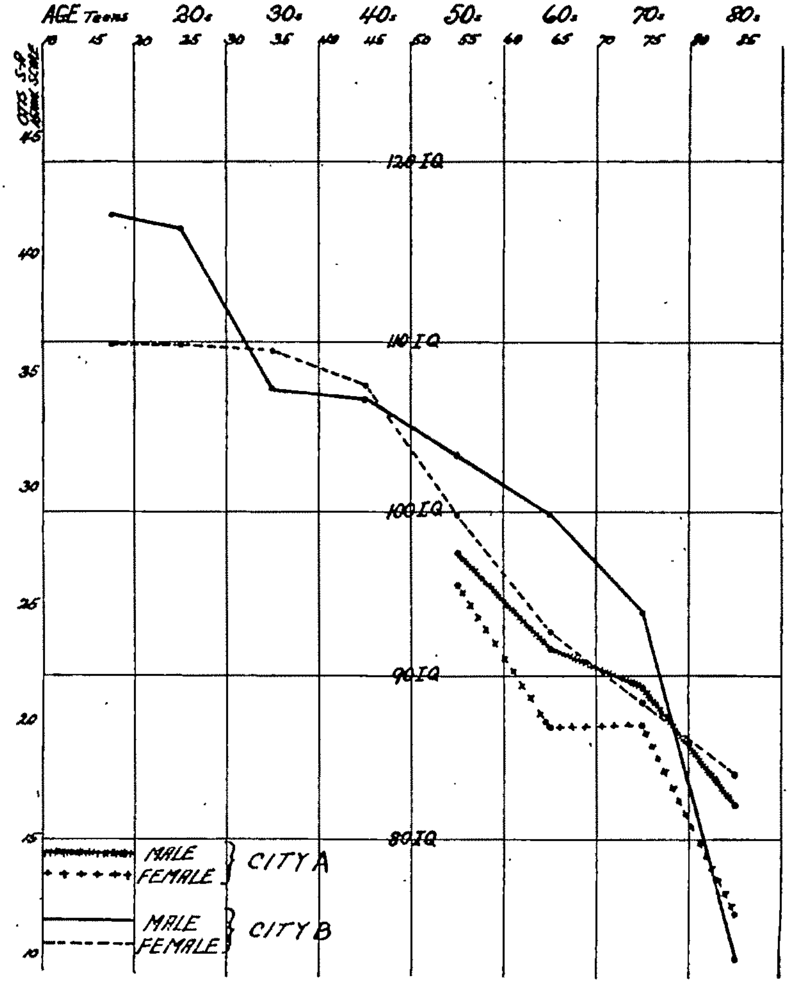

FIG. 4. DISTRIBUTION OF MEAN INTELLIGENCE SCORES BY SEXES AND BY DECADES

(3) *Educational levels.* The lower position of the mean curve for City A as compared to that for City B has been referred to. The differences between the means for the two cities at successive decades from the 6th to the 9th are 2.0, 3.1, 1.2, and 0.06 times the respective σ diff. When the two populations were analyzed as to educational status an explanation was found in the lower educational rating of the individuals in City A compared to those in City B. In our tested population from City A there were 92 persons who had given answers about their education which made possible classification into one or another of three groups: grade school (0 to 8 years); high school, normal or business course (9 to 12 years of schooling); college (1 to 10 years of college training). In City B

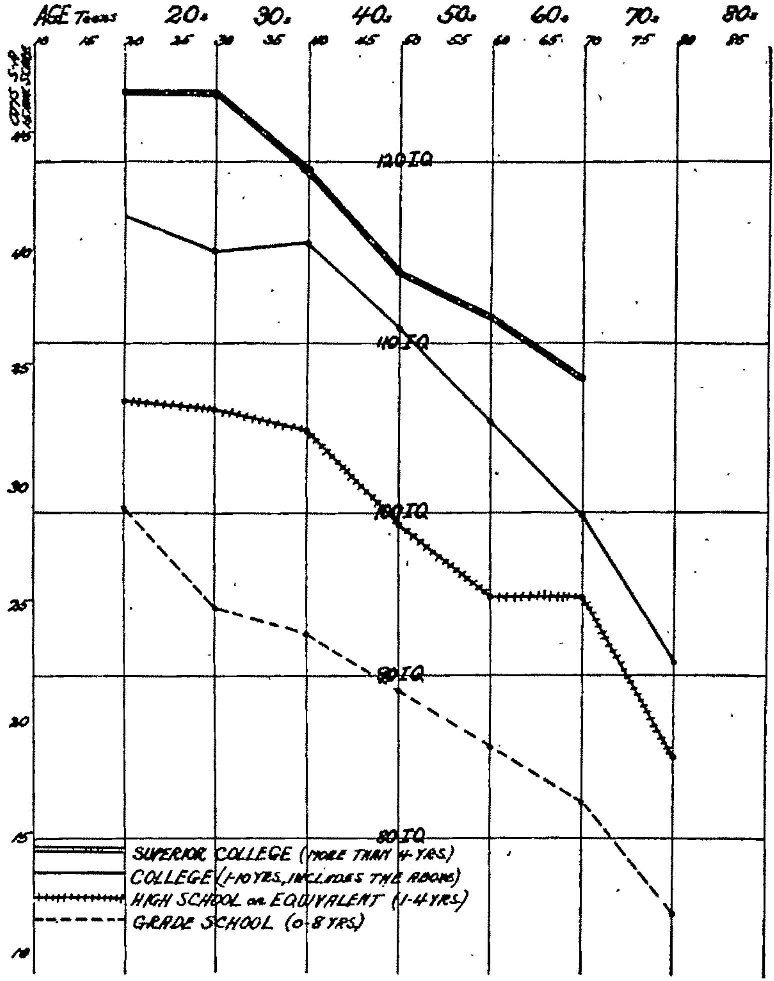

Fig. 5. Decade Distributions of Mean Intelligence Scores for Groups Representing 4 Educational Levels
(Data from both cities combined)

for the same age period there were 289 persons who could be so classified. The percentage of *S*s in each of the classes was as follows.

	City A	*City B*
Grade School	60%	27%
High School	26%	44%
College	14%	29%

This striking dissimilarity in the constitution of the two groups and the resultant divergence of the curve of one from that of the other suggested the desirability of analyzing both populations in terms of educational status.

In deriving mean scores for the three educational levels decade by decade the results from the two cities are combined. The means are presented in Tables XIX to XXI inclusive with the ratios giving the significance of the differences between successive means, and also the equivalent scores in terms of Otis mental age, IQ, percentile, and Army Alpha score. Table XXII gives the significance of the differences between the educational means decade by decade, and Table XXIII the correlations between age and score for each sex and the sexes combined in each educational level. Plotted curves for the three contrasted groups appear in Fig. 5. Here in addition to the college group as represented in the tables we have set off a superior (more than 4 yrs.) college population.

TABLE XIX

Otis S-A Intelligence Scores by Decades for Groups with Grade School (0-8 yr.) Education

	Otis 15-min. S-A Scores				Equivalent Measures				
No. Cases	Decade	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
3	20s	29.3	6.4	.88	42.5	15'5	101	52	84
11	30s	24.9	10.8	.33	36.0	14'4	94	30	67
18	40s	23.7	6.8	1.10	35.0	14'2	93	28	64
41	50s	21.3	9.6	1.25	31.0	13'5	89	18	56
45	60s	18.9	8.0	1.47	27.0	12'8	85	10	41
36	70s	16.5	6.4	1.64	24.0	12'2	82	6	32
12	80s	11.7	9.6		17.0	10'10	75	2	19

TABLE XX

Otis S-A Intelligence Scores by Decades for Groups with High School (9-12 years) Education

	Otis 15-min. S-A Scores				Equivalent Measures				
No. Cases	Decade	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
24	20s	33.7	6.4	0.22	48.7	16'4	107	72	103
36	30s	33.3	8.0	0.39	48.3	16'3	106	71	101
29	40s	32.5	8.8	2.06	47.2	16'2	105	67	98
62	50s	28.5	8.8	1.80	41.5	15'2	100	48	81
51	60s	25.3	9.6	0	36.8	14'6	95	33	69
30	70s	25.3	8.0	2.27	36.8	14'6	95	33	69
6	80s	18.5	6.4	1.54	26.5	12'7	85	9	39
1	90s	14.5	0		21.0	11'7	79	4	26

It is strikingly evident, first of all, that the same tendency for increasing age to be associated with intelligence score decrement is a characteristic of each of the educational groups. In IQ points lost from the 'fifties' to the 'nineties' there is little to choose between the three, the people of grade school education lose 26 points, those with high school training 28, and those with college opportunity 27. The correlations suggest that there is a slightly stronger relationship between score and age the higher the educational class.

TABLE XXI

OTIS S-A INTELLIGENCE SCORES BY DECADES FOR GROUPS WITH COLLEGE (1-10 YR.) EDUCATION

Otis 15-min. S-A Scores					Equivalent Measures				
No. Cases	Decade	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
51	20s	41.7	6.4	1.19	59.5	17˙9	118	93	142
39	30s	40.1	7.2	−0.21	58.0	17˙6	116	91	136
37	40s	40.5	9.6	1.80	58.5	17˙7	117	92	138
42	50s	36.9	7.6	2.00	53.0	16˙11	111	82	117
35	60s	32.9	9.6	1.47	48.0	16˙3	106	70	100
19	70s	28.9	9.6	2.91	42.0	15˙4	100	50	84
1	80s	22.5	0		33.0	13˙10	91	22	60

TABLE XXII

INTELLIGENCE SCORES BY DECADES AT THREE EDUCATIONAL LEVELS WITH THE SIGNIFICANCE OF THE DIFFERENCES BETWEEN THEM

(Both Cities, Both Sexes)

Decade		No. cases	Mean	σ	D/σ diff.	
20s	Sup. Col. & Col.	51	41.7	.9	5.0	3.2
	H.S. & Norm. & Bus.	24	33.7	1.3	1.0	
	Gr. sch.	3	29.3	3.8		
30s	Sup. Col. & Col.	39	40.1	1.0	4.2	4.3
	H.S. & Norm. & Bus.	36	33.3	1.3	2.3	
	Gr. sch.	11	24.9	3.3		
40s	Sup. Col. & Col.	37	40.5	1.6	3.5	7.3
	H.S. & Norm. & Bus.	29	32.5	1.6	3.8	
	Gr. sch.	18	23.7	1.6		
50s	Sup. Col. & Col.	42	36.9	1.2	5.2	8.2
	H.S. & Norm. & Bus.	62	28.5	1.1	4.0	
	Gr. sch.	41	21.3	1.5		
60s	Sup. Col. & Col.	35	32.9	1.6	3.6	7.0
	H.S. & Norm. & Bus.	51	25.3	1.4	3.6	
	Gr. sch.	45	18.9	1.2		
70s	Sup. Col. & Col.	19	28.9	2.2	1.4	5.0
	H.S. & Norm. & Bus.	30	25.3	1.5	3.1	
	Gr. sch.	36	16.5	1.1		
80s	Sup. Col. & Col.	1	22.5	0	1.5	4.0
	H.S. & Norm. & Bus.	6	18.5	2.6	1.8	
	Gr. sch.	12	11.7	2.7		

TABLE XXIII

Correlations between Age and Intelligence Score in the Age Range 20 to 95 yr. at Three Educational Levels

(Cities A and B Combined)

Educational Level	Sex	No. Cases	Product-moment r
Grade School	M	74	−.401 ± .065
(0-8 Grades)	F	92	−.361 ± .061
	M & F	166	−.394 ± .045
High School	M	64	−.413 ± .070
(9-12 Grades)	F	175	−.509 ± .038
and N. and B.	M & F	239	−.403 ± .037
College	M	121	−.436 ± .050
(1-10 years)	F	103	−.544 ± .047
and S. Coll.	M & F	224	−.439 ± .036

The decrement from decade to decade within the three educational levels is not large. No one of the differences is statistically significant for our comparatively small groups. But the fall is almost regularly continued whatever the degree of education and the result is a loss of 4 to 5 mental age years during the period of maturity.

The striking feature of the present comparison is the practical advantage of the person of greater education and higher initial IQ over the person of lesser education and poorer initial mental equipment. The individual who starts with the ability that measures during the period of growth at or near the average falls to the 6th intelligence score percentile for adults by the time he reaches age 75. The person who starts near the upper quartile falls during the same period to the low average score indicated by the 33rd percentile. The person whose rating during the period of growth registers at the 93rd percentile scores even in the eighth decade at the 50th intelligence score percentile which is the average for adults in general and an average based in large part on groups in the early maturity age range. Individuals having at this later period in life an intelligence scoring ability that is average or above for active adults and combined with it a great wealth of experience, easily maintain their eminence and leadership in the affairs of men and may successfully continue their business and professional activities to very advanced age. Such people are honored and revered, and exemplify human life at its acme of effectiveness and fulfillment.

Finally, in Table XXIV, the percentages by decade and educational level are given for the seven brightness classes. The correspondence between degree of education and degree of brightness is striking, and perhaps we might say encouraging. Class A numbering 117 persons shows no one who did not go beyond the grade school, and a total of 41% of those with some college training fall in this class. Class C registers only 13 people with college records as against 59 for high school and 50 for the grades. The D and E classes, as anyone might have predicted beforehand, were not wholly devoid of 'college people;' however they are few, 4 as against 60. But age shows its influence also and the fall from the higher to the lower classes persists regularly in a diagonal line across the table from decade to decade, whatever the extent of educational training and advantage.

TABLE XXIV

Brightness Classes by Decades at Three Educational Levels

(Cities A and B; both sexes)

		Decade								Total % all decades combined	Total no. cases all decades combined
		20s	30s	40s	50s	60s	70s	80s	90s		
A	Gr. sch.									0	
	HS & NB	16.6	20.0	24.1	4.9	7.8	3.2			10.9	26
	S. Col. & Col.	58.0	47.4	60.5	31.0	23.5				41.0	91
B	Gr. sch.	33.3	25.0		9.8	2.2				5.4	9
	HS & NB	33.3	34.3	10.4	18.0	9.8	19.3			18.9	45
	S. Col. & Col.	38.0	34.2	10.5	35.7	29.4	36.8			30.6	68
C+	Gr. sch.	33.3	16.6	33.3	24.4	20.0	5.7	8.3		18.7	31
	HS & NB	33.3	31.4	44.8	32.8	29.4	25.8	16.6		31.9	76
	S. Col. & Col.	4.0	15.8	23.7	26.2	23.5	26.3			18.5	41
C	Gr. sch.	33.3	33.3	44.4	29.3	24.5	34.3	16.6		30.1	50
	HS & NB	16.6	11.4	13.8	31.2	29.4	32.3	50.0		24.8	59
	S. Col. & Col.		2.6		4.8	14.7	21.0	100.0		5.9	13
C−	Gr. sch.		8.3	22.2	12.2	22.2	25.7	8.3		18.1	30
	HS & NB		2.9	6.9	9.8	11.8	6.5		100.0	7.6	18
	S. Col. & Col.			5.3	2.4	5.9				2.3	5
D	Gr. sch.				7.3	11.1	11.4	16.6		8.4	14
	HS & NB				3.3	5.9	6.5	16.6		3.4	8
	S. Col. & Col.						10.5			.9	2
E	Gr. sch.		16.6		17.1	20.0	22.9	50.0		19.3	32
	HS & NB					5.9	6.5	16.6		2.5	6
	S. Col. & Col.					2.9	5.3			.9	2
Total %	Gr. sch.	99.9	99.8	99.9	100.1	100.0	100.0	99.8		100.0	
	HS & NB	99.8	100.0	100.0	100.0	100.0	100.1	99.8	100.0	100.0	
	S. Col. & Col.	100.0	100.0	100.0	100.1	99.9	99.9	100.0		100.1	
Total no. cases	Gr. sch.	3	12	18	41	45	35	12			166
	HS & NB	24	35	29	61	51	31	6	1		238
	S. Col. & Col.	50	38	38	42	34	19	1			222

DISCUSSION

The results presented in this paper we have qualified because of the size of the samples and the character of the populations studied, but we have no reason to surmise that the findings would be significantly different if the investigation were to be repeated on a larger scale in several communities representing different social and economic strata. We have been careful to describe and discuss our data as measures of intelligence in the form of scores on the 15-min. Otis S-A Higher Examination. While not inclined to weave a halo about this test we are nevertheless bound to express some confidence in it as an indicator. For the practical purpose in hand we know of no better present-day instrument. The examination was administered with great care as to procedure and detail and under ideal conditions. It bore the caption, "Good-Judgment Question Series," with no mention of "intelligence" or "mental ability." It was not forbidding, in fact the contrary, as proven by the record that only 3 people out of 856 for any subjective reason balked at trying. A positive score could be made by practically everybody from 7 years of age to the oldest persons who could leave their homes, while at the same time only 4 members of the rather large group examined were able to finish, having tried all 60 items offered. The trustworthiness of the 15-min. test score compared to a similar test of 20- or 30-min. duration has been amply demonstrated, as well as in comparison with much longer examinations.

We examined intelligence by what is called a speed test in distinction to a power test. The circumstances of the experimental program seemed to demand this technique. Of course it may be objected that aging is itself a biological slowing down and that effective intelligence in the mature is not necessarily a thing that, to work well, must function at whip-cracker speed.

This common observation on the decreasing speed of the old is doubtless a popular way of recognizing and naming the progressive decrement discussed in this paper. While the speed test may seem to some unfair to the old, we can not close our eyes to the strong probability indicated by many well conducted studies, which we need not discuss now, that speed and power in mental operations are closely related and at base may well be dependent on the same conditions.

The factor of disuse or practice we have not been able to analyze or equate in the successive decade groups. It seems probable that the test items are not greatly at fault in this respect, as evidenced by the very wide applicability of the test. Subjects did not complain of inability to understand the wording of the items nor did they claim that the questions were unfamiliar in form or substance. Naturally some of those in the 'teens' were more familiar with being tested but there was probably little difference in this respect for all adults over 30 years of age.

Taking all of these things into consideration we see no conditions requiring that a special discount be levied against our findings nor, on the other hand, should they be considered beyond criticism. To use a phrase from Claude Bernard, "The investigator should have a robust faith and yet not believe." We have tried to do both of these things in the present study. But it is our opinion that anyone who works with a large or moderately large body of data, representative of normal human intelligence throughout the life-span, is certain to be impressed with the general regularity of the results and the characteristic course taken by the mean values for the successive ages.[19]

SUMMARY

(1) The correlation of intelligence scores with chronological age during childhood and youth is well-known and has often been demonstrated. Hypothetical curves for the course of intelligence in later life have been drawn, usually from studies on more or less atypical groups and with relatively few data for ages beyond 50 years. The correlation coefficients previously reported have ranged from −0.011 to −0.35.

(2) The relation between age and intelligence throughout the life span was investigated in the present study by securing intelligence scores on samplings, (from two fair sized American towns), for every decade and semi-decade from 7 to 92 years of age.

(3) All the intelligence tests for both children and adults, males and females, were made by means of a large type edition of the first 60 items of the Otis Self-Administering Test of Intelligence, Higher Examination, Form A, given as a 15-min. test. Such a 15-min. test

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[19] We cannot close this brief presentation of the tabular analysis of these data on later maturity without acknowledging the financial help of the Carnegie Corporation of New York and the faithful work of three assistants: Mrs. J. S. Carroll, Mrs. W. L. Iversen, and Mrs. I. J. Cooper, all of whom conducted examinations.

has been found to correlate +0.86 ± .02, with scores on a similar 30-min. test in the limited range of superior ability represented by 121 university students. The test was administered individually under ideal conditions. Only 3 people out of a total of 856 for any reason objected to taking the examination, which passed under the name of "Good-Judgment Question Series." Only 4 people of the entire group were able to complete the test in the 15-min. period.

(4) For the age span 7 to 17 years inclusive the coefficient found between age and score was +0.80 ± .023. The age group ranging from 15 yr. 0 mo. to 16 yr. 11 mo. registered a mental age of 17·4, and the next 2-yr. group, which represents the peak of ability in these findings, exceeds this level by only one month, (17·5).

(5) From the high point in the intelligence score curve represented at about 18 yrs. of age the trend is at first almost level, then gradually declines, dropping 15 or 16 mental age months by the chronological age of 50 years. In this period covered by the 3rd to the 5th decade the decline in Otis IQ points is from 115 to 104.5 and the correlation coefficient indicating the relation of score to age is −0.283 for males and females combined.

(6) The drop in intelligence score from age 18 to age 85 (City B) amounted to 62% of the original high score and in mental age months to a decline of 5 yr. and 10 mo. The correlation between age and score from the age of 20 to 95 years is approximately −0.50 ± .02 and the difference between the average scores in the 'twenties' and in the 'eighties' (City B) is 9.9 σ diff.

(7) Distribution of the Otis 15-min. scores in terms of the Army brightness classification, A to E, shows that the Classes A and B progressively decrease with increasing age, and disappear altogether in both populations in the 9th decade. Groups C−, D, and E, practically non-existent in the earliest adulthood in our population, include altogether 35% in the 8th and 55% in the 9th decade.

(8) The downward trend of intelligence scores as age increases is shown to be definitely a characteristic of both sexes. In 3 or 4 comparisons out of 5 the men demonstrate somewhat better ability to score on the test, but this superiority is not regular throughout the life span and may be attributable chiefly to errors in sampling.

(9) The two cities studied exhibited considerable average difference in educational level, but the curves for decline of score with age were found to follow parallel courses. When classified according to educational level all groups exhibited the same tendency for in-

creasing age to be associated with intelligence score decrement. From 50 to 90 years people of grade school education lost 26 Otis IQ points, those with high school training 28, those with college opportunities 27 points.

(10) The correspondence between the degree of education and the degree of brightness was prominent. There was a slightly stronger relationship between score and age in the highest educational class, but the great advantage of the individual who starts with high initial ability is evident. The person who registers, in youth, at the 93rd percentile score, according to the rate of decline found, would be in the 50th intelligence score percentile for average adults on reaching the 8th decade of life.

THE RÔLE OF FORM IN PERCEPTION

By Harry Helson and Elizabeth V. Fehrer, Bryn Mawr College

The work of the configurational psychologists has brought the question of form into the focus of interest once again. In much of the experimentation on apparent movement form has been found to play a determining part and this has led to generalizations concerning the rôle of form in perception and behavior. A number of terms, supposedly descriptive of forms, have crept into the literature and have been accorded the status of 'laws.' Such terms as *Prägnanz*, closure, and good or bad have been used to describe, explain and predict how complexes behave, but their meanings and the precise limits of their operation have not been clarified with usage.[1] While the suggestiveness and fruitfulness of new terms are not denied it is customary in science to demand that words shall have precise, univocal meanings. Moreover it is time that the concepts underlying broad generalizations forming a new system of psychology be submitted to experimental verification. If the form of a stimulus complex influences what is seen or reacted to, then we should be able to find quantitative evidence for such influences and if some forms are 'better' than others their superiority should emerge under changing conditions in which the only variable is the form factor. This study was therefore undertaken to determine if form plays a decisive rôle in perception by measuring the lower thresholds for light when, all other factors constant, the form of the stimuli was changed. A further aim was to discover if some forms, notably the circle which has been proclaimed the perfect figure by the configurationists, were better than others according to some quantitative criterion.

If the liminal value changes for different forms, then form is certainly of importance in many ways: in the measurement of the light sense, in many laboratory experiments in vision or in other modalities, and in the systematic position to be accorded form as such. But if

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[1] For a review and criticism of Gestalt psychology the reader is referred to H. Helson, The psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-216. W. D. Turner (Intra-serial effects with lifted weights, *ibid.*, 43, 1931, 1-25) has criticized the concept of Prägnanz as unclear.

differences in form do not result in measurable differences in observation then the generalization, often made, that form is of universal and primary importance in perception and other modes of behavior will have to be retracted or modified in favor of a truer statement of its actual efficacy.[2]

HISTORICAL

The problem of form *qua* form, as well as the general problems raised by *Gestalttheorie*, is by no means a new one in psychology. Perhaps one of the first to emphasize and recognize the rôle of form in perception was Ernst Mach who wrote as early as 1886: "In examining two figures which are alike but differently colored we recognize their sameness of form at the first glance, in spite of the differences of color-sensation. The sight-perceptions, therefore, must contain some like sensation components. These are the space-sensations which are the same in the two cases."[3] He also points out that two squares, one on a side, the other on a corner, could never be recognized as the same type of figure without "mechanical and intellectual operations in addition to the sensory processes in the perception." Husserl,[4] writing in 1891, expressed himself in terms that sound strangely modern: "Wir erfassen ihre Configuration (*i.e.* eine sinnliche Menge) ganz wie einer Qualität in einem Blick." He later declares that the quality belonging to a group as such is its figural factor and, anticipating Köhler, while admitting the importance of the relations among the members of the group, denies that they are solely responsible for the unified quality of the whole.

It remained for Von Ehrenfels to give the facts such a twist that the attention of psychologists was gained and a large literature devoted to the subject of the *Gestaltqualität*. The controversy over the status of the form-quality involved almost every psychologist of the time and contains almost every argument, pro and con, advanced today with regard to the importance of the concept of *Gestalt*. While on the theoretical side it drew Meinong, Schumann, Cornelius, Höfler, Bentley, and even Stumpf, through his student Gelb, into the fray, it gave rise to some beautiful experimentation as found in the work of Schumann, Benussi, Seifert and a host of others.[5] It is impossible to review this voluminous literature at this point, so mention only will be made of the original statement by Von Ehrenfels and the contributions of Schumann in order to show what general problems were introduced through a consideration of the form factor in perception.

In asking his question which was to be debated for the next forty years, Von Ehrenfels said: "An individual *S* comprehends a melody which consists of n tones, and, on the other hand, n individuals hear each one of the n tones. Does *S* comprehend more than the other n individuals taken together? There must be something more in the form than a sum of elements. In the perception of spatial figures, is there more in a space form than n determinations of position, or the lines plus relations making up the figure?"[6] He then advances arguments which demonstrate the difficulties of atomistic theories designed to explain *Gestaltqualitäten* as sums of sensations: "for if a space-form is nothing but a sum of local determinations, various space-forms should be the more alike the more their single elements are alike. But since the local determinations depend upon the state of the visual field, then they should be more alike the nearer they are placed together. On this hypothesis it is impossible to see how two rectangles remain exactly alike no matter how far apart they are separated from each other and why two rectangles near each other are no more alike than two separated from each other."[7] Form depends therefore upon something other than similarity of elements and the relation of similarity depends upon more than a likeness of parts. The two criteria determining form-quality are then given: it is over and above the elements in the configuration (a new positive ideational content in consciousness) and it is transposable from one set of elements to another, hence independent of them.

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[2] The configurationists' emphasis upon form will be found in practically all of their publications. It recurs again and again. Less definite synonyms for *Gestalt* which are often used are: unity, whole, pattern, complex, structure, and *Bauplan* (Köhler). This is not a matter of different translations of the same word, for the German literature abounds in numerous terms and statements referring to factors supposed to have a part in the total *Gestalt*.

[3] E. Mach, *Analysis of Sensations*, 1897, 137.

[4] E. G. Husserl, *Philosophie der Arithmetik*, 1891, ch. 11, 22.

[5] A full discussion will be found in I. M. Bentley, Psychology of mental arrangement, this JOURNAL, 13, 1902, 268 ff. From 1902 on the literature is covered by Helson's review, *op. cit.*

It can be easily seen from this statement of the problem raised by Von Ehrenfels and elaborated by many others who discussed it after him that we really have a number and variety of issues, some of which are independent from our concern in this paper. Before passing to the work of Schumann which relates directly to our narrower problem of form, suffice it to say here that the reaction to Von Ehrenfels' "positive (and new) ideational content" was negative! While most psychologists were ready to admit the reality of the form-quality as a perceived datum, many refused to grant that a new sensation had been discovered or that radically different concepts were necessary to explain it. The consensus of opinion seemed to be that "parts in relation" would suffice to take care of the facts stressed by von Ehrenfels although Husserl had examined this type of explanation and demonstrated its weakness.

Schumann, while refusing to grant that there was anything radically new in Von Ehrenfels' findings, proceeded to investigate the problem of form in perception and published between 1900 and 1904 a series of papers on the problem that should, and probably will, be classical. Anyone reading these papers cannot doubt for a moment that in them are to be found the really direct precursors of Wertheimer, Köhler, and Koffka; in them will be found observations, explanations and glimmerings that might well be mistaken for material published in the *Forschung* of 1930. Schumann, moreover, *consciously* said these things. To find half-baked, unconscious, semi-poetical anticipations of present trends in earlier writers is to give credit where none is really due. But in the case of Schumann no one can doubt that he realized the importance of the factors stressed by Von Ehrenfels before him and the *Gestalttheoretiker* afterwards. Moreover, Schumann was careful to point out, what later writers have not been so careful to do, that the concept of form is ambiguous and that it may become a mere name for a phenomenon and not an explanation.[8] To attempt to summarize Schumann's contributions would be beyond the scope of this paper and tantamount to repeating much that passes for *Gestaltpsychologie* today. We mention only a few of the points stressed by Schumann: equal distances between members help form groups; surfaces between groups appear larger than equal surfaces between members of groups; nearness helps in the formation of groups; contours of unities tend to stand out in perception; incomplete figures tend to be seen as complete; ambiguous figures may be seen as 'good' figures now this way, now that; certain figures display properties characteristic of them as such; some parts of a figure bring out certain properties while others bring out still other properties; vertical symmetry is conducive to connectedness; *probably the main locus of the properties of figures is to be sought in central factors.* Schumann rejects Von Ehrenfels' new element, preferring to explain his findings by asserting that figures possess characteristic properties belonging to them as such. Nor is his discussion limited to vision: analogies and suggestions abound in his articles for memory, the affects, and the various sense modalities.[9]

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[6] C. von Ehrenfels, Über Gestaltqualitäten, *Vierteljahrsschrift f. wiss. Phil.*, 14, 1890, 249 ff.

[7] *Idem.*

From Schumann to the work of the modern school of *Gestalt* is but a very small step, if any at all. The work of this school has been summarized and criticized by Helson[10] so that it is only necessary to point out here that much of the experimental work of the configurationists and much of their theorizing is directed toward showing the importance of the form factor in perception and behavior. While there is ample evidence that form plays a part in the perception of movement, in binocular and stereoscopic vision, after-images, figure-ground phenomena and in many other places, it appears from a survey of the literature that the concept, with the concept of the *Gestalt*, suffers from ambiguity. In his articles on *Gestalt* Helson pointed out that the concept of the *Gestalt* suffered from manifold ambiguities and unclearnesses in meaning the following: (1) the form of an apprehended whole; (2) some factor within a group which dominated the whole; (3) the totality of conditions determining a perception, memory, or behavior pattern; (4) physical structures; (5) physiological structures; (6) biological structures; (7) logical structure; (8) psychological structure; (9) purpose; (10) necessary and sufficient conditions, etc.[11] While Köhler in his recent book has somewhat dispelled the haze surrounding the concept of the *Gestalt* by defining it as any "segregated whole or unit" and has elucidated the relation between the *Gestalt* and form by telling us that "in some respects form is the most important property which a whole may have,"[12] nevertheless, as Schumann pointed out, the word "unity" is itself open to criticism. Furthermore, the assertion that

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[8] F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmungen, *Zsch. f. Psychol.*, 23, 1900, 1-32.

[9] *Ibid.*, 29.

[10] *Op. cit.*

[11] *Op. cit.* The ambiguity of the concept of *Gestalt* which was pointed out by the senior author on the basis of his survey of the configurational experimental literature in the articles referred to above reappeared when univocal criteria defining "goodness" of figure were sought in the present study. It may be of interest to note that the criticisms levelled against the configurationists by the senior author in 1926 on theoretical considerations are amply borne out by this experimental study.

[12] W. Köhler, *Gestalt Psychology*, 1929, 219.

"the most primitive phenomena are figural"[13] and statements to the same effect which assert that form is present in the earliest and simplest experiences need to be tested as generalizations by an appeal to experiment in as wide a variety of problems as possible. So, too, must the various characterizations of forms as good, bad, *prägnant*, and the like, be submitted to observational and quantitative tests to determine (1) whether they have precise, verifiable and unequivocal meanings; and (2) whether they denote the constitutive or important determiners of perception.

Some work has been done directly upon the question of the priority of form *versus* content and the results do not substantiate Koffka's claim given above. The work with patients suffering from brain lesions resulting in disturbances of visual function has shown that form perceptions may be lost while brightness and color vision remain. This fact has been reported not only by Poppelreuter and others, but substantiated by Gelb and Goldstein[14] who belong to the so-called school of *Gestaltpsychologie*, yet nothing has been made of it, in spite of its crucial importance for the claims of configurationists who assert that form is primary and prior in perception.

Several investigations carried out in this country both on the skin and in vision agree that form is not primary as such. Zigler and Northrup, in their 1926 study, report that, in spite of the fact that their *O*s were instructed to look for forms when geometrical figures were applied to the volar surface of the left forearm, "all *O*s were often unable to perceive any shape at all, not even enough to warrant a guess." "A definite and complete form was the exception rather than the rule."[15] The *O*s felt pressures as such before forms were felt. They conclude that "tactual form is not so definitely and unfailingly apprehended as tactual pressure. . . . Tactual pressure is invariably perceived, while tactual form is not definitely given in a considerable percentage of cases, and is not apprehended at all in an appreciable number of instances."[15]

Again, in 1927, Zigler and Barrett found that form does not appear first in tactual perception.[16] They describe three stages in perception: (1) an initial or preliminary stage in which a touch of indeterminate shape is felt; (2) an intermediate stage in which some prominent feature suggested a tentative but unsatisfactory shape; (3) the stage of definitely perceived form with greater or less clarity.

When we come to vision we discover that forms do not appear first in perception any more than they do on the skin. Zigler, Cook, Miller, and Wemple in an investigation of the perception of form in peripheral vision report:[17] (1) there is no form in the periphery of vision, only *figureless-background*; (2) as the stimulus passes toward the center of vision, formless figures are seen; (3) then comes a form resembling the figure actually used as stimulus; (4) and finally, clear form.

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[13] K. Koffka, *The Growth of the Mind*, 1925, 132.

[14] W. Poppelreuter, Zur Psychologie der optischen Wahrnehmung, *Zsch f. d. ges. Neurol. u. Psychiat.* 83, 1923, 26 ff.; A. Gelb and K. Goldstein, *Psychologische Analysen hirnpathologischer Fälle*, 1920, 36 ff.

[15] M. J. Zigler and K. M. Northup, The tactual perception of form, this JOURNAL, 37, 1926, 393 f.

[16] M. J. Zigler and R. Barrett, A further contribution to the tactual perception of form, *J. Exper. Psychol.*, 10, 1927, 186.

[17] M. J. Zigler, B. Cook, D. Miller, and L. Wemple, The perception of form in peripheral vision, this JOURNAL, 42, 1930, 246-259.

Nor do they find any differences among forms in the place where they change from formless to clear—all change at about the same region of the retina. Form as such not only fails to appear, but exerts no effect upon what is seen and where it is seen, otherwise there would be a difference for different forms. These authors tell us that "No type of figure manifests a consistent advantage over others in the matter of being perceptible farther afield." Exactly similar results have been stated by Kleitman and Blier.[18]

PROBLEM

The present study was undertaken in an endeavor to determine if form as such exerted a constitutive effect upon the lower threshold of vision and, if such effects were found, what forms were 'good,' 'bad,' or 'indifferent.' Furthermore, we hoped to be able to set up criteria for the various types of forms, believing that those which emerged in the various categories could be classified by some quantitative or qualitative norm coming from the experiments themselves. In all of these our expectations were disappointed as the following pages will show.

EXPERIMENTAL PROCEDURE

Our experiments consisted in determining the lower limens for light, just noticeable form, and certain form for 6 different figures, all of the same area. The figures used were: an isosceles triangle, standing on its base, 44.72 × 44.72 × 63.24 mm.; a rectangle, on its longer side, 50 × 20 mm.; a circle, diameter of 35.6 mm.; a semicircle, resting on its straight side, diameter of 50 mm.; an angle which consisted of a square, 35 × 35 mm. from which a smaller square 15 × 15 mm. had been taken, leaving an angle having an area of 1,000 sq. mm. like the other figures and presented like an inverted V; and finally, a square, 31.62 mm. on a side.

The figures were cut out of black cardboard and placed in front of the milk glass screen forming the front surface of the Nagel adaptometer.[19] The experiments were carried on after 30 min. of dark adaptation in a dark room. Before going into the dark room all *Os* gazed at a white wall illuminated by a lamp of small wattage in order to start dark adaptation from approximately the same level of light adaptation. Both periods of adaptation insured a fairly stable state for the observations to determine limens.[20] A 1-min. rest intervened between observations. A small red cross was so reflected from a mirror that it appeared to come from about 3 cm. above the stimulus figure. The purpose of the red fixation light was to insure foveal vision both because forms are best seen in the fovea and because it is necessary to work only on one part of the retina. The red of our fixation light helped insure the latter because red is a photopic color, *i.e.* its brightness is no greater for the rest of the retina than for the fovea and hence there is no tendency to turn the eye away from foveal vision to a place of greater sensitivity. Furthermore, in order to insure foveal vision the *Os* sat at a distance of 275 cm. from the instrument which made the angle subtended by both fixation and stimulus lights not greater than one degree—viz. 57′. This angle falls well within the foveal region which is usually said to be 2° 30′.

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[18] Quoted by Zigler, Cook, Miller, and Wemple, *op. cit.*, 247.

[19] For a description of the apparatus see W. Lohmann, Über Helladaptation, *Zsch. f. Sinnesphysiol.*, 41, 1907, 304 ff.

[20] This is the time usually taken to insure equilibrium, although some experimenters believe 15-20 min. sufficient.

With 3 *Os*, limens for 'just perceived light,' 'just perceived form,' and 'certain form' were determined separately, while the fourth *O* was instructed to increase the intensity of the light by turning a handle until she first saw light when she was to report its form if she could and the degree of certainty. If she was wrong or uncertain she was instructed to keep turning until she was right in her report and certain of it. We thus obtained three sets of limens for each *O* on each figure: the limen for just noticeable light—since light was practically always seen before any trace of form was visible; the limen for just noticeable form—whether or not the form named was correct; and the limen for certain form—when the form exposed was correctly named and the *O* felt sure of his report. We have called these values respectively the Light Limen; Form 1; and Form 2.

By dividing the values given in our tables into the constant 0.0144 they can be translated into foot-candles of illumination or apparent foot-candles. As they stand, our figures represent measures of sensitivity and hence the actual limens vary inversely with them.

Only the ascending series in the Method of Limits was finally used in our computations for the following reasons: (1) at its best the lower limen for light is subject to large standard deviations when measured by a fine instrument and the inclusion of ascending and descending values would have given such large deviations that we feared differences among forms might be masked; (2) had we used the descending series throughout our experiments, we would not have been able to determine whether forms are seen coincidently with light, before, or afterwards—one of the points our study was undertaken to settle; (3) knowledge of the form prevented the *Os* from reporting when they actually disappeared in the descending series and thus contributed to the large deviations. In order therefore, to keep our *Os* in ignorance of the stimulus at each observation and to reduce the size of the deviations, which is considerable in any case, we presented our stimuli in chance order, used only the ascending series in the Method of Limits, and began with a different setting of the handle which the *O* turned to increase the intensity of the stimulus light.

RESULTS

(*1*) *Form as a primary datum.* The first and, perhaps, most significant result appears from a comparison of the limens for just noticeable light, Form 1 and Form 2, in Tables I, III, and V. In Table I are given the values of the light limens for each *O* on each figure, as well as the average value for all *Os* on each figure. Tables III and V give individual and group values for Form 1 and Form 2. We find a tremendous difference in the limens for the three stages of perception as the following averages, for all *Os* on all figures, for the three cases show: Light = 647.11; Form 1 = 44.76; Form 2 = 26.03.

TABLE I

VALUES OF 'LIGHT' LIMENS AND σ FOR EVERY *O* ON EVERY FIGURE

		Triangle	Square	Rectangle	Circle	Semicircle	Angle
D	L	33.38	32.39	38.75	40.00	34.10	39.50
	σ	19.69	19.43	18.21	17.60	13.98	21.73
H	L	2003.20	1852.60	1916.23	1943.40	1658.70	1972.84
	σ	781.25	648.41	638.74	621.89	727.83	789.13
M	L	401.50	385.92	425.51	377.19	351.24	410.75
	σ	128.48	100.34	127.72	125.73	122.93	127.33
Z	L	271.26	280.00	264.47	294.58	219.06	284.23
	σ	100.37	117.60	111.08	94.27	74.48	127.90
Av. L		677.35	637.72	661.24	663.79	565.77	676.83
	σ dis.	873.00	787.00	807.00	815.00	741.00	861.00
	σ m	61.74	55.66	57.07	57.64	52.40	60.89

TABLE II

SHOWING FOR THE VARIOUS PAIRS OF LIGHT LIMENS THE DIFFERENCE (D), THE STANDARD DEVIATION OF THE DIFFERENCE (σ DIFF.), AND THE RATIO (D/σ DIFF.) BETWEEN THOSE VALUES

Figures		Tri.	Sq.	Rect.	Circle	Semi-circle
	D	39.61				
Square	σ diff.	83.13				
	D/σ diff.	0.47				
	D	16.09	23.52			
Rect.	σ diff.	84.07	79.72			
	D/σ diff.	0.19	0.29			
	D	13.54	26.07	2.55		
Circle	σ diff.	84.46	80.13	81.11		
	D/σ diff.	0.16	0.32	0.03		
	D	111.56	71.95	95.47	98.02	
Semi-c.	σ diff.	80.98	76.45	77.48	77.90	
	D/σ diff.	1.37	0.94	1.23	1.25	
	D	00.50	39.11	15.59	13.04	111.06
Angle	σ diff.	86.72	82.50	83.46	83.84	80.33
	D/σ diff.	0.00	0.47	0.18	0.15	1.38

TABLE III

Values of Form I Limens and σ for Every *O* on Every Figure

		Triangle	Square	Rectangle	Circle	Semicircle	Angle
D	L	6.57	6.18	7.50	5.53	4.42	4.20
	σ	1.77	1.79	1.95	1.60	1.37	1.93
H	L	135.63	142.14	207.66	147.96	90.42	74.40
	σ	86.08	63.96	97.60	76.94	46.11	37.94
M	L	15.36	11.54	14.21	11.20	7.08	9.30
	σ	8.22	6.23	8.67	6.27	3.11	4.84
Z	L	30.42	27.70	33.00	30.00	25.33	26.56
	σ	16.73	14.40	15.50	12.30	14.44	14.61
Av. L.		46.99	46.89	65.59	48.67	31.81	28.61
σ dis.		68.04	64.59	96.65	69.97	42.35	34.45
σ m		4.81	4.56	6.83	4.95	2.99	2.42

TABLE IV

Showing for the Various Pairs of Form I Limens the Difference (D), the Standard Deviation of the Difference (σ diff.), and the Ratio (D/σ diff.) between those Values

Figures		Triangle	Square	Rectangle	Circle	Semicircle
	D	0.10				
Square	σ diff.	6.63				
	D/σ diff.	0.001				
	D	18.60	18.70			
Rect.	σ diff.	8.35	8.21			
	D/σ diff.	2.22	2.27			
	D	1.68	1.78	16.92		
Circle	σ diff.	6.90	6.73	8.44		
	D/σ diff.	0.24	0.26	2.00		
	D	15.18	15.08	33.78	16.86	
Semi-c.	σ diff.	5.66	5.45	7.46	5.78	
	D/σ diff.	2.68	2.76	4.52	2.91	
	D	18.38	18.28	36.98	20.06	3.20
Angle	σ diff.	5.38	5.15	7.25	5.51	3.85
	D/σ diff.	3.41	3.54	5.10	3.64	0.83

These differences mean that the sensitivity of our *O*s is nearly 25 times as great to 'just noticeable light' as to 'certain form;' nearly 15 times as great to light as to 'just perceived form' wherein the forms are usually reported incorrectly; and nearly twice as great to 'just perceived form' (unclear, hazy, guessed-at forms) as to 'certain forms.' The lowest limen is for light without form, the highest for clearly perceived forms, with the uncertain stage between the other two extremes. It appears that far from being primary in perception, form perception is the result of mechanisms at a much higher level of integration than those responsible for the perception of formless light. Our results substantiate the conclusions reached by various other workers and are in direct opposition to the assertion that form as such is present at the lowest levels of perception or that every perception must necessarily be of the figure-ground variety.

(*2*) *Light limens.* It might be maintained that even though form is not perceived at the lower limit of vision that it is nevertheless effective below the limen in that it determines what is seen, or has some influence upon the threshold value. This assumption may be tested by determining if there are significant differences between the limens for the different figures.[21] This has been done in Tables II and VII, the former giving the differences, σ, and σ diff. for all *O*s as a group, the latter giving the same information for each *O* separately in order to show individual differences, if any, between *O*s. Whether we group our data or not, both tables fail to show any significant differences among figures in their limens for just noticeable light. One exception to this statement is found for two figures for one *O* (*Z*) who happens to give a significant difference between her limens for circle and semicircle but since this is only one case out of a possible 60 and disappears when we treat her among the other *O*s in a group, it may well be due to chance, especially since this *O* shows

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[21] When the ratio of the difference between two measures to the σ diff. is 3 or more, we have considered the difference as significant, otherwise not. It is obvious that the chances of a significant difference are nearly as great when the value of this ratio is 2.99 as when the value reaches 3. Our reason for neglecting all ratios below 3, no matter how close they may be to 3, is that the line must be drawn somewhere and since 3 is the generally accepted value indicating significance, we have adopted it. A lower value would not affect our conclusions in any way even though more significant differences among figures would thereby appear. The reader must keep this fact in mind throughout the rest of our discussion because we have taken as our criterion of significance this statistical value. On the other hand, the absolute values speak for themselves and justify our conclusions. But when we come to compare figures with each other some standard or statistical norm is necessary before we can even guess as to whether a difference in absolute values is significant.

TABLE V

Values of Form 2 Limens and σ for Every *O* on Every Figure

		Triangle	Square	Rectangle	Circle	Semicircle	Angle
D	L	2.10	2.20	2.16	1.63	1.44	1.11
	σ	0.63	0.51	0.63	0.51	0.45	0.36
H	L	80.37	95.04	197.30	77.97	60.00	46.17
	σ	38.58	52.27	82.87	36.65	25.80	20.78
M	L	3.70	3.60	3.46	3.09	2.77	2.99
	σ	1.25	0.97	1.03	0.96	0.62	0.77
Z	L	6.09	6.26	7.80	5.90	6.28	5.50
	σ	2.56	2.50	3.27	1.71	2.01	1.43
Av. L		23.06	26.77	52.68	22.15	17.62	13.94
σ dis.		38.35	46.95	93.25	37.12	27.74	21.38
σ m		2.71	3.32	6.59	2.62	1.96	1.51

TABLE VI

Showing for the Various Pairs of Form 2 Limens the Difference (D), the Standard Deviation of the Difference (σ diff.), and the Ratio (D/σ diff.) between those Values

Figures		Triangle	Square	Rectangle	Circle	Semicircle
Square	D	3.71				
	σ diff.	4.28				
	D/σ diff.	0.86				
Rect.	D	29.62	25.92			
	σ diff.	7.13	7.38			
	D/σ diff.	4.15	3.51			
Circle	D	.91	4.62	30.53		
	σ diff.	3.78	4.23	7.09		
	D/σ diff.	0.24	1.09	4.30		
Semi-c.	D	5.44	9.15	35.06	4.53	
	σ diff.	3.34	3.86	6.80	3.27	
	D/σ diff.	1.62	2.37	5.15	1.38	
Angle	D	9.12	12.83	38.74	8.21	3.68
	σ diff.	3.19	3.65	6.76	3.02	2.47
	D/σ diff.	2.94	3.51	5.73	2.71	1.48

TABLE VII

SIGNIFICANT DIFFERENCES BETWEEN FIGURES FOR LIMENS OF LIGHT, FORM 1, AND FORM 2

		Triangle			Angle			Semicircle			Circle			Rectangle		
O		Light	F-1	F-2	Light	F-1	F-2	Light	F-1	F-2	Light	F-1	F-2	Light	F-1	F-2
	Angle	1.60	5.08	9.17												
	Semi-c.	.20	6.31	5.80	1.71	.62	3.99									
D	Circle	1.82	2.98	4.00	.12	3.26	5.79	2.05	4.58	1.93						
	Rect.	1.36	2.49	.47	.34	6.33	9.56	1.45	6.04	6.26	.53	5.17	4.49			
	Square	.03	1.08	.84	1.84	4.45	10.13	.49	4.92	6.90	2.07	1.86	5.12	1.60	3.41	.31
	Angle	.18	5.13	5.59												
	Semi-c.	2.21	2.99	3.16	1.98	1.83	2.87									
H	Circle	.42	.72	.31	.20	6.08	5.38	1.98	4.51	2.81						
	Rect.	.60	3.43	8.13	.38	7.01	11.55	1.78	7.16	10.94	.53	3.25	8.41			
	Square	1.91	.41	1.61	1.56	5.95	6.80	.64	4.34	4.65	2.07	.40	1.91	1.41	4.00	6.14
	Angle	.61	4.47	3.59												
	Semi-c.	1.76	7.48	14.06	2.33	2.68	1.58									
M	Circle	.73	2.74	2.71	1.34	1.70	.58	1.04	4.58	2.04						
	Rect.	1.20	.66	1.04	.58	3.66	2.61	2.88	6.04	4.27	1.92	1.98	1.80			
	Square	.42	2.57	.42	1.08	1.99	3.49	1.49	4.92	5.31	.38	.26	2.54	1.75	1.78	.70
	Angle	.58	1.20	1.47												
	Semi-c.	2.40	1.59	.40	2.79	.42	2.26									
Z	Circle	1.20	.14	.22	.46	1.21	1.24	3.75	1.62	1.03						
	Rect.	.31	.89	2.86	.82	2.13	4.88	1.72	2.50	2.92	1.40	1.21	3.96			
	Square	.45	.86	.32	.12	.43	1.90	2.71	.80	.05	.62	.83	.85	.71	1.82	2.63

the fewest number of significant differences under all conditions. We are justified, on the basis of these results, in concluding that form is not a constitutive factor in the perception of light as such.

(*3*) *Form limens.* If we compare each of the 6 figures with every other there are 15 possible comparisons for the *Os* as a group, 60 comparisons if we treat the results separately for the 4 *Os*. This is true for Form 1 and Form 2 as well as for light perception. In Table VII the individual results have been listed for all three cases, the lack of significant differences for the light limens having been mentioned already. We can therefore turn our attention to the results for Form 1 and Form 2, discussing the former first. Treating the *Os* as a group, we see from Table IV that out of 15 possible significant differences, only 5 are actually significant as follows: triangle and angle; square and angle; rectangle and semicircle; rectangle and angle; and circle and angle. Four out of these 5 significant differences concern the angle and some other figure. It appears as if the low sensitivity value of the angle makes it a 'bad' figure compared with the others. But when we inspect the results for each *O*, as given in Table VII under Form 1, we find that other figures show as many significant differences as the angle, indeed more: the semi-circle shows 11 significant differences and the angle only 10 whereas in the group results the angle emerges as significantly different from several other figures. From the individual results the rectangle emerges with 11 significant differences, and since it appears significantly different from two other figures in the group results we might conclude that its high sensitivity value sets it apart as a 'good' figure from some of the others, even though less than half the figures are significantly different from it. Out of 60 possible significant differences in the individual results, we find a total of 26 really significant.[22] One *O*, (*Z*) shows no significant differences in the Form 1 limens! Before drawing any conclusions as to 'goodness' or 'badness' of forms we must see what still different criteria tell us.

Comparing the values given in Tables V, VI and VII for Form 2 (correctly perceived forms), we notice that out of 15 possible significant differences for the group results (Table VI) only 6 are significantly different. These differences concern the following figures: triangle and rectangle; square and rectangle; square and angle; rectangle and circle; rectangle and semicircle; rectangle and angle. Five of these 6 differences concern the rectangle which appears to have a significantly higher sensitivity value than any of the other figures since, for the group as a whole, it gives a significant difference when compared with every other figure used. While according to the criterion of just perceived form the angle emerged significantly worse than four of the other figures (all but one!), according to the criterion of certain or correctly perceived form the angle is significantly lower than only two other figures. Hence the angle does not appear so 'bad' when the *O*s are required to report its form when certain of it. This shows, as we shall have occasion to point out later, that although the angle was not seen as readily as the other figures when just noticeable forms were reported, when it was seen it tended to be reported correctly, having fewer false reports than other figures which might have appeared 'better' from the point of view of 'apparent shape.' (Cf. Table XIV which gives the percentage of times each figure was correctly reported.)

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[22] It should be noted that the number of significant differences is just twice as great when we add the number for each figure as it is when we take the total number of all figures because in the former case each significant difference is counted twice, *e.g.* if rectangle and angle are significantly different so are angle and rectangle for both will be counted when we treat the figures individually, although there is really only one difference involved.

It is difficult to summarize what we have found with respect to 'goodness' of form from the results of Form 1 and Form 2. While the angle appeared to have a lower sensitivity value than the other figures for Form 1 in the results of the group as a whole, as a matter of fact an examination of the individual results showed that the semicircle was perhaps worse, having 11 significant differences in all. The rectangle also showed 11 significant differences in the individual results but when the results were pooled in the group results, both the superiority of the rectangle and the badness of the semicircle fail to appear. On the other hand, in the group results for Form 2 the superiority of the rectangle clearly is demonstrated since it is found to have a significantly higher sensitivity value than any of the other figures. Here, too, the angle does not appear to be so bad as it was in Form 1 instructions. If we take the individual results for form 2, we find 12 significant differences in all for the angle, 11 for the rectangle, and 10 for the semicircle; the latter, like the angle, having a lower sensitivity value, in general, than the other figures. The individual results (Table VII, under Form 2) make the semicircle appear nearly as bad as the angle, but in the group results its sensitivity value is significantly lower than one figure only!

Logically, and according to statistical canons, we have no right to draw conclusions from the group results to the exclusion of the individual differences, and *vice versa*. But we may take what both methods of computing significant differences agree in, viz., the angle is not a good figure under Form 1 conditions, but is fairly good under Form 2 conditions (being little worse than the semicircle in the individual results for Form 2): if we were to choose the best figure, it would be the *rectangle* whose limen, according to both group and individual results, is significantly lower than that of any other figure.

To those who are familiar with the configurational literature it may appear strange that the circle which has been touted as such a good form fails to qualify as either a good or the best shape since it neither tends to be seen first among all the forms nor to be reported correctly at a lower liminal value than the other figures—in fact its sensitivity average is third for Form 1 and fourth for Form 2. While the circle may be a 'good' figure on the basis of physical considerations, and under other conditions of experimentation, in our experiments it never fulfilled the high expectations which the hypotheses based upon *physische Gestalten* would lead one to entertain.[23] This merely demonstrates that what is a good configuration from the point of view of physical considerations may be a mediocre or poor shape from the point of view of psychological observation.

It appears, therefore, that while there may be significant differences among forms for some *Os when they are seen*, different conclusions respecting such differences may be arrived at, depending upon whether we base them upon individual results, group results, or Form 1, or Form 2 conditions. But the possibilities governing our judgments of form have not yet been exhausted for there are still other criteria, springing directly from observation, upon which forms may be called good, bad, or indifferent.

(*4*) *Goodness of form as a matter of the criterion used.* If we scrutinize our results still further we find that a form may be good or bad on several counts, each leading to a different form as the best or worst.

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[23] Thus Köhler states that the law of *Prägnanz* is exemplified in the tendency of *der Kreis als Endergebnis in der Physik wie im Gesichtsfeld besonders ausgezeichnet* [*zu sein*] (*Brit. J. Psychol.*, The problem of form in perception, 14, 1923, 268). It should be noted that Köhler says explicitly that the circle is preferred not only from physical considerations but in the visual field as well, as this quotation proves. In none of our experiments and by none of the numerous criteria which we are using does the circle appear *besonders ausgezeichnet*. Nor does the failure of the circle to qualify as a good or the best figure arise from the fact that we have chosen criteria unfavorable to it as a figure—we have tested each figure by every criterion that our quantitative results would allow leaving it to the facts to speak for themselves.

If we take as a criterion of goodness that figure which was reported as seen first the largest number of times in the first 50 observations on Form 1, we find from Table VIII, wherein the figures actually exposed are given horizontally and the figures reported, whether correct or not, are given vertically, that the triangle was seen 298 times; rectangle, 215; square, 215; circle, 198; semicircle, 142; and angle, 132. By this token the triangle could be taken as the "best" figure since it tends to be seen more frequently than any other form when any form at all is seen. Table VIII also shows the number of times each figure was "confused" or mistaken for every other in the first 50 observations on Form 1. Thus, reading from the top of the table to the left, we see that the triangle was reported as triangle 189 times out of 200 (4×50), but 52 times the semicircle was reported as triangle and 57 times the angle was reported as triangle, while the square and circle were not reported as triangle by any *O* at any time in the observations for Form 1. The average number of times each figure was reported as seen and the σ of this average are given in Table VIII and utilized in Table IX where the differences, the σ diff. and the D/σ diff. are shown in order to determine if there is a significant difference in the number of times each figure tended to be reported. Significant ratios, *i.e.* 3 or above, appear for every figure, but the triangle alone is significantly different from *all* the other

TABLE VIII

REPORTS FROM ALL *O*S ON FORM

(First 50 Observations)

Figure Seen as	Figures exposed Tri.	Rect.	Sq.	Circ.	Semi-c.	Angle	Total	Av.	σ
Tri.	189	0	0	0	52	57	298	74.5	9.23
Rect.	1	198	0	0	16	0	215	53.7	0.82
Square	0	0	180	30	1	4	215	53.7	5.80
Circle	0	0	20	170	1	7	198	49.5	5.59
Semi-c.	4	2	0	0	124	12	142	35.5	7.08
Angle	6	0	0	0	6	120	132	33.0	8.51

TABLE IX

Significant Differences in Reports on Form for All *Os*

(First 50 Observations)

		Triangle	Rectangle	Square	Circle	Semicircle
Rect.	D	20.75				
	σ diff.	4.63				
	D/σ diff.	4.48				
Square	D	20.75	0.00			
	σ diff.	5.45	2.93			
	D/σ diff.	3.81	0.00			
Circle	D	25.00	4.25	4.25		
	σ diff.	5.40	2.83	4.03		
	D/σ diff.	4.63	1.50	1.05		
Semi-c.	D	39.00	18.25	18.25	14.00	
	σ diff.	5.82	3.56	4.58	4.51	
	D/σ diff.	6.70	5.13	3.98	3.10	
Angle	D	41.50	20.75	20.75	16.50	2.50
	σ diff.	6.28	4.28	5.15	5.09	5.54
	D/σ diff.	6.61	4.85	4.03	3.24	0.45

shapes while the semicircle and angle are both significantly different (in tending to be reported less frequently than other figures) from 4 other figures. Whether the 3 significant differences each for rectangle, square, and circle mean any more than the fact that the other three figures are reported a greater (triangle) or fewer (semicircle and angle) number of times, we do not attempt to say. It should be noticed that the significant differences for the last three figures account for the significant ratios found for the first three. Certainly on the basis of these findings, different forms tend to be seen more or less frequently and hence form, if seen at all, plays a part. But since each figure was presented the same number of times every other figure was, it appears as if the conditions favoring apparent form reside as much within the organism as outside in the objective configuration. The ambiguous figures are certainly the semicircle and angle since they are apt to be called something else more frequently than the other figures. If we were to pick the best figure from the point of view of the figure which 'appears' most frequently, regardless of the form actually presented, then the triangle emerges as the best figure! Yet the theorizing of the configurationists which has been based upon the properties of physical configurations would never have led us to believe that the triangle would be preferred in perception!

Lest we generalize too hastily concerning which is the 'best' figure let us turn to the other criteria which may be used to determine the 'best' shape. If we take as the best figure that one which was reported *correctly*, or required the fewest number of repetitions for 50 *correct* reports on form (Form 2 conditions),[24] we find that the rectangle gets first place. In Table X are given the number of times each figure had to be presented to each *O* in order to get 50 correct reports on form, the averages for all *O*s and the σ of these averages. Again the angle and semicircle get a bad rating since they required the largest number of exposures before being named correctly. In Table XI are given the differences between the averages of the different figures, the σ diff. and D/σ diff. From this table we learn that the rectangle required a fewer number of exposures than three other figures, while the semicircle and angle required a larger number of exposures than three other figures, significant differences only being taken account of here. The square and circle would appear to be neither significantly good nor bad.

TABLE X

NUMBER OF TIMES EVERY FIGURE HAD TO BE PRESENTED TO OBTAIN 50 CORRECT REPORTS ON FORM

	Rect.	Circle	Square	Angle	Tri.	Semi-c.
D	50	56	50	70	50	74
H	51	56	62	89	58	67
M	50	65	50	53	50	68
Z	51	77	58	82	53	80
Av.	50.50	63.50	55.00	73.50	52.75	72.25
σ	1.00	8.61	5.19	10.78	3.27	5.45

There are still other criteria of goodness. We have found that there are no significant differences in the amount of light necessary to see the various shapes at the lower threshold of vision, *i.e.* in the limens for light. In Table XII are given the limens in order of

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[24] Here the *O*s were required to increase the intensity of the stimulus until they were sure of the form seen. If they reported they were certain and named the form incorrectly, the stimulus was repeated at a later time. Since we were trying to determine the lower threshold for 'certain' form it was necessary to adopt a procedure that would give the lowest value possible for all figures. On the other hand the number of times figures were incorrectly reported could be counted and the different figures compared not only with respect to their thresholds but also as regards the number of exposures necessary for 50 correct reports on form. Under the conditions of Form 1, on the contrary, we were interested mainly in 'apparent' form, whether correctly reported or not, so far as stimulus was concerned.

TABLE XI

Significant Differences between Figures in the Number of Times They Were Presented to Obtain 50 Correct Form Reports

		Semi-c.	Triangle	Angle	Square	Circle
Triangle	D	19.50				
	σ diff.	3.17				
	D/σ diff.	6.15				
Angle	D	1.25	20.75			
	σ diff.	6.03	5.63			
	D/σ diff.	.21	3.86			
Square	D	17.25	2.25	18.50		
	σ diff.	3.76	3.06	5.90		
	D/σ diff.	4.58	.73	3.13		
Circle	D	8.75	10.75	10.00	8.50	
	σ diff.	5.08	4.59	6.89	5.02	
	D/σ diff.	1.72	2.34	1.45	1.69	
Rect.	D	21.75	2.25	23.00	4.50	13.00
	σ diff.	2.76	1.70	5.41	2.64	4.32
	D/σ diff.	7.88	1.32	4.25	1.70	3.00

TABLE XII

Limens in Order of Magnitude

	Observer								
	Z		*D*		*M*		*H*		
	Order								
Light	(1) Circle	294	Circle	40	Rect.	425	Tri.	2003
	(2) Angle	284	Angle	39	Angle	410	Angle	1972
	(3) Square	280	Rect.	38	Tri.	401	Circle	1943
	(4) Tri.	271	Semi-c.	34	Square	385	Rect.	1916
	(5) Rect.	264	Tri.	33	Circle	377	Square	1852
	(6) Semi.-c.	219	Square	32	Semi-c.	351	Semi-c.	1653
Form 1	(1) Rect.	33	Rect.	7.5	Tri.	15	Rect.	207
	(2) Tri.	30	Tri.	6.5	Rect.	14	Circle	147
	(3) Circle	30	Square	6.2	Square	11.5	Square	142
	(4) Square	27	Circle	5.5	Circle	11.2	Tri.	135
	(5) Angle	26	Semi-c.	4.4	Angle	9	Semi-c.	90
	(6) Semi-c.	25	Angle	4.2	Semi-c.	7	Angle	74
Form 2	(1) Rect.	7	Square	2.2	Tri.	3.7	Rect.	197
	(2) Semi-c.	6.2	Rect.	2.1	Square	3.6	Square	95
	(3) Square	6.2	Tri.	2.1	Rect.	3.4	Tri.	80
	(4) Tri.	6	Circle	1.6	Circle	3.1	Circle	77
	(5) Circle	5.9	Semi-c.	1.4	Angle	2.9	Semi-c.	60
	(6) Angle	5.5	Angle	1.1	Semi-c.	2.7	Angle	46

magnitude for the various figures and for each *O* separately; the limens being for light, Form 1, and Form 2. We have summarized the data given in Table XII for all *O*s in Table XIII, where the figures are arranged according to their average limens and average rank order for light, Form 1 and Form 2. While an order is given in Tables XII and XIII for light limens, it means little or nothing since there are no significant differences in the limens for light. But if form is perceived, either under the conditions of Form 1 or Form 2, then significant differences do appear among the various shapes. From the positions of the rectangle (first) and the semicircle and angle (occupying the last two places) in Table XIII we might conclude that the former requires the least amount of light in order to be perceived while the latter require the greatest amount of light. These conclusions hold whether the average limens are taken or the average rank order of the different figures for Form 1 and Form 2 (Table XIII). Again the rectangle emerges as the best figure, a result we should never have expected from the configurationists' hypotheses regarding the physical bases of goodness of figures, while the angle and semicircle emerge as 'bad' figures.

TABLE XIII

ORDER OF FIGURES ACCORDING TO AVERAGE LIMEN AND AVERAGE RANK ORDER

Average Limen			Average Rank Order		
Light	Form 1	Form 2	Light	Form 1	Form 2
Tri.	Rect.	Rect.	Angle	Rect.	Rect.
Angle	Circle	Square	Circle	Tri.	Square
Circle	Tri.	Tri.	Tri.	Circle	Tri.
Rect.	Square	Circle	Rect.	Square	Circle
Square	Semi-c.	Angle	Square	Semi-c.	Semi-c.
Semi-c.	Angle	Semi-c.	Semi-c.	Angle	Angle

There is a final criterion by which we may seek to establish the goodness of shapes: the number or percentage of times each figure was reported *correctly* when it was first seen (under conditions of Form 1). We have seen how, in the first 50 reports on form (Form 1 conditions), some figures tended to be reported more often than others, hence were confused or reported incorrectly more frequently. By dividing the number of times each figure was reported as itself (*i.e.* correctly) by the total number of times it was seen, we get a measure of the correctness of report under the conditions of just barely perceived forms. The requisite data, taken from Table VIII, appear in Table XIV wherein it will be found that the rectangle was reported correctly more often than any other figure and the triangle, which was reported most frequently, was confused the largest number of times with other figures. Thus the triangle which seems to be the best figure from the point of view of tendency to be present phenomenally, would be adjudged the poorest figure in so far as its tendency to be confused or reported incorrectly is concerned. It is noticeable that the angle and semicircle get second and third positions by this criterion, showing that they are not so often mistaken, when reported, as other figures. The circle, supposedly a perfect figure from physical considerations, appears third from the bottom.

TABLE XIV

PERCENTAGE OF TIMES EVERY FIGURE WAS REPORTED CORRECTLY

Rectangle	92.0	Circle	85.8
Angle	90.9	Square	83.7
Semicircle	87.3	Triangle	63.4

What then does one mean by a good figure or a poor one? What reason have we for choosing one criterion of goodness rather than another? And if the results from different criteria do not agree, what generalizations can be drawn regarding goodness of form? If observation furnishes no single, univocal criterion of goodness, recourse to physical considerations followed by inference to phenomenal properties confounds confusion the more. The circle which should be a good figure from considerations of physical *Gestalten* stands first in none of our lists according to none of our criteria. Is goodness of form a matter of simplicity, compactness, differentiation, homogeneity or what? How should these terms be interpreted, in the language of physical distributions of energy or from the standpoint of observational behavior? If we are to be guided by physical considerations, then the circle which represents a balanced, minimum distribution of forces should be the best figure as has been asserted. In observation it might be said to be simple since it is a more homogeneous figure than the rectangle and triangle. But simplicity and other descriptive concepts, as Koffka has pointed out, require functional (and quantitative) tests. By none of our functional and quantitative tests did the circle qualify as the best figure. Unanimity as to what constitutes a 'good' figure in crude observation has not yet been reached and when we come to experimental tests of the concept its ambiguity is more apparent than ever.[25] What may be a good figure according to the judgment one makes in looking at a drawing under everyday conditions, or a good figure according to the shapes assumed by soap bubbles, or the behavior of figures in gamma movements, cannot be generalized into an all-pervading law and made the basis of a system of psychology. Worse still, the whole problem is further complicated by the fact of individual differences which we now discuss.

(5) *Individual differences.* The 4 *O*s in our experiments seem to fall into three groups so far as the influence of form in perception is concerned. An inspection of the individual results in Table VII for Form 1 and Form 2 shows that some *O*s are affected much more than others by the factor of form and at least one *O* is not affected at all or does not respond to form as such. Thus, for Form 1, out of a possible 60 significant differences (15 for each *O*) we find 20 each for *D* and *H*,[26] 12 for *M*, and 0 for *Z*. The results for Form 2 follow closely those for Form 1: 22 for *D*, 20 for *H*, 10 for *M*, and 4 for *Z*. It would appear that *O*s *D* and *H* are most amenable to the form factor in perception, *M* less amenable, and *Z* probably absolutely uninfluenced by form, even when forms are seen. These results, moreover, are what might be expected, both on *a priori* grounds and the findings of previous investigators: some *O*s respond rather easily to the form of a stimulus complex and all that it entails, other *O*s do not respond at all, and still others (if the 'trait' is normally distributed) fall in between the first two groups.[27] How, then, is it possible to assert that form is a primary datum for all *O*s, that it is first in the history of the individual and the race, that it is constitutive in perception and behavior? Even under the conditions most favorable to form as such, *i.e.* when the *O*s were required to report the form seen, some *O*s fail to discriminate between forms or to be affected by the shape of the stimuli. Having found that it is impossible to set up criteria governing the effectiveness of form *qua* form, since various criteria give

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[25] Rupprecht Matthaei (*Das Gestaltproblem*, 1929) has discussed the difficulties with trying to settle what a 'good' configuration is. It might well be asserted that while the circle is simple it is not membered, hence not a 'good' configuration.

[26] See note 22 for the explanation of the number of significant differences when individual figures are counted and added.

[27] Differences in individual reactions to the form factor in perception have led some writers (Benussi, Seifert) to assert that we are dealing with a difference in 'type'. If so, it would be interesting to discover if there are any correlations between susceptibility to forms or lack of it and other, perhaps more complex, functions. For a further discussion of types, see H. Helson, *op. cit.*, 210 ff.

contradictory results, we now find that it is impossible to legislate for all *O*s regarding any influence of form whatsoever since some *O*s are apparently unaffected by differences in forms, even when set to observe them. We are therefore forced by our experiments to deny that the factor of form is always or even, as Köhler has tried to ameliorate the generalization, "the most important property which a whole may have." It may be a determining factor for some individuals in some types of experiment, but this is different from the assertion that it is the primary or most important condition in perceptual behavior. The broad generalizations respecting the rôle of form in perception made by the configurationists constitute another example of the fallacy of generalizing from "some to all."

SUMMARY AND CONCLUSIONS

We have found that the problem of form is an old one in psychology, having been explicitly recognized by Mach, Von Ehrenfels, Husserl, and Schumann. To the latter belongs the credit for having *consciously* said that certain characteristic properties of figures belong to them by virtue of their figural properties. But Schumann was careful to point out that the concept of form, of a unity, and such terms was ambiguous and might hide important psychological facts. One of these facts as disclosed in the present study and confirmed by the work of others, is that form is not prior or primary in visual experience since the limens for light are in every case lower than the limens for just barely perceived form and are not influenced in any detectable manner by form. So far as the light sense goes, form is not a constitutive factor and plays no part in determining the value of the lower threshold. Furthermore, the fact that it requires 25 times as much light to recognize forms correctly as to barely perceive light, and nearly twice as much light to perceive forms in a hazy, indistinct manner as compared with just noticeable light shows that the light sense is mediated by a simpler, more primitive mechanism than that for forms. The assertion that forms are first and primary in perception therefore falls to the ground.

Can generalizations be made regarding goodness of form, supposing forms are perceived? Our quantitative results show that different criteria favor different forms: the form requiring the least amount of light is the rectangle; the form reported the largest number of times on the barely perceived level is the triangle; the form confused the least number of times with other forms is the rectangle; the form requiring the fewest number of reëxposures for 50 correct and certain reports on form is the rectangle; the form appearing neither good nor bad according to any criteria, the circle. If we were to choose the best form on the basis of all our criteria taken together, the rectangle would get first place. The circle, from the configurational point of view the best figure, never received first place according to any test, nor should we be led to expect from their considerations that the rectangle would have shown up so well.

Generalizations concerning goodness of form are still further complicated by the fact of individual differences: what emerges as the best figure according to group results does not necessarily hold for each member of the group: thus for some *O*s square and triangle have a lower limen for form than the rectangle. Finally, it appears that not all *O*s are equally affected by the form factor: one *O* showed no significant differences in her limens even when forms were being reported upon; another *O* shows so few that it is doubtful if they are significant; the other two *O*s seem to be affected by some forms and not by others.

We are thus led to conclude that while form may play some part in certain fields of perception, it has played, all in all, relatively little in these experiments designed to show its influence. That the configurationists have over-emphasized and over-generalized the importance of form as such seems to us to be borne out by the results of this study.

A GENETIC STUDY OF COLOR NAMING AND WORD READING

By Ernest Mayfield Ligon, Union College

The normal adult requires a longer time to name colors than to read the names of the colors presented as words. This fact has been known for more than 50 years, during which time a number of studies have been made of the problem and a number of theories advanced in explanation, but the problem is still unsolved. It was, therefore, in the hope of gaining further knowledge of the phenomena which would lead to the ultimate explanation, that the present study was undertaken.[1]

Experimental Procedure

The observations for the present study were made in the public schools of New London, Connecticut. The experimenters were students of psychology in Connecticut College. Each experimenter was provided with the Woodworth-Wells color naming test.[2] A word reading test was prepared, which was a typewritten sheet containing 100 words, the names of the colors on the color naming test. The two tests were made as nearly identical as possible, except that one contained the colored squares and the other the names of the colors. The two tests were arranged in reverse order so that the sequence of responses in them would not be identical. Time was taken with a stop-watch.

The experimenters were divided into two groups. Group I presented to their *S*s the color-naming test and then the word-reading test. Group II reversed this order. This was done to equalize in the averages any effects of practice and fatigue. A record was kept for every *S* of the name, sex, age, school grade, time for each test, errors, and any unusual observations. Later, class marks were obtained for two grades and intelligence test scores for seven.

The *S*s used from the 1st to the 6th grade constituted the entire student body of one public school, thus including all grades of intelligence and an approximately equal number of boys and girls. The 7th, 8th, and 9th grades were in different schools. Caution was taken to assure the presence of a random sample of each grade. In all except the 9th grade, pupils had been divided into at least two and sometimes three sections, called A, B, and C. This division had been made in an effort to

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[1] I am indebted in this study to my students at Connecticut College who acted as experimenters; to the public school authorities of New London, Conn., whose permission to work in the schools made the study possible; and to the teachers and pupils in the schools whose hearty coöperation was necessary to the success of the work.

[2] R. S. Woodworth and F. L. Wells, Association tests, *Psychol. Monog.*, 13 1911, (No. 57), 1-85.

have the best students in section A, and the poorest in C. This sectioning was also recorded in the experiment, thus affording a rough intelligence classification, which for group comparisons might be reliable.

The experiment was conducted during school hours with the most hearty coöperation of both teachers and pupils.

RESULTS

An analysis of the data obtained will now be presented. The problems on which data are available are as follows: (*1*) a comparison of the time required for the color naming and word reading functions, with *S*s classified both by school grade and chronological age; (*2*) the correlation between these functions; (*3*) the relationship between each function and the difference between them; (*4*) the relationship between each function and intelligence; (*5*) the frequency of errors and the relationship between the two functions for erring; and (*6*) sex differences in the two functions.

TABLE I

AVERAGE TIME FOR COLOR NAMING AND WORD READING TESTS BY SCHOOL GRADE

(The numbers in parentheses are the medians)

Grade	No.	Colors	P.E.	Words	P.E.	Diff.	P.E.	Diff./P.E
I	76	130.2	3.1	100.6	2.9	29.6	3.4	8.7
		(123.0)		(93.0)		(30.0)		
II	83	107.4	1.7	79.6	1.7	27.8	2.0	13.9
		(107.0)		(75.0)		(32.0)		
III	69	90.7		63.2	1.5	27.5	2.1	13.1
		(89.0)	1.6	(59.0)		(30.0)		
IV	74	88.3		59.6	1.0	28.7	1.3	22.1
		(87.0)	1.2	(58.0)		(29.0)		
V	75	81.0		52.1	.6	28.9	1.0	28.9
		(80.0)	1.2	(50.0)		(30.0)		
VI	84	77.3		50.5	.7	26.8	1.3	20.6
		(75.0)	1.3	(49.5)		(25.5)		
VII	59	73.5	1.1	45.0	.5	28.5	1.1	25.9
		(72.0)		(43.5)		(28.5)		
VIII	64	68.7	1.6	45.3	.7	34.4	1.6	14.6
		(68.0)		(45.0)		(23.0)		
IX	54	69.1	1.1	45.8	.6	23.3	1.1	21.2
		(69.5)		(46.5)		(23.0)		
Total	638							

(*1*) *Time required by the two functions.* In Tables I and II are the general results of the investigation. From them we may compare the temporal relations of the two functions.

Table I shows a comparison of all *Ss*, classified by school grades for the two functions. Attention is called to the very large number of cases, since this increases the reliability of the averages computed from them.

The average will be used as a measure of central tendency throughout the paper. The median is shown in this first table to show the very small difference between the average and the median. This justifies the exclusive use of the average as a measure of central tendency.

In Fig. 1 the data are presented graphically.

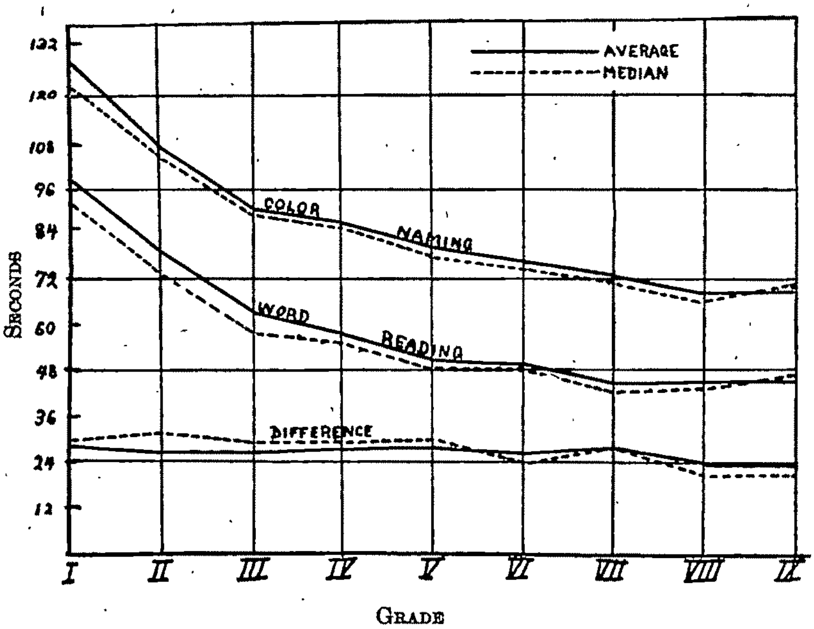

FIG. 1. COLOR NAMING COMPARED WITH WORD READING

It will be observed that in both functions there is a progressive increase in speed throughout the range tested. The improvement in the first few grades is much greater proportionately than in the last grades. This resembles most learning curves and immediately suggests that both functions contain some factor which is practiced in the normal education of the subjects.

The most striking feature of these data, however, is the approximate constancy of the difference between the two functions. The curves of the two functions are almost parallel. The curve of the difference is nearly a straight line, showing almost no decrease from the 1st to 9th grades. This difference between the two functions is the phenomenon which we are studying, and this feature of it will be of the utmost importance in its interpretation.

The probable error of the difference is so small as compared to the difference itself, that there can be no question of its reliability. It will be seen that it is always much more than four times its P.E.

TABLE II

AVERAGE TIME FOR COLOR NAMING AND WORD READING TESTS BY CHRONOLOGICAL AGES

(Ages 17 and 18 yr. not added because of the small number of cases)

Age	No.	Colors	P.E.	Words	P.E.	Diff.	P.E.	Diff./P.E.
6	16	128.8	6.0	103.5	4.7	25.3	6.9	3.7
7	54	128.9	3.8	92.5	3.0	36.3	4.8	7.5
8	78	107.2	1.8	80.6	1.6	26.6	2.4	11.0
9	52	97.0	2.3	73.4	2.7	23.6	3.5	6.7
10	74	89.6	1.5	55.0	1.1	34.6	1.9	18.2
11	78	83.5	1.1	55.4	.8	28.1	1.4	20.1
12	86	79.5	1.1	52.8	1.1	26.7	1.6	16.6
13	72	75.3	1.5	57.0	.9	18.3	1.7	10.8
14	80	74.7	1.2	48.9	1.0	25.8	1.6	16.1
15	37	70.6	1.0	46.5	.8	24.1	1.3	18.6
16	15	71.1	2.2	45.9	1.3	25.2	2.6	9.7

Table II shows the same data, with the *Ss* classified by chronological age. The range of ages included is from 6 to 16 years. There were 3 *Ss* 17 yr. of age and 1 who was 18. These were omitted in this table, both because of the unreliability of so small a group and because of the obvious selected nature of these samples, due to the fact that these *Ss* are obviously much retarded in their school progress and are therefore not representative of their age. The opposite may be true of the 6-yr. old *Ss* also, but since there is a larger group of them, they have been included.

There is very little difference between the data when classified in the two ways. In Table II the differences are slightly more variable. But it will be observed that there is no consistent tendency for the difference to become smaller as the subject grows older. Some of the inconsistency can be explained by the presence of a larger number of boys or girls in the age group. There are sex differences, especially in the case of color naming.

The general results, then, show that older and more advanced *S*s perform both functions faster than the younger ones, but that the superiority of the speed of word reading over color naming is approximately constant despite age or school advancement.

(*2*) *Correlation between functions.* It has been suggested that the color naming function is the same process as the word reading, plus another process. *S*s, introspecting, often insist that this is true. If it is true, however, word reading would never be slower than color naming. There were a number of *S*s in this experiment who did read the colors faster than the words. Also, if the hypothesis be valid, there should be a very high correlation between relative ability on the two tests. Even if the additional process does not correlate highly with the common factor, the correlation should in groups be high and fairly constant.

TABLE III

RELATIONSHIP BETWEEN THE TWO FUNCTIONS

Grade	r	P.E.	Grade.	r	P.E.
I	.709	.047	VI	.506	.060
II	.588	.052	VII	.275	.087
III	.162	.077	VIII	.394	.068
IV	.594	.502	IX	.423	.080
V	.405	.068	Av.	.450	.066

Table III shows the coefficients obtained between the two functions in the several grades. While, in most cases, the relationship indicated by these coefficients is high enough to be reliable, it seems neither high enough, nor constant enough, to substantiate the foregoing hypothesis.[3]

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[3] During the experiment, 17 *Ss* succeeded in naming the colors faster than they read the words. Of these, 5 were in grade I, 5 in grade II, 3 in III, and 1 each in grades IV, VI, VIII, and IX. Such scores might be due either to unusually fast color naming or unusually slow word reading. An analysis of the scores seems to indicate that both conditions were present. Of the 17 *S*s, 12 made color naming scores that excelled the average of their respective grades, and 5 made scores poorer than these averages. For the entire 17, the average deviation from the mean of their grades was −12.1 sec. On the other hand, all but one *S* read words slower than the average of their groups. The average deviation from the mean of their grades was +44.6. Of the 17 *S*s, 5 excelled their groups in color naming by larger amounts than they proved inferior in word reading; the other 12 reversed this condition. These results indicate that both factors then combined to produce these unusual scores, but that inferiority in word reading was the more important of the two.

The correlation between the functions is always positive and usually fairly high, showing that the two functions are certainly not entirely independent. Yet, the variability of the coefficient and the low level to which it drops, also, indicates that there are some factors at least in which the two functions vary independently.

(3) *Relationship of functions to differences.* Since the difference between the functions forms the central problem of this study, correlations were computed between it and each of the functions. The difference was computed by subtracting time for word reading from time for color naming. Those cases in which the word reading was the larger number were treated as negative differences. Table IV shows these results.

TABLE IV

CORRELATIONS BETWEEN FUNCTIONS AND DIFFERENCES

CN refers to color naming; WR to word reading; Diff. to the difference between the two.

Grade	r CN-Diff.	P.E.	r WR-Diff.	P.E.	σ CN/σ WR
I	.500	.055	−.220	.083	1.08
II	.460	.055	−.400	.063	1.00
III	.663	.047	−.599	.051	1.09
IV	.544	.047	−.284	.073	1.14
V	.944	.008	−.122	.079	1.90
VI	.763	.032	−.062	.079	1.73
VII	.855	.025	−.249	.083	1.94
VIII	.746	.038	−.323	.077	1.38
IX	.893	.018	−.222	.091	1.64
Av.	.717		−.274		

Rank Square Difference Correlations

First series	Second series	Rho
r CN-Diff.	r WR-Diff.	−.49
r CN-WR	r CN-Diff.	−.56
r CN-WR	r WR-Diff.	−.33
ratio of σ	r CN-Diff.	+.90
ratio of σ	r WR-Diff.	−.55
σ CN	r CN-WR	+.44
σ WR	r CN-WR	+.47

Let us examine this table carefully, for in it there are many clues leading to the solution of our central problem; namely, the explanation of the difference between the two functions.

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*Professor F. C. Chillrud checked these coefficients by computing them by formula instead of actual plotting as I have done.

If the two functions were entirely independent, color naming being simply a longer process than word reading, certain results could be expected. In the first place, color naming would correlate positively with the difference, since (except for the chance variation in word reading time) the greater the time required for color naming, the greater would be the difference. In the same way, in the second place, word reading should correlate negatively with the difference, since (except for chance variation in color naming time) the longer the time required for word reading, the smaller would be the difference. In each case, the smaller the relative variability of the other, the higher the correlation. All of these conditions are fulfilled in the above data. Color naming correlates positively with the difference. The higher the ratio between the standard deviations of the two functions, the higher the correlation. The rank square difference coefficient between this ratio and the size of the correlation is 0.90. Word reading correlates negatively with the difference, and the corresponding rank square difference score is =0.55.[4]

There is, however, one further condition which should be fulfilled if the two functions are entirely independent; namely, there ought to be no relationship between them. There is, however, a fairly high relationship between them—it averages 0.450 (see Table III).

This would indicate a definite relationship between them. The two functions are not independent. If the assumption is made that they are very highly related, so that excellence in one signifies excellence in the other and vice versa, there ought to be much higher coefficients between the two functions than are found, and word reading ought to correlate positively with the difference; for in this case, the worse the score for word reading, the worse ought color naming time to be proportionately and the wider the difference between them. This condition is not found. This hypothesis cannot be maintained.

It is obvious then that the two functions cannot be regarded as simple entities, since they are both related and unrelated to each other. As will be pointed out in the conclusion, this practically eliminates the 'practice hypothesis.' Let us consider other possibilities.

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[4] It will be observed that the ratio between the σ is computed by dividing the σ for color naming by that of word reading. Hence the greater the ratio, the relatively greater is the σ of color naming to that of word reading.

Suppose the color naming function is made up of two factors, in one of which it is entirely independent of the word reading function and in one of which it is identical. The latter may be referred to as the common factor and the former as the color naming special factor. The difference would then be a part of this color naming special factor. This would account for the high correlation between the color naming function and the difference and, since there is a common factor, between color naming and word reading.

But, why the negative relationship between word reading and the difference? If the common factor and color naming special factor were entirely independent, there ought to be a zero relationship between them. The only answer is that word reading is not entirely constituted of the common factor, but also has a special factor, which though very much smaller and less variable than the color naming special factor, influences the difference, and hence correlates negatively with it.

The common factor, then, causes the two functions to correlate with each other. The special factors cause this relationship to be low. It is easy to see that the common factor constitutes most of the word reading function, since the effect of the word reading special factor has so little influence upon the difference, as indicated by the correlation between word reading and the difference.

It is in the suggesting of this word reading special factor that I have entirely departed from previous hypotheses. It not only permits an explanation of the data just discussed, but it allows for those cases in which a longer time was required for word reading than for color naming.

That the two special factors are entirely independent is indicated, not only by the low correlation between the functions, but by the fact that the more each varies, the higher its correlation with the difference. Obviously, since the common factor would vary equally in the two functions, any difference in the variability of the two must be attributed to the special factors. The ratio between the σ of the two functions, then, is a statement of the variability of the color naming special factor as compared to the word reading special factor. If they are independent, the higher this ratio, the greater the relative variability of the color naming special factor and the higher will color naming correlate with the difference and the lower will word reading do so. The rank square difference coefficient between this ratio and the r of color naming with difference is 0.90; between this ratio and the r of word reading with difference is −0.55.

The question also arises, as to whether the common factor is very variable or not. Any difference in variability between the two functions must be attributed to the color naming special factor. But of the variability of the entire word reading function, is it all in the common factor, in the special factor, or in both?

If all the variability were in the special factors, since they vary independently of each other, the larger this variability the smaller would be the correlation between the two functions. In this case the rank square difference coefficient between the size of the standard deviations and the r between the functions would be negative. In the case of color naming it is 0.439 and for word reading 0.470. This indicates that the greater the variability of the functions the greater the relationship between them. Hence a greater part of the variability must be in the common factor, a very little in the word reading special factor, and the same amount plus the difference in variability in the color naming special factor.

Since, according to this hypothesis, the common factor determines the relationship between the functions, it follows that the higher the correlation between them, the greater the influence of the common factor and the less the influence of the special factors. This would be indicated in their relationship to the difference. Hence the higher the correlation between the two functions, the lower ought to be the correlation between the functions and the difference. The rank square difference coefficients which express this are in the case of color naming −0.37 and for word reading −0.33.

It also follows, that since there is improvement in both functions with age, but with a constant difference, that it is the common factor which improves, whereas the special factors do so very little. This would indicate that the special are organic in their nature, whereas the common factor is a learned reaction.

(*4*) *Intelligence.* It will be valuable for the understanding of the problem, to find out whether either or both functions are related to intelligence. Three measures were secured in this experiment which might be expected to be measures of intelligence. In 2 [redacted] marks for the year were obtained. In 7 of the 9 grades, intelligence test scores were available. In all the grades, except the 9th, there were two or three sections of each grade, made on the basis of school success. We have very little confidence in any of the three sources as accurate indices of intelligence. Group scores should, however, be more reliable than individual and if the same results are obtained from all three sources, that in itself is evidence of reliability.

Class marks were available for the 7th and 8th grades. The correlations obtained with them are as follows: color naming with class marks; 8th grade −0.127, P.E. .036; seventh grade, −0.110, P.E. .094; for word reading with class marks, eighth grade −0.328, P.E. .076; seventh grade 0.099, P.E. .094. It will be remembered, of course, that a negative correlation indicates a positive relationship. We should expect that if the tests are indicators of school success or vice versa that high negative coefficients would be obtained. It is obvious that this is not true. Three of the 4 coefficients are approximately zero.

TABLE V

Correlation between IQ and the Two Functions

Grade	r. IQ-CN	P.E	r. IQ-WR	P.E.
I	−.148	.093	−.420	.080
II	−.049	.087	−.324	.079
III	.125	.094	−.006	.095
IV	−.026	095	−.285	.086
V	.076	122	.146	.120
VII	−.014	095	−.004	.095
VIII	.192	084	−.143	.085
Av.	.021		−.146	

An examination of this table only adds to the conclusions drawn from the class mark correlations There are only three coefficients with any considerable size. It may be noted that both the significant coefficient in connection with school marks and the three significant scores in this table are in the word reading function and none of them in color naming.

There is one further line of evidence in connection with this problem. The average scores for the two functions may be obtained for the sections of each grade. Most of the grades were divided into three sections; three had only two sections; and the ninth grade was not divided at all. It has therefore been omitted from Table VI, which contains these results.

There is more indication of intelligence playing a part in this group comparison than in either of the other methods of comparison. The same tendency appears, however. While there is some tendency in color naming for section A to excel section B, and B to excel C, it is by no means a marked tendency and has several exceptions. In word reading the tendency is much more pronounced. There is no definite trend in the comparison of differences at all.

TABLE VI

Averages for Sections of School Grades

	Color Naming			Word Reading			Differences		
Grade	A	B	C	A	B	C	A	B	C
I	118.7	118.2	133.7	85.3	92.7	106.8	33.3	25.5	26.9
II	103.9	105.6	116.3	72.2	85.0	86.5	31.7	20.6	29.8
III	92.2	89.8	—	64.1	62.6	—	28.1	27.2	—
IV	85.4	88.7	94.9	55.0	61.4	67.0	30.4	27.3	27.9
V	79.3	82.8	—	51.3	52.9	—	28.0	29.0	—
VI	74.1	76.0	79.4	48.6	51.5	53.4	25.5	24.5	26.0
VII	73.8	73.4	—	45.7	42.7	—	28.1	30.7	—
VIII	67.2	73.2	69.6	41.9	47.7	45.9	25.3	25.5	23.7

From these data, the conclusions seem tenable that the common factor is related to intelligence, and that neither of the special factors are appreciably so related. This is indicated by the closer relationship between measures of intelligence with word reading, in which the common factor predominates, than with color naming, where the common factor is a much smaller portion. That the color naming special factor is very little related to intelligence is indicated by the complete absence of significant differences in the case of the different scores. The word reading special factor is likely to be the cause of the relatively low correlation between intelligence and word reading.

(*5*) *Errors.* A record was kept of the number of errors made by the *S*s taking the tests. An analysis of these records shows some interesting features, which will be valuable in the explanation of the phenomenon.

Table VII shows that the percentage of *S*s making errors is larger in color naming than in word reading. It shows that there is comparatively little improvement in the percentage making errors in either function through the nine grades studied. The average number of errors made by those who made errors is very strikingly constant for all grades in color naming. It is more variable for word reading, though showing little signs of improvement. In most cases, the average error for the erring is greater in color naming than in word reading.

For the group, the average error for color naming is always much greater than for word reading, due to the high percentage of *S*s who make errors in color naming.

Since the common factor has been shown to improve throughout the 9 grades, and since there is almost no sign of improvement in eliminating errors, we are almost forced to assume that the errors are found largely in the special factors. The rank square difference correlation between the averages of the errors for those erring in the two functions is 0.30, which is surprisingly small, indicating a very low relationship between the two functions in the matter of erring.

TABLE VII

ERRORS IN THE TWO FUNCTIONS

Grade	Color Naming			Word Reading		
		Av. of			Av. of	
	% erring	erring group	entire group	% erring	erring group	entire group
I	71	2.40	1.71	50	2.65	1.36
II	63	2.76	1.73	31	1.69	.49
III	65	2.63	1.70	29	2.18	.61
IV	59	2.39	1.40	30	1.90	.59
V	55	2.89	1.59	25	2.47	.42
VI	68	2.65	1.81	29	1.68	.49
VII	58	2.40	1.41	15	2.33	.35
VIII	57	2.17	1.25	29	1.78	.52
IX	53	2.14	1.13	31	1.47	.46

It seems especially desirable to establish the last point. The rank square difference coefficient is hardly sufficient for this purpose. Recourse was taken to the individual cases. A table has been prepared, Table VIII, in which the average number of errors made in word reading is computed for those making a given number of errors in color naming. Thus in the first column are found individuals who made no errors in color naming; in the second column the average of those who made only one error in color naming, and so on. The number of cases has been placed at the bottom, so that an estimate of the reliability of the data obtained may be made.

An examination of this table shows that there is a very slight tendency for the errors made in one function to predict what will be made in the other. Even the averages, however, show great inconsistency. Certainly this table affords strong evidence of the high degree of independence of the two functions. Since it may be assumed that most of the errors are due to the special factors, this still further establishes the comparative independence of these. That there is a very slight correlation indicates that there may be some tendency to error in the common factor.

TABLE VIII

RELATIONSHIP BETWEEN ERRORS IN THE TWO FUNCTIONS

		Errors in color naming										
Grade		0	1	2	3	4	5	6	7	8	9	10
I	Errors in word reading	.4	.8	2.0	2.2	.0	2.0	1.0				
II		.2	.3	.4	.7	1.1	2.0	2.0	2.0	.0	2.0	
III		.3	1.2	.6	.4	1.0	.0					
IV		.5	.5	.5	.5	.0	1.0	.0	7.0			
V		.1	.7	.6	.8	.0	.3	.0			.2	
VI		.2	.4	.4	1.1	.4	.8			2.0		2.0
VII		.1	.5	.4	.0	2.0	1.5	.0				.0
VIII		.4	.4	.7	.8	1.0		2.0				
IX		.3	.5	.6	1.0	2.0		.0		2.0		
Av.		.3	.6	.7	.8	.7	1.3	.8	5.5	1.0	1.0	1.0
No.		249	137	107	69	33	23	10	2	4	2	2

TABLE IX

SEX DIFFERENCES IN FUNCTIONS BY SCHOOL GRADE

Bold face type indicates those instances in which the difference is more than four times the P.E. and so statistically reliable

	Color naming				Word reading				Differences			
Grade	Boys	Girls	Diff.	P.E.	Boys	Girls	Diff.	P.E.	Boys	Girls	Diff.	P.E.
I	139.7	121.0	**18.7**	**3.1**	113.1	91.7	**21.4**	**2.9**	26.6	29.3	−2.7	4.2
II	108.5	107.4	1.1	1.7	83.1	76.7	6.4	1.7	25.4	30.7	−5.3	2.4
III	93.4	86.9	**7.5**	**1.6**	60.3	66.8	**−6.5**	**1.5**	33.1	20.1	**13.0**	**2.2**
IV	89.8	86.8	3.0	1.2	59.9	59.3	.6	1.0	29.9	27.5	2.4	1.6
V	85.5	76.9	**9.6**	**1.2**	52.0	52.3	−.3	.6	33.5	24.6	**8.9**	**1.3**
VI	84.3	72.4	**11.9**	**1.3**	51.7	49.8	1.9	.7	32.6	22.6	**10.0**	**1.5**
VII	78.4	68.9	**9.5**	**1.1**	43.8	44.8	−1.0	.5	30.5	24.1	**6.4**	**1.2**
VIII	72.5	64.3	**8.2**	**1.6**	45.8	44.9	.9	.7	26.7	19.4	**7.3**	**1.7**
IX	71.8	67.2	**4.6**	**1.1**	48.4	46.6	1.8	.6	23.4	20.6	2.8	1.3

(6) *Sex differences.* Previous investigators have found girls superior to boys in these functions. No extensive set of data has, however, been available for studying these differences.

Table IX indicates that there are very much greater sex differences in color naming than in word reading. The girls excelled the boys in color naming in every grade, with statistically reliable superiority in 7 of the 9 grades. In word reading, on the other hand, there were 3 grades in which the boys excelled, and in only one did the girls have a reliable difference in their favor.

The logical deduction from this would be that girls are superior to the boys in respect to the color naming special factor, not in the common factor, with the word reading special factor in doubt. In a comparison of the differences, which constitute the major portion of the color naming special factor, we find the girls with a decided advantage except in the first two grades. There is evidence that the greater the influence of the color naming special factor, the greater the superiority of girls; and the greater the influence of the word reading special factor, the greater the superiority of boys.

TABLE X

SEXES COMPARED IN MAKING ERRORS

(Italics indicate sex making best score when difference is not reliable; bold face indicates sex making best score when difference is reliable)

	Color Naming						Word Reading					
	% erring		Av. erring		Av. group		% erring		Av. erring		Av. group	
Grade	B	G	B	G	B	G	B	G	B	G	B	G
I	*61*	63	*2.3*	2.6	*1.4*	1.6	47	*43*	3.3	*2.0*	1.6	*.9*
II	63	**40**	**2.7**	3.1	1.7	**1.2**	40	**26**	1.5	1.6	.6	*.4*
III	**49**	63	*2.1*	2.4	**1.0**	1.5	*23*	27	*2.0*	2.3	*.5*	.6
IV	63	**47**	*2.4*	2.5	1.5	*1.2*	38	15	**1.7**	2.6	.7	*.4*
V	62	**45**	**2.8**	3.4	1.8	*1.5*	24	*23*	1.7	1.7	.4	*.4*
VI	63	*61*	2.8	*2.4*	1.7	*1.5*	31	**24**	*1.6*	1.8	.5	.4
VII	64	**50**	2.8	**2.0**	1.8	**1.0**	*14*	16	**2.0**	2.6	*.3*	.4
VIII	59	**50**	2.5	**1.7**	1.5	**.9**	29	*27*	1.8	*1.8*	.5	*.5*
IX	57	**52**	2.3	*2.0*	1.3	*1.0*	*29*	32	*1.4*	1.5	*.4*	.5

Table X shows that the girls excel in both functions in the percentage of the availing errors. This is especially true in respect to color naming. In regard to the average errors made by those making errors, the boys excelled in the first 5 grades and the girls in the last 4, in color naming. The differences in word reading were variable and negligible. The average errors for the groups give the girls an advantage, especially in color naming. This shows that it is in the number of those making errors rather than in errors made in which the girls excel. We may conclude, then, that girls show a reliable superiority to boys in the color naming special factor. This advantage seems to lie, not in a general superiority of all girls over all boys, but in the greater percentage of girls who make a minimum of errors. The superiority of boys over girls in the first 5 grades in average error made by the erring and the reverse in the last 4, might seem to be an interesting phenomenon in need of interpretation. The differences, however, are not great at any time. Carelessness may be found more in girls and older boys. This may account for the fact.

It was observed that girls tended to be more advanced in school for their ages than did boys. This made it desirable to study these sex differences by ages as well as by grades. It might be possible that the superiority of girls over boys is entirely due to their age. Or on the other hand, if given equal age, the boys may not be inferior to the girls in color naming. Table XI shows the differences when computed on the basis of age, and its reliability.

TABLE XI

SEX DIFFERENCES BY CHRONOLOGICAL AGES

(Bold face type indicates a reliable difference)

Age	Av. grade			Color Naming			Word Reading			D-D
	B	G	Diff.	Diff.	P.E.	D/P.E.$_D$	Diff.	P.E.	D/P.E.$_D$	
6	1.00	1.08	.08	15.0	15.1	1.0	22.1	15.0	1.5	−7.1
7	1.32	1.44	.12	28.4	8.8	3.2	24.6	7.7	3.2	3.9
8	1.85	2.08	.23	12.6	3.5	3.6	13.6	3.4	**4.0**	−0.9
9	2.68	3.13	.45	7.6	4.7	1.6	0.1	5.3	0.0	7.6
10	3.74	3.96	.22	10.9	3.2	3.4	1.5	2.3	0.6	13.4
11	4.46	4.70	.24	11.1	2.1	**5.3**	−0.9	1.8	0.5	12.0
12	5.58	5.81	.23	11.2	2.0	**5.6**	3.9	2.3	1.7	17.3
13	6.50	6.78	.28	3.6	2.9	1.3	1.3	1.7	0.8	1.4
14	7.03	7.36	.33	10.3	2.4	**4.3**	0.3	1.9	0.2	9.9
15	8.59	8.40	−.19	13.6	2.0	**6.8**	3.8	1.8	2.1	9.9
16	8.73	8.75	.02	6.2	3.5	1.8	5.9	3.3	1.8	0.3

When this table is compared with Table IX, the general trends are found. The girls excel still more in color naming. The girls show a little more advantage in word reading than in the other classification. This would indicate that the color naming special factor is a product of age and organic, since when girls are given an equal advantage in age, their superiority is greater. On the other hand, the common factor seems to be in part at least subject to the influence of training, since girls show to better advantage when classified by age, where they have the advantage in school advancement.

A study of sex differences, then, shows the girls to be superior in the color naming special factor, regardless of classification. The common factor seems to be subject to school advancement, as is indicated when the subjects are classified both by grades and by ages.

Discussion. The first hypothesis intended to explain the phenomenon was suggested by Cattell.[5] He states the so-called 'practice hypothesis.' "The association between the color word and its name has been repeated so often that it has become automatic, whereas in the case of colors a voluntary effort is required."[6] When the data were secured from adult *S*s only, this hypothesis was difficult to refute. The only effort was made by Brown who had his *S*s practice for long periods of time.[7] On the basis of his results, he decided against the practice hypothesis, although as Lund points out, his data can be interpreted in either way.[8]

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[5] J. McK. Cattell, The time it takes to see and name objects, *Mind*, 11, 1886, 63-65.

[6] *Ibid.*, 65.

[7] Warner Brown, Practice in associating color-names with colors, *Psychol. Rev.*, 22, 1915, 45-55.

[8] F. H. Lund, The rôle of practice in the speed of association, *J. Exper. Psychol.*, 10, 1927, 424-433.

The final test of the practice hypothesis could only be made by a genetic study. This Lund did. If the difference between color naming and word reading is entirely due to practice, children before the school age ought to read the colors faster than the words. But when reading habits are acquired, since words are used so much oftener than color names, we would expect the two curves to cross. This is exactly what Lund concludes upon the basis of his experiment.[9] Starting with 5-year-old children, he tested *S*s up to 19 yr. of age. Unfortunately he does not show a table, but presents only a graph, showing the curves of the two functions. I have endeavored to construct a difference curve from his graph, represented by the solid line in Fig. 2.

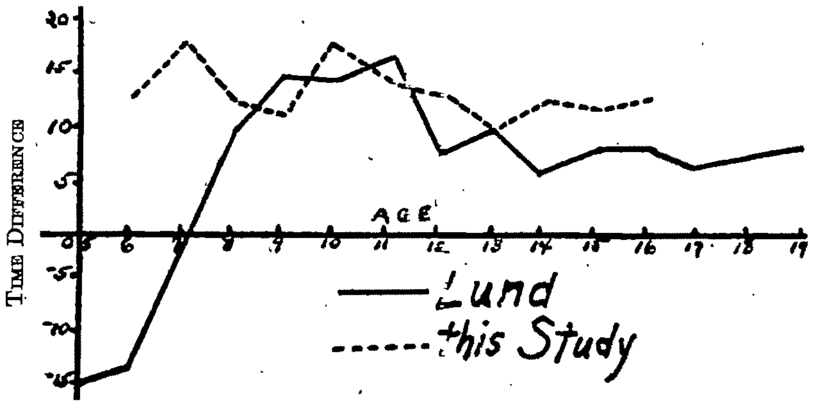

Fig. 2. Comparison of the 'Difference Curves' Obtained from Lund and and from This Study

(The 'Difference Curve' is obtained by subtracting the time required for word reading from that required for color naming)

He used 50 colors, whereas I used 100. I have, therefore, divided my differences in half. It is well known that the first half of the color naming test is faster than the second half, so that my corrected differences should be slightly greater than his. It is apparent that except for 6- and 7-yr. old *S*s, we have obtained approximately the same results. At the 6-yr. old level, I used *S*s already in the first grade. I do not know what type of *S*s he used. But at the 7-yr. old level, where his would also be in the public school, I cannot account for our very wide disagreement. He used, however, only 6 or 7 *S*s at each level. This would give much greater reliability to my results.

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[9] *Ibid.*, 433.

It should be noted, however, that after the 8-yr. old level, his differences are almost constant, showing no gradual increase as would be expected on the basis of his hypothesis.

The 'practice hypothesis' assumes the independence of the two functions. The correlations found between them at the various grade levels averaged 0.450. These two facts seem to me to be quite complete proof that the 'practice hypothesis' is not tenable.

There is no doubt that both functions can be improved by practice. But it is the common factor which is practiced and not the special factors. The practice hypothesis cannot explain the difference between the functions.

Brown considered the possibility of the color naming function including the word reading function plus another factor. and found that this was untenable.[10] The data of my experiment also indicate that such an hypothesis cannot be supported. The fact that some *S*s read colors faster than words is one such bit of evidence.

Hollingworth suggests that there is a time required for articulation, plus, in the case of color naming, an association time.[11] This is an hypothesis, which like the preceding one, is refuted with the same facts. In discussing articulation he speaks of the mispronunciation of the words in color naming, such as "bleen," "rellow," and the like. But he uses the word reading test as an articulation test, in which no such errors occur. He seems to neglect the necessity of an association time in the word reading test, and assumes that the association time for color naming is sufficient explanation of the difference. This is refuted by the constancy of the difference. If association were the only difference, time and practice should narrow this difference. Such is not the case.

In the study made by Gates, it is significant to note that the test showing the highest relationship with color naming was tapping, a much more structural function than the other tests used, adding, word building and multiplying tests.[12] This adds evidence to my contention that the color naming special factor is structural.

Garrett and Lemmon apparently abandon the 'practice hypothesis' entirely.[13] They assert that the difference is due to an interference factor. This factor is based on the introspective evidence of their *S*s. They measure it by obtaining a ratio between the color naming test and a color finding test. The latter consists of asking the *S* to pick out all the squares of the same color at once. They assert that the *S*s can recognize the colors fast enough, as is shown by their speed in color finding, but are prevented by the interference factor, from naming them. The question that arises is, why does this interference factor not operate in the word reading test? This they do not consider. If *S* can recognize the color as fast as the word, why can he not pronounce it as fast? To be sure, there is an interference factor, but it is in the color naming test and has nothing to do with the articulation process in pronunciation.

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[10] *Op. cit.*, 45 ff.

[11] H. L. Hollingworth, Articulation and association, *J. Educ. Psychol.*, 6, 1915, 99-105.

[12] G. S. Gates, Individual differences as affected by practice, *Arch. Psychol.*, 8, 1922, (no. 58), 1-74.

[13] H. E. Garrett and V. W. Lemmon, An analysis of several well-known tests, *J. Appl. Psychol.*, 8, 1924, 424-438.

Since none of these hypotheses are supported by the data of this investigation, it seems desirable that a new hypothesis be formulated, which will explain these data. Such an hypothesis can only be tentative, awaiting further experimentation for its testing. Three experiments are now under way in the Union Laboratory, which will have very direct bearing upon this problem when they are completed.

As I have already indicated, the three-factor hypothesis seems tenable in view of these data. The question that now arises is what are these factors? What is the common factor? What are each of the two special factors? Before answering, even tentatively, it must be stated that the common factor may be and probably is a group of factors, which are common to both functions. The same may be said of the special factors.

Perhaps an analysis of each function may help. In color naming, the *S* must recognize the color, must recall its name, and must articulate its name. In word reading, the *S* must recognize the word, must recall its pronunciation, and must articulate it. I recognize that this analysis is entirely logical and may not be an actual description of the psychological process at all.

In looking for the common factor, there seems little doubt that the articulation time for both is common. Whether this is sufficiently influenced by practice to explain the preceding data is doubtful. The recognition process may be so similar that it will be common except for the difference in reaction time to the colors as contrasted with the words. A reaction time experiment, which is now being done, may give evidence that will help answer this question.

The recognition process and the articulation process are both evidently necessary to either function. But what happens between the two seems likely to form the basis for the special factors. It seems logical to assume that it requires more time to translate a recognized color into the spoken color name, than to translate a recognized word into a spoken word. That this should make color naming slower than word reading is a very reasonable assumption. But why the difference between the translation time for the two functions should have an almost constant value irrespective of age and educational advancement is difficult to see.

This seems as far as it is possible to go toward an hypothesis to explain the phenomenon, until further data are secured from the experiments now in progress.

SUMMARY AND CONCLUSION

The Woodworth-Wells color naming and a word reading test, consisting of a sheet on which the color names as found in the color naming test were listed in the same order, was given to 638 *S*s ranging from the 1st to 9th grades in school and from 6 to 18 years of age.

(1) There was a progressive improvement in the performance of both functions throughout the range of those tested.

(2) The differences between the two functions remained remarkably constant throughout the 9 grades. Color naming was consistently poorer than word reading. Only 17 *S*s gave opposite results.

(3) There was a fairly high correlation between the two functions. The relationship between color naming and the difference between the functions was positive and high, and that between the difference and word reading was negative and relatively low.

(4) The assumption that there was a common factor for the two functions and special factors peculiar to each was defended statistically. If this assumption is true, there was a very low correlation between the two special factors and between each special factor and the common factor.

(5) An analysis of the errors indicated that they were in the special factors more than in the common factor.

(6) Measures of intelligence correlated low with the two functions. There was evidence of a higher correlation with the common factor than with the special factors.

(7) The low relationship between tendency to err in the two functions added evidence of the independence of the special factors.

(8) Girls excelled the boys consistently in color naming. No great differences were found in word reading.

While previously suggested hypotheses for the ability to read the words faster than the colors have been shown untenable in the light of the data of this study, no new hypothesis beyond the three-factor theory could be offered. Unquestionably recognition, mental translation of the stimulus into the response, and articulation are factors likely to be found among the three factors.

UNSYNTHETIC MODES OF THINKING AMONG ADULTS: A DISCUSSION OF PIAGET'S CONCEPTS

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The difference between the modes of 'logical' or coherent thinking in the child and in the adult, as revealed through the medium of verbal exchange, is generally conceded to be characterized by inability to analyze and discriminate, to use integrated symbols, to form noetic syntheses. According to Piaget,[1] this defective synthesis on the part of the child manifests itself not only by way of too little synthesis, by inadequate expressing of relations of cause and effect, by irrelevant sequences in the train of thought, but also by exaggerated synthesis, by the tendency to bind everything together and to justify by ingenious devices. These two 'prelogical structures,' lack of synthesis or juxtaposition, and excessive synthesis or syncretism,[2] are due not merely to lack of maturity on the part of the child, to insufficient scholastic training in the use of grammatical forms and symbols, to inexperience in the world around him, but also and primarily, as Piaget believes, to the fact that the child is not entirely free from the 'egocentrism' that characterizes all autistic thinking, dreaming and day-dreaming, as opposed to 'logical' and synthetic thinking.[3] Because of this self-centered attitude, the child employs in his verbal communications juxtaposed and syncretic modes of expression, since he is under no compulsion to understand or make himself understood. It is only through the interplay of greater maturation and the exigencies of the world around him that he is shaken out of his egocentric point of view into a more socialized and objective one.

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[1] J. Piaget, *Le jugement et le raisonnement chez l'enfant* (referred to hereafter as *JR*), 1924, 73-81.

[2] Juxtapositions and syncretisms are found not only at the level of verbal intelligence or of logical thought, but at the perceptual level, the level of sensori-motor understanding, J R, 77.

[3] Piaget places the 'logical' thinking of the child intermediate between the autistic thinking of child and adult, and adult 'logical' thinking. In fact, he shows that syncretism is a link between the Freudian mechanisms of condensation and displacement and the generalizations and abstractions of synthetic thought. *Le langage et la pensée chez l'enfant* (hereafter referred to as *LP*), 1923, 207.

Now Piaget recognizes the fact that the adult is not entirely free from 'egocentrism' when he thinks logically, so that juxtapositions and syncretisms may persist in his verbal exchanges, particularly in novel or complex situations, or under the stress and strain of emotion (bad humor or passion), which reduce him to the level of infantile or semi-autistic psychological functioning.[4] But from his observations of and experiments on children at the Institut J. J. Rousseau, Piaget was struck by the apparent frequency with which these unsynthetic verbal expressions occurred among children; predominantly at the younger age levels while disappearing at the older ones.

Unfortunately, Piaget did not make use of his technique of investigation[5] in analogous forms on adults or on other groups of children before drawing his conclusions.[6] Isaacs, at the Malting House School, working with a group of young children superior intellectually and in cultural background to the children at Geneva, employed Piaget's method of recording conversation, questions, and explanations. She found some instances of egocentric or syncretic logic, but no hard and fast "mental structures" of first, egocentrism, then socialized logic; but rather a "continuous advance in the scope and clarity of noetic synthesis." Isaacs believes as does Piaget that the adult may also be egocentric, particularly in affective situations, unfamiliar issues, when taken unawares, or influenced by religious prejudice or social tradition.[7]

In a recent article,[8] Piaget has replied to Isaacs, admitting that the age levels where childish structures of thought disappear are certainly dependent on the intellectual capacity of the child, as well as on his cultural heritage; and furthermore, he adds, it is perfectly possible for juxtapositions and syncretisms in different fields of interest, of conceptual knowledge, or at different levels of 'consciousness' (sensori-motor or verbo-intellectual), to disappear by means of a stagger system.[9]

One investigator, Huang at Smith College, had the advantage of placing both children and adults in an identical experimental setting.[10] He employed one of Piaget's methods, that of asking the *Ss* for the causal explanation of strange phenomena (conjurer's tricks, optical illusions, a less familiar physical force opposed to a better known one) that were presented to them. His *Ss* consisted of a group of young children from professional families and a small number of college students. He was interested primarily in getting at attitudes and concepts (magical or natural), but he also discussed the question of logical thinking. He contended that the adult was more capable of synthesis than was the child, but that in the case of both there was distortion of the material, "modifications, supplementations and falsifications of the data of observation and memory because of the subjective character of the explanations."[11] Huang did not discuss how far these distortions were in the nature of syncretisms, but he did say that he found both the children and the college students capable of explaining cause and effect as far as their knowledge and experience allowed, and of not giving mere juxtaposition of ideas not implying active causation.

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[4] *LP*, 131; *JR*, 79.

[5] Piaget employed three methods: (1) the recording and analyzing of spontaneous conversations of children; (2) the recording and analyzing of children's answers to adult questions; and (3) the giving of tests and tasks to be performed.

[6] In justice to Piaget, it should be said that he is not blind to this defect.

[7] S. Isaacs, *Intellectual Growth in Young Children*, 1930, 92-93.

[8] Le développement intellectuel chez les jeunes enfants, *Mind*, 40, 1931, 137-160.

[9] In his books, Piaget certainly gives the impression that he accepts definite age levels; *i.e.* juxtapositions and syncretisms of perception disappearing at the age of 7-8 yr., and of verbal intelligence at 11-12 yr.

[10] I. Huang, Children's explanations of strange phenomena, *Smith Coll. Studies in Psychol.*, 1930, No. 1.

The preceding discussion suggests that the presence or absence of syncretic and juxtaposed verbal expressions is not entirely a matter of whether child or adult is doing the understanding and thinking, but dependent rather on the type of individual (child or adult), the extent of development of his psychological functions (*i.e.* intelligence), his past experiences, his organic set-up at the time of being observed, and his cultural heritage. Therefore any results obtained will vary with the nature, the difficulty, and the complexity of the material handled by the individual.

For this reason, we decided to repeat some of Piaget's work on a group of adults, not as Huang had done by presenting identical material to both children and adults, but by offering an experimental setting which would be considered difficult for the *S*s in question. This method would exclude any direct comparison with children, but would reveal whether and to what degree an adult resorts to the use of juxtapositions and syncretisms in his language communications, in a particular setting where presumably he is not day-dreaming or thrown off his guard by any appreciable emotional attitude.

METHOD AND PROCEDURE

One of Piaget's procedures, which he believed to be successful, in getting at the understanding and thinking of children, particularly in bringing out differences in the use of syncretisms and juxtapositions at two different age levels (6–7 and 7–8 yr.), was that of having stories repeated from one *S* to another.[12] *E* told a child he would read him a story to see how well he could tell it to another child. The first child, the 'explainer,' was sometimes asked to repeat his story to *E* before proceeding to another room to tell it to the second child, the 'reproducer.' At other times the 'explainer' was immediately asked to repeat the story to the 'reproducer' in the presence of *E*. The 'reproducer' was then asked to give his version of the story to *E*, who next questioned each child separately as to the different items in the story, not for detail so much as for comprehension of the subject-matter.[13] A complete record of the language of the story-telling and of the replies to questions was kept. By his technique of questioning, Piaget believed he could find how much more both the 'explainer' and the 'reproducer' understood than they had expressed before the prompting of *E*, and in this way the test became something more than a test of memory.[14]

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[11] *Op. cit.*, 176. [12] *LP*, Chap. 3.

[13] The 'reproducer' was questioned only on the material presented to him by the 'explainer.'

Our task was to find material fairly hard for and somewhat longer than the immediate verbal memory span of the adults on whom we intended to experiment (college students). We chose selections from articles in the *Scientific Monthly*, on topics with which the students might be vaguely familiar, but not being specialists would find fairly difficult. We used two selections, which we shall designate as A and B. A was on the subject of the effects of climate and was 186 words in length. B was on the subject of the development of astronomy and was 187 words long. Both selections contained 13 essential items to be remembered and understood.[15]

As *Ss* we had 42 women students, freshmen and sophomores at Sarah Lawrence College, whose ages ranged from 17 to 21 yr., with the median at 19. In Series A, 21 *Ss* acted as 'explainers' (X) and the other 21 as 'reproducers' (Y). In Series B, 20 of the Y *Ss* of Series A acted as X and 20 of the X *Ss* of

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[14] The stories ranged from 87 to 147 words in length and contained from 9 to 14 items to be remembered or understood, *LP*, 116. Piaget employed explanations of mechanical devices as well as these stories, but since the former involved gesturing as well as verbalizing, we decided not to introduce a factor of which we could not have a record.

[15] The two selections with divisions into items are as follows: Series A. Man's climatic environment affects him in many ways: / His clothing, dwellings, food, occupations and customs; his physical and mental characteristics; his system of government; his migrations; his history—all are affected to a greater or less degree. / The atmosphere is as essentially unalterable as it is all-pervading./ Montesquieu declared that the most enduring of all empires is the empire of climate. Its dominion is invincible. Its laws must be obeyed. It makes no compromise and it grants no pardon. / We may irrigate a portion of the earth's surface and thus remove it partially from the dominion of its local climate, / but we maintain this change only by unending effort. / If left to itself, the desert will encroach upon the irrigated area. / Ancient cities which have been buried in the sands of the desert are evidences of the dominion of climate, / rather than of the destruction accomplished by man. / Man often thinks that he is conquering nature / when he really merely discovers her laws and conforms to them. / We have learned to adjust ourselves to some extent to our climates, / but we cannot change those climates. / (186 words).

Series B. The ultimate object of pure astronomical investigation is to determine the form, motions and physical constitution of our universe, in the broadest sense. / Even its limits are unknown / and no serious scientist would be so bold as to attempt to set them, / any more than he would to the ultimate form which man himself will acquire under the influence of evolution. / Evolution plays its part in the acquisition of knowledge of our universe / as truly as in biology, although of course not in the same sense. / The ancients first occupied their awakening scientific intelligences with the geographical features of their immediate neighborhood / and very naturally concluded that the earth was flat, / which conclusion brought a large crop of difficulties / when they tried to explain the motions of the sun and moon. / When they became more observant and took thought of the apparent forms of the sun and moon and of drops of water, / they were led to the true explanation of a spherical form for the earth as well as for the sun and the moon. / With the invention of the telescope a new era was opened. / (187 words).

Series A acted as Y. The two groups of *Ss* (AX-BY and AY-BX) were equivalent in the scores they made on the Thorndike Intelligence Examination for High School Graduates.[16]

Our procedure was slightly different from that of Piaget, as we were desirous of having graphic records of the repetitions, and yet to avoid oral recitations in the presence of *E*, which, we believe, produced an unfavorable setting. The Xs and Ys, therefore, were each asked to make a written record of what they understood and remembered. The directions to X were:

> This is an experiment to see how well you can repeat to another student what you have remembered and understood of a passage that is read to you. Listen carefully, and when I have finished reading you are to write down what you remember. When you have finished writing you are to go into the next room and read what you have written to your partner.

These directions were given by *E* to Y in modified form to meet the requirements of her task. While Y was writing, X was questioned by *E*. At the end of the experiment Y was in turn questioned by *E* (not in the presence of X).[17]

The experiment was carried on during the spring term of 1931.

RESULTS

We obtained the following numerical results:

	Series A	Series B
Av. no. of items repeated by X to Y..........	7.471 ± 1.161 (57%)	6.60 ± 1.649 (51%)
Av. no. of items understood by X as a result of questioning..............................	8.524 ± 1.011 (66%)	7.40 ± 1.690 (57%)
Av. no. of items reproduced by Y.............	4.235 ± 1.08 (57%)	3.70 ± 1.068[18] (55%)
Av. no. of items understood by Y as a result of questioning..............................	4.667 ± 1.057 (62%)	3.90 ± 1.315[19] (59%)

These figures give an estimate of the difficulty of the material for the group of *Ss* in question. They indicate that Y recalled and comprehended the same relative amount of material as did X in each of the two series, but there is a suggestion that Series B was somewhat more difficult than Series A for both X and Y,[20] a fact to which we shall turn again when we discuss the frequency of the distortions of the materials. The obvious fact that *S*s would recall more after the added stimulus of questioning was also revealed. This was more true of X than of Y, however.

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[16] The average score for the 42 students was 65.4 ± 9.1, range 44 to 80; for the group AX-BY it was 66.8 ± 5.6, range 45 to 80; and for the group BX-AY it was 65.6 ± 6.1, range 44 to 77.

[17] There is no guarantee, of course, that our method of questioning was like that of Piaget. We tried to do as he had done, to ask questions about the memory for and comprehension of the various items, not in a stereotyped manner but quite freely.

[18] The percentages for Y are the proportion of the Y averages to the X averages, not to the total of 13.

[19] We correlated the scores for AX, BX, AY, and BY with the Thorndike scores, and the scores of AX with BY and of AY with BX, by the method of rank differences, translating the ρ obtained into r coefficients by Pearson's correction. The results were low but positive, ranging from +0.25 to +0.41, but with large probable errors (0.14 to 0.15), which was to be expected from the small number of cases.

From his averages, Piaget set up four ratios which he considered indicative of the various degrees of comprehension: γ, the number of points comprehended by X divided by the number of points or items to be comprehended; δ, the number of points X repeated to Y divided by the number of points comprehended by X; α, the number of points comprehended by Y divided by the number of points comprehended by X; and β, the number of points comprehended by Y divided by the number of points X repeated to Y. Table I gives these ratios for Piaget's two chronological age levels and for our two series.

TABLE I

RATIOS SHOWING RELATIVE DEGREES OF COMPREHENSION FOR X AND Y

	α	β	γ	δ
Piaget 6-7	.48	.61	.70	.87
Piaget 7-8	.58	.64	.82	.95
Series A	.55	.62	.66	.88
Series B	.53	.59	.57	.89

Except in the case of γ, our ratios for both series seem to be similar to those of Piaget for the 7-8 yr. level, in fact, they are slightly lower. Our γ ratios are decidedly lower, showing that the texts we chose were relatively more difficult to repeat and to comprehend.

Piaget did not claim from his high δ ratios, however, that children understand adults better than they understand each other, for he recognized the fact that the story as originally told was an adult product prepared ahead in a coherent manner. He limited himself, therefore, to the conclusion that children do not understand each other any better than they understand the adult. We can conclude in a similar manner that our *S*s did not understand each other any better than they understood a logical set-up presented to them.

According to Piaget, the egocentricity of the child is revealed by the fact that he comprehends more than he repeats.[21] Now, in view of the fact that the δ ratios are high (very markedly so at the 7-8 yr. where it is higher than in either of our series), it would seem that this deduction is not entirely justified, and that the children communicated most of the material they understood and remembered.[22] In this respect, the children showed no more egocentrism than did the students, so that we should consider the following conclusion more adequate:

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[20] We did not treat the results by reliability formulae, since there were fewer than 25 cases in each of the four groups.

[21] Only in the case of X, Piaget did not give the ratio of repetition to understanding for Y. In our experiment, this ratio was 0.95 for Series A, and 0.91 for Series B.

[22] These ratios were lower in the case of the experiment requiring explanations of mechanical devices. But here the set-up was different; the explanations were made to the children in a manner not so suggestive of a verbal memory test.

depending on the nature and difficulty of the material, the individual shows a limitation of comprehension and of verbal memory even after receiving the added stimulus of suggestive questioning.

In analyzing the records in this particular experiment, Piaget made use of four criteria of egocentricity (as he had previously done with the records of conversations among children); errors of style and grammar, attitude of satisfaction, the presence of juxtapositions and, lastly, the presence of syncretisms; the last two criteria being the ones with which we were chiefly concerned in this investigation.[23]

Juxtapositions at the level of understanding and thinking are recognized,[24] according to Piaget, by the absence of order in the recitation, by causal connection being not expressed but indicated by a simple juxtaposition of terms, by vague explanations in which the why of explanation is omitted, and by the reversal of cause and effect (independent of memory limitations), due to the fact that for the child 'because' does not indicate a univocal link from cause to effect, but rather a vaguer, indifferentiated link (a link of juxtaposition). In the case of the stories, the older children were far less prone than the younger to employ these unsynthesized or 'prelogical' verbal expressions.

Among our *S*s we found 8 instances (4 made by X *S*s in each of the two series) of reversals of order in the context.[25] These juxtapositions were not due to momentary oversight, forgetfulness, or "slips of the pen," for, as a result of questioning, the *S*s still stuck to the reversed order which meant that their original comprehension had been incorrect. For instance, in AX one *S* stated: "Because of the ravages of war rather than the effects of climate, ancient cities are buried in the sands of the desert." The original text read: "Ancient cities which have been buried in the sands of the desert are evidences of the dominion of climate rather than of the destruction accomplished by man." This student did not comprehend the article as a synthetic whole, the main thesis being that the effects of climate are the most powerful factors of destruction in the world. She undoubtedly made use, in interpreting the material, of her past experience about the evil effects of war.

We found only 3 cases (1 each in AX, BX and in AY) where there was juxaposition of statements without connecting link, thus producing discrepancies of logic, and which, upon questioning, the students neither remembered nor comprehended. We give one example, (from AY): "Man has tried to cultivate the land and with the aid of irrigation has succeeded. Man cannot control the climatic conditions that surround him."

There were no instances of absence of order in the recital (at least sufficient to destroy the proper sequence),[26] nor of the wrong use of 'because.'

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[23] The children had had no formal scholastic training, but in the case of the students there could be no way of telling how far correct use of language and grammar was due to a socialized attitude independent of formal training. Although we made no point of investigating the attitude of satisfaction, we heard frequent comments from the students suggesting dissatisfaction with what they had written.

[24] See footnote 2.

[25] Piaget did not give figures for the frequency of juxtapositions and syncretisms.

[26] The students were evidently 'socialized' in the matter of 'order', for they frequently inquired as to its necessity in the written task.

Relatively few of our *Ss* (6 in Series A and 5 in Series B) used juxtaposed verbal expressions, but in view of the fact that, despite scholastic training, greater maturity, and more experience in 'socialization,' juxtapositions did creep in, there is still the possibility of assuming that their frequency might be augmented by a further increase in the complexity and difficulty of the presented material.

The reverse phenomenon, that of syncretism, the result of incorporating two or more separate items into an all-inclusive whole, so that the original significance is lost, occurred with greater frequency: 2 times in AX; 4, in AY; 17, in BX (13 different *Ss*, 4 having 'syncretized' twice), and 8 times in BY. The students distorted the material far more frequently in Series B than in Series A, and more particularly in BX than in BY. Evidently, the greater difficulty of this series had an effect on the ability to synthesize correctly.

An instance of a high condensation, where the first six items of the text were misconstrued, can be seen in the record of one *S* in BX: "The ultimate astronomical results by astronomers today can be no more ultimate in prediction than what may be the result of evolution in the future, evolution in the world and customs as well as biologically speaking."[27]

From some of the 'syncretic' reports could be traced the set of the individual, her 'egocentric' interpretation. In AY, one *S* was impressed by the statement "man only discovers the laws of nature," and having decided that the theme of the text was that of "man's finding out or discovering," she wrote: "In irrigating a desert we are merely finding out the possibilities of nature." And again she stated: "Just as if we were to find a hidden city buried the thing would have been there always but we are just finding it out." Her X partner had not made this error, but had kept to the original text.

Further 'egocentric' trends are revealed by inventions of subject-matter not given in the original text. These "pure fabrications" did not occur at the 7-8 yr. level, but were more frequently at the younger level, so that Piaget concluded that imagination of this sort was an autistic tendency that disappeared very young.[28] Contrarily, we had a few cases, 3 in A and 3 in B.[29]

If we add together the total number of errors of commission, the juxtapositions, the syncretisms and the inventions, for each of the series, we obtain a better picture of the greater deformation of Series B. In Series A there were 17 distortions and in Series B there were 33.

DISCUSSION AND CONCLUSION

We do not mean to imply that there is anything new or surprising in the fact that material is distorted and transformed when it receives individual interpretation, nor that with greater complexity of material these distortions will occur oftener. The results of myriads of tests of memory, of intelligence, of the reliability of testimony bear witness to this fact. But we have shown that in carrying out an experiment on the memory and understanding of adult students, in a manner similar to Piaget's test procedure, both of administration and treatment, our results approximated his. The amount repeated and understood by 'explainers' and 'reproducers' alike was equivalent to Piaget's 7-8 yr. level, except in the case of the amount of the original text retained by the 'explainers' (γ ratio), suggesting the fact that adults do not understand each other relatively better, nor do they impart relatively more information, than do children in an experiment of this kind.

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[27] See footnote 15.

[28] Piaget meant, of course, "under the conditions of his experiment."

[29] For example, one *S* in AY wrote, "Man believes he can command large industries," and another in AX wrote, "Man may be stranded on a desert with the cool wind blowing across his face."

Furthermore, the adult students as well as the children showed insufficient and exaggerated synthesis of thought by employing verbal expressions of juxtaposition and syncretism, whose frequency varied inversely with increase of age in the case of Piaget, and, in our case, directly with the complexity of the material.

As we have stated previously, Piaget attributes limitation of understanding, of ability to communicate one's thoughts, and the use of distortions of logic to 'egocentrism,' to the incapacity for self-reflection.[30] The breakdown of this egocentric or autistic trend comes about through experience in the social world and by scholastic training as the individual matures, so that in the adult we find considerably more 'socialization,' self-reflection or what Washburn calls "ejective consciousness."

Now, assuming the students to be fairly well 'socialized,' to have developed 'awareness' of their own thoughts and of the thoughts of others, nevertheless, under the conditions of our experiment (where they were presumably free from upsetting conditions and in fairly good 'rapport'),[31] this degree of sophistication did not prevent limited understanding and ability to communicate, or reversion to 'prelogical' or infantile modes of verbal expression.

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[30] It has not been our concern in this paper to criticize the use of the terms 'egocentric' and 'socialized.' Two good comments as to their use have been made. Markey says the fact is not so much that the child is egocentric, or believes himself to be the centre of the universe, as that he actually is the center of his known universe; and that the use of the term 'socialized' is "unfortunate on account of the implication that the integration preceding is not social." (*The Symbolic Process and Its Integration in Children*, 1928, 150.) Lorimer has criticized Piaget's use of the term 'egocentric,' as implying too much introversion on the part of the child. (F. Lorimer, *The Growth of Reason*, 1929, 128.)

[31] Six records were rejected because of obvious fatigue or bad humor on the part of the students making them.

Piaget realized the fact that it is not possible either to oppose or to identify the child and the adult,[32] but he did find differences in the 'structure' of thought sufficient to warrant marked comparisons. We have seen that these comparisons hold true only under limited conditions; where the material presented is not difficult for the adult to comprehend, where one child is inferior in chronological age, in ability, or in cultural heritage to another. We feel, therefore, that a more adequate conclusion, and one which would hold true for children and adults of different degrees of maturity, of experience, and of cultural background, would be, that for every normal individual (exclusive of the physically diseased, the mentally pathological and the emotionally unbalanced) there is some compulsory stimulus-situation, requiring coherent or 'logical' thought, which will reveal his limitations of memory, of understanding, of means of communication through verbal exchange; and which will force him into the use of unsynthetic or 'prelogical' modes of thought.[33]

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[32] In his article in *Mind*, Piaget says, "Tout ce qui est donné chez l'enfant se retrouve chez l'adulte et tout ce qui est donné chez l'adulte se trouve déjà chez l'enfant." (160).

[33] By this generalization we do not mean to imply that a similar one can be made for the attitudes and beliefs of children and adults, another field in which Piaget has made valuable contributions. We have employed on adults Piaget's method of recording spontaneous conversations over a period of time, in order to investigate attitudes and interests, but the work is not sufficiently advanced as yet to predict results.

A QUANTITATIVE ANALYSIS OF INDIAN MUSIC

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In contrast to the conceptual nature of poetry and literature and to the representative character of the plastic arts, music is often considered an immediate language of the emotions. Many have claimed that music produces its effects 'naturally' even upon untrained auditors.[1] So far as Western music is concerned, however, there stands against this view the recent studies showing the effects of practice on preferences for various musical endings, and on the emotional quality of various types of chord.[2] These studies indicate that previous experience and training may play very important rôles in our appreciation of music. Individuals holding a generalization of such views consider most aesthetic preferences as dependent upon the observer's learned emotional responses to the elements of the artistic object. Since this experimental work is too recent to be assimilated into aesthetic theory, the arguments used by relativists against the generalizations of classic aestheticians are seldom based upon experimental evidence. The arguments usually cited may be summarized as follows; The 'canons of art' are inconstant, holding neither for different periods of time nor for different individuals at the same time; the origins of many preferences can readily be explained on the basis of learning, as many of the psychoanalytic interpretations point out; and finally, with particular respect to music, there are marked differences in the scales of different peoples as well as in the type of music developed with their use, although the strangest music may become, with sufficient repetition, a source of great enjoyment.

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[1] See, for example, R. Wallaschek, *Primitive Music*, 1893, 1-326; J. Combarieu, *Music: Its Laws and Evolution*, 1910, 164-175; A. Balz, Music and emotion, *J. Philos.*, 11, 1914, 236-244; F. Densmore, The rhythm of Sioux and Chippewa music, *Art and Arch.*, 9, 1920, 59-67; H. Roberts, Study of primitive music, *Amer. Anthropol.*, 24, 1922, 144-160; G. Wilkinson and A. Gray, *The Mechanism of the Cochlea*, 1924, 188 ff.

[2] P. R. Farnsworth, Ending preferences in two musical situations, this Journal 37, 1926, 237-240; Ending preferences among the three positions of the tonic chord, *J. Comp. Psychol.*, 6, 1926, 95-102; Concerning art standards, *Psychol. Rev.*, 33, 1926, 324-328; C. M. Heinlein, The affective characters of the major and minor modes in music, *J. Comp. Psychol.*, 8, 1928, 101-142.

Such suggestive evidence calls for an experimental attack upon a problem which has been noted frequently enough, and which has enlisted some meager efforts towards solution. Myers long ago declared that to "investigate the degree of universality of those signs in verbal and musical language which serve to communicate states of feeling, is a matter of no small interest." "We would know," he continues, "how far the modification of verbal language in respect to pitch, loudness, timbre and tempo serve as universal methods of communication, and similarly how far the feelings of sorrow, joy and the like, which a given piece of music evokes in the community that produced it, are shared by members of other communities more or less advanced in civilization."[3] Much later Diserens added the comment that "the precise influence of different modes and types of music has not been determined, and waits upon an adequate classification of musical selections, which must probably proceed at first by introspective and statistical methods."[4] Only tentative steps have been made in the direction of such an analysis. Balz offers observations on the differences in martial, dance, and operatic music.[5] Densmore, on the supposition that the music of a race is an expression of its life, has analyzed the rhythm and melody of a few Indian songs.[6] There seems, however to have been but one statistical study, that of Watt, who tabulated the various intervals in Schubert's songs and compared them with tabulations of the intervals in the songs of the Chippewa and Teton-Sioux.[7] Watt found that, for all types of song, the average number of any particular interval was practically the same from both sources.

Most of the experimental work in music, however, has centered about two other problems. The first—the studies of the physiological and behavioral effects of tones, chords, and music—has been fully though uncritically reviewed by Diserens.[8] The second includes the studies of musical enjoyment. These studies indicate that the experimenters have often assumed, in their selection and characterization (as of 'sad' and 'gay' pieces) the very knowledge the present paper attempts in part to determine.[9]

PROBLEM

The problem of the present paper is to determine whether certain objective characteristics of a piece of music form the basis for the mood which it may arouse; and whether these objective characteristics are fairly universal, or are largely dependent upon local musical traditions.

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[3] C. S. Myers, Ethnological study of music, *Anthrop. Essays Presented to E. B. Tylor*, 1907, 236-7.

[4] C. M. Diserens, Reactions to musical stimuli, *Psychol. Bull.*, 20, 1923, 173-197.

[5] A. Balz, *op. cit.* [6] F. Densmore, *op. cit.*

[7] H. J. Watt, Functions of the size of interval in songs of Schubert and of Chippewa and Teton-Sioux Indians, *Brit. J. Psychol.*, 14, 1924, 370-386.

[8] C. M. Diserens, *The Influence of Music on Behavior*, 1926, 1-224. Here is summarized the folk-lore on the effects of music on animals, on the sick, on workers and on its use in myth and magic. The book concludes with the history of the experimental studies, ending with a study by the author himself.

[9] Cf., for example, B. Gilman, Musical expressiveness, this JOURNAL, 4, 1892, 558-576; 5, 1893, 42-73; J. Downey, A musical experiment, *ibid.*, 9, 1897, 62-69; C. S. Myers and C. W. Valentine, A study of individual differences in attitude towards tones, *Brit. J. Psychol.*, 7, 1913, 68-111; C. S. Myers, Individual differences in listening to music, *ibid.*, 13, 1922, 52-71; H. P. Weld, An experimental study of musical enjoyment, this JOURNAL, 23, 1912, 245-308; and most of the studies in *The Effect of Music*, (M. Schoen, ed.) 1927, 1-273.

Method

The method by which this problem is attacked has passed through a number of phases. (1) A direct attack upon Western European music seemed out of the question; this music has tremendous complexity and it offers no obvious basis for classification according to mood; it is divorced from any important rôle in daily life; and it alone in the world of music is structurally harmonic in fact. The type of music common today is said to have originated in the fifteenth century. Any generalizations based upon a more primitive music, however, must be tested upon our Western varieties.

(2) An examination of folk-music again meets great difficulties. There are many ballads in which some simple melody is repeated for an indefinite number of verses, verses which tell a story appealing to a number of contrasting emotional attitudes. Non-ballads, on the other hand, very often refer to characteristics of a people or neighborhood not shared by other folk groups.

(3) The music finally subjected to analysis has been selected from records made of North American Indian tribes. The Indian songs have the advantage, for our purposes, of being closely connected with important aspects of tribal life, and are hence sung upon definite occasions. In consequence they fit more nearly a single emotional attitude. This fact is further attested by the brevity of most songs, and their maintenance of a fairly uniform character. Again, the songs of a number of tribes have been transcribed and published; this allows a check of the reliability of the results among peoples of the same general cultural level. After the development of standards of classification and comparison with the Indian songs, a group of folk songs were analyzed for comparison.

The Indian songs used have been taken from the Chippewa (118 songs), the Teton division of the Dakota Sioux (83 songs), the Mandan and Hidatsas (35 songs), the Northern Utes (31 songs), the Papagos (36 songs), and the Ojibways (31 songs). All save the songs from the last-named tribe were collected by Frances Densmore.[10] Burton's collection of Ojibway songs[11] includes practically none but love songs; and frequently he omits the metronomic rate or even a general indication of the speed of the song. This is not as bad, for our purposes, as Gilman's little collection of Zuni songs[12] which give not the slightest indication of what they are about or when they are sung. The folk songs used in comparison include representatives of Irish, English, Welsh, Scottish, Swedish, German, Italian, Slavic and Asiatic Indian songs.[13]

The selected songs have been subjected to a fairly complete analysis. The major classification of the songs into groups having fairly constant emotional tone was made largely on the basis of the situation in which the song was sung.

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[10] The collections are published by the *Smithsonian Institution, Bureau Amer. Ethnol.*, Vols. 45, 53, 61, 75, 80, and 90.

[11] F. R. Burton, *American Primitive Music*, 1909, 1-284.

[12] B. Gilman, Zuni melodies, *J. Amer. Ethnol. & Archaeol.*, 1, 1891, 65-91.

[13] Twelve songs taken from *Irish Country Songs* (H. Hughes, Ed.), 1909; 8 from *Gems of Welsh Melody* (J. Owen, Ed.), 1886; 31 from *One Hundred Folk Songs* (H. F. Gilbert, Ed.), 1910.

There are three major classes; healing the sick or injured, love songs, and war songs. The healing songs form one group. The love songs are further divided into three groups; 'sad' (parting or absence of lover, death of lover), 'gay' (merry-making, trysts), and the remaining, unspecified songs. The war songs are divided according to the stage of war-like activity which the songs accompany: preparation in camp, on the war path, scouting, in battle, victory (and scalp dances), and the recital of great deeds. War-medicine songs are usually included with the war songs, and the love-medicine songs with the love songs. The various game, dance, gambling and parade songs are omitted from treatment. These various groups of songs are then compared with respect to their pitch, range, speed, types of interval, and types of rhythm. Only the treatment of the intervals and of the rhythms needs comment. Intervals were counted under three heads; those larger than a third, thirds (major and minor), and those smaller than a third. The divisions were made large enough to eliminate any errors in the transcription of the intervals into divisions of Western European music. The correct intervals are probably not always accessible. The divisions used are unsatisfactory in two respects. There should be a separate division for notes sung on the same tone, and there should be a separate division for fourths. A great deal of difficulty was encountered in making classifications of the rhythms. No general scheme, repeated throughout, such as is found in *My Country 'Tis of Thee* occurred, except in the rarest instances. Although a scansion of the songs with similar titles showed certain general resemblances, these could not be stated. The common unit of the rhythmic scheme had to be reduced, finally, to the extent of a measure or less. The percentage of recurrence of each rhythmic unit which was repeated enough to occupy at least one tenth of the song was recorded. A tabulation of these rhythms gave so many divergent patterns that it took several pages to indicate them all. A utilization of those units that occupied at least 20% of the song, on the other hand, would have cut out very many significant occurrences. The tables on rhythms here presented, finally, are based upon the incidence of the short phrases (about 35) each of which constituted at least 15% of the song. It is thus possible for a song to be represented in 6 types of rhythm (6 × 15 = 90), or in none.

An indication of the reliability of the differences based on data from the various tribes may be obtained by comparing the variations of each tribe with that of the entire collection. Since the tribes are not represented by an equal number of songs in each category, differences shown by composite tables could be due simply to the peculiarities of the music of the tribe most represented. Tribal variations will be indicated as the data are discussed.

RESULTS

Table I presents a classification of the songs from each tribe according to the various situations selected as emotionally homogeneous.

Table II presents, by classes, the data for range, pitch, and speed of the songs. Many of the differences are statistically significant, although none of the differences are great enough to form the basis of any successful predicting. About 25 of the differences are large

TABLE I

THE NUMBER OF SONGS FROM THE SEVERAL TRIBES REPRESENTED IN EACH CLASS

	Chip. I	Chip. II	Teton-Sioux	Mandan Hidatsa	Ute	Ojibway	Papago	Total
War-songs								
Organization	11	13	18	7	2	1	1	53
War-path	—	7	8	3	—	1	4	23
General	10	6	2	3	5	—	—	26
Scout	—	1	—	2	2	1	—	6
In battle	1	4	3	3	—	—	2	13
Victory	7	10	15	8	7	—	15	62
Recitatives	2	4	7	1	—	2	—	16
Healing songs	8	—	15	—	15	—	11	49
Love songs								
Sad	9	7	7	2	—	6	1	32
Happy	5	5	1	3	—	13	—	27
Other	5	1	2	1	—	7	—	16
Miscellaneous	—	2	5	2	—	—	2	11
Total	58	60	83	35	31	31	36	334

TABLE II

THE AVERAGE RANGE, DEPTH, AND SPEED OF EACH CLASS OF SONG

	Range			Pitch of lowest note*			Speed†		
	Mean	σ	σ_m	Mean	σ	σ_m	Mean	σ	σ_m
War songs									
Organiz.	10.61	1.73	.24	4.00	2.93	.40	91.5	16.87	2.34
War-path	8.85	1.68	.35	5.80	3.65	.76	85.5	16.67	3.55
General	10.27	1.61	.32	3.81	3.10	.61	95.8	20.20	3.96
Scout	8.83	1.86	.26	4.60	2.22	.91	79.0	22.45	3.58
In battle	9.92	1.94	.54	4.54	1.98	.55	73.5	15.62	4.33
Victory	9.00	1.88	.24	4.52	2.80	.36	85.9	19.70	2.50
Recitatives	9.53	1.41	.35	6.23	2.64	.66	83.7	17.46	4.67
Healing songs	8.28	2.07	.27	5.96	3.03	.43	80.0	19.85	2.86
Love songs									
Sad	9.62	1.77	.31	5.26	3.66	.65	75.0	15.18	2.87
Happy	9.50	2.14	.41	7.79	4.27	.82	80.0	13.50	2.70
Other	9.93	2.04	.51	6.25	3.20	.80	77.8	19.82	5.55
Av.	9.46			5.30			84.		

*Substitute pitch A for 1, A♯ for 2, B for 3, etc.

†The number of songs represented in certain of these figures is reduced due to the fact that the rate for a number of the Ojibway songs was not given.

enough to occur by chance once in a thousand random samplings (where the difference between the means is at least 3.08 times the standard error). A few of the D/σ values are:

Organization *vs.* Healing: for range, 6.47; pitch, 3.32; speed, 3.11;
General war *vs.* Healing: for range, 4.76; (pitch 2.75), speed, 3.90;
Organization *vs.* Sad Love: for pitch, 3.75; speed, 4.45;
Organization *vs.* Gay Love: for pitch, 4.12; speed, 3.22;
General war *vs.* Gay Love: for pitch 3.90; for speed, 3.29;
Organization *vs.* Scout: for range 5.05

TABLE III

THE PROPORTIONAL FREQUENCY OF THE DIFFERENT INTERVALS, AND THE TYPES OF RHYTHM FOUND IN THE VARIOUS CLASSES OF SONG

	Type of interval (percent)			Reliability of intervals	Type of rhythm* (percent)		
	'large'	'thirds'	'small'	(by tribes)	even	uneven	rough
War-Songs							
Organization	7	22	71	2.31	30	30	40
War path	8	17	75	4.80	23	41	36
General war	8	19	73	6.11	28	39	33
Scout	14	17	69	5.27	0	31	69
In battle	11	23	66	3.83	16	22	62
Victory	8	21	70	3.79	25	24	51
Recitatives	6	19	75	3.82	31	23	46
Healing songs	9	14	77	3.31	40	40	19
Love Songs							
Sad	15	20	65	6.50	42	43	14
Happy	9	20	71	3.43	39	32	29
Other love	15	20	65	2.77	27	50	22
Av.	9	19	71	5.22	31	34	35

*See Table IV for examples of the various rhythmic units.

The Chippewa in general sing with a wider range, greater speed and depth than the other Indians; but in most cases the mean for each class of Chippewa songs varies in parallel with that of the rest of the tribes. We can conclude that organization and general war songs are low, rapid, and of wide range. By contrast the love songs tend to be high, slow, and of medium range. Healing songs have a very narrow range, are moderately slow, moderately high. The higher pitch of the love songs is not due to the fact that some women sang, for in those cases the pitch was recorded an octave lower.

Table III presents the general data for types of interval and types of rhythm. The reliability of the types of interval has been measured in two ways. The first is a measure of consistency. The percentages of the intervals for every other song from a random arrangement of each group were averaged and compared with the average percentages for each entire group. In only 4 of the classes of song were the differences for any type of interval as large as 3–4%; these 4 are scout songs, war path, happy love, and the miscellaneous love songs. The second method consisted in using a fairly elaborate device. A table was made giving the percentages of types of interval in each class of song for each tribe. (Where two tribes had only one or two songs in a given class, these were combined.) A composite distribution of the differences of these values from the grand average of 'large,' 'thirds,' and 'small' intervals for all of the songs was made. The σ of this distribution was 5.22. Next, a similar distribution was made for the differences of the songs by tribes in each class. Thus, Chippewa organization songs have 4% 'large,' 22% 'thirds,' and 74% 'small' intervals. The first and last values each differ by 3% from the average song of organization. The values of these σ are presented in the fourth column of Table III. Two values are less than half that for the entire group, and three exceed this value. The low validity of the Scout songs is due to the low value of large intervals in the Chippewa and the Ojibway samples. In the general war songs the Chippewa have but 4% 'large' intervals, as opposed to 18% for the Utes; the Teton-Sioux, and the Mandan-Hidatsa have but 10% 'thirds,' while the Chippewa have 23%. In the love songs the Chippewa and the Ojibway have many 'large' intervals when the songs are sad (19% and 22%), and few when the songs are happy (8% and 7%); but the Teton-Sioux have just the reverse (7% 'large' in sad, 16% 'large' in happy love songs).

The data for differences in types of rhythm are the most striking and significant that we have to deal with. The degree of rhythmic staccato or punctuation in the songs seems highly correlated with the degree of excitement represented by the situation. Comparing the three major groups of song with respect to the percentage of rough rhythms, it appears that the healing songs have fewest, the love songs next, and the war songs most. An inspection of the values for the even rhythms shows just the reverse. The correlation holds further within the members of these major groups. The happy love songs have more rough and fewer even rhythms than the sad love songs. The scout, battle, and victory songs have a majority of rough rhythms, while the scout and battle songs have the fewest even rhythms.

An interesting comparison can be made between the healing and the sad love songs. These two groups are practically the same with respect to the proportion of rhythmic units. But they differ markedly so far as types of interval are concerned. While the healing songs have the greatest number of small intervals, the sad love songs have the fewest of any of the classes; and while the healing songs have the fewest number of thirds, the sad love songs have slightly more than the average.

Tables IV and V present certain additional information about the rhythmic units and the incidence of extreme values in percentage of intervals. From Table V it may be seen that nearly a third of the songs composed of 85% or more of very small intervals are healing songs. Of the remaining 27 songs in this category, 11 are charm songs. The organization of war parties, the victory songs, and the songs in battle contribute half or more of their songs represented in this table to the group having a large percentage of thirds. The greatest contribution to the songs having a high percentage of large intervals comes from the sad and unclassified love songs, and, strangely enough, from the victory songs.

The complex facts relative to the general classification of the Indian songs in the various items of comparison are brought together and summarized in Table VI. Here the war songs are classed into two or three groups, and the love songs into one or two groups, depending upon the closeness of agreement of the members. A total picture of the songs warns us that no particular item can be picked out and labeled as the typical characteristic of a certain class of songs. One can readily see that the characteristics analyzed in this paper are not component parts of the songs, but aspects holding simultaneously for every phrase of a song; and that the effect of one or another characteristic depends not simply upon itself, but also upon the other characteristics. The rhythmic units composing sad love and healing songs are quite comparable in isolation. But the emphasis in the healing songs upon small intervals, narrow range, and greater speed makes the rhythmic structures seem quite different. Again, the rhythmic analysis of the gay love and the first group of war songs (organization, war path, general), and the analysis of the types of interval would indicate that they were highly similar songs. But the greater speed, range and depth of the war group changes even the apparent character of the analytically similar aspects. Of course the words, facial expressions, gestures, intonation, and loudness of the song as performed are additional factors of importance which cannot be taken into this account at all.

TABLE IV

Distribution of Cases of Typical Rhythmic Units

	Songs of										
	War							Healing	Love		
	Organiz.	War path	General	Scout	Battle	Victory	Recitatives		Sad	Happy	Other
Even*											
22-22	12	4	3	0	1	10	0	15	4	0	4
8	7	3	5	0	2	15	7	24	13	8	4
44 (4)	10	4	8	0	2	6	3	13	9	9	2
88	0	0	1	0	0	1	0	5	3	3	1
Other	4	1	2	0	0	4	1	0	0	3	0
Total	33	12	19	0	5	36	11	57	29	23	11
Uneven*											
62	1	1	3	0	0	2	2	11	6	2	7
624	0	2	3	0	0	2	1	4	4	1	1
422	5	5	5	1	0	6	2	11	5	3	2
224	7	3	6	1	1	8	0	8	3	3	0
4222-2224	2	1	2	1	1	2	0	9	0	2	0
84	2	1	4	0	0	2	0	2	0	0	0
Other	16	9	4	1	5	14	3	13	10	8	10
Total	33	22	27	4	7	36	8	56	28	19	20
Rough*											
314	9	2	3	1	0	7	4	3	2	2	1
431	10	3	0	1	6	7	0	8	1	5	0
31-211	3	1	3	0	0	9	0	3	0	0	1
31-31	5	3	2	1	2	6	1	2	0	0	0
22-31	1	1	2	1	1	5	1	1	0	2	0
13-31	1	4	1	0	1	6	1	0	0	1	1
Other	15	5	12	5	10	32	9	9	7	7	6
Total	44	19	23	9	20	72	16	26	10	17	9
Grand Total	110	53	69	13	32	144	35	139	69	59	40

*Rhythm expressed in musical notation, in 16ths., *i.e.* 1 equals a sixteenth; 2, an eighth; 3, a dotted eighth; 4, a quarter; 6, a dotted quarter; and 8, a half note.

There are a number of other factors which tend to decrease the reliability of what differences actually exist. Even though we assume that the songs are all properly classified, it does not follow that every song in each class typifies or expresses the mood of that situation

TABLE V

The Occurrence of Songs with Extreme Values of Intervals

	Songs of										
	War						Healing	Love			
	Organiz.	War path & Gen.	Scout	Battle	Victory	Recit.		Sad	Happy	Other	Total
85% intervals less than third	7	8	0	1	4	2	11	1	3	0	37
33% intervals thirds	11	8	1	2	14	1	1	4	4	0	46
20% intervals greater than thirds	3	3	2	1	9	0	4	12	4	5	43
Total occurring	21	19	3	4	27	3	16	17	11	5	126
Proportion of all songs of this class	40	41	50	31	44	19	33	53	41	31	39

TABLE VI

Summary of the General Data on Indian Music in Terms of Rank Order

					Type of interval			Type of rhythm		
Class		Range	Pitch	Speed	'large'	'third'	'small'	even	uneven	rough
War	Organization war path general	5	2	5	1	3	4	2	3	4
	recitative victory	4	4	4	1	3	4	2	1	5
	scout battle	2	1	2	4	2 5	2	1	1	5
Healing the sick and wounded		1	5	3	2.5	1	5	4	4	2
Love	Sad	3	3	1	5	4	1	5	5	1
	Gay		6		2.5		3	3	2	3

The rank order 1 always indicates the class with the smallest value: low range, low pitch, slow, and small percentages of types of interval and rhythm.

equally well. The old and established tribal songs that have managed to survive several generations might best represent the typical characteristics of a song for a given mood; but there are only a few songs known to be old. Our groups actually contain many that are 'personal' and a number that are relatively new. A second factor that decreases reliability is the difference in attitude of the various tribes upon the same objective situation. For such people as the Chippewa or Teton-Sioux, a war expedition was often a sort of game in which horses, or a few safely taken scalps, were the stakes. The Papagos were, on the other hand, very pacific, and never went to war against their enemies the Apaches except in retaliation. The expeditions were well organized crusades; a definite routine of activity was required especially on the part of the medicine men. The warriors themselves had to do certain things on the various nights out from home, including the recital, by members of an appropriate group, of a pair of set speeches. Upon completion of the expedition those warriors who had killed Apaches were required to undergo a 16-day ordeal of purification, involving hardships even greater than those of the warpath. It was considered a crime to kill even an enemy, punishable by the spirits of the dead. Another factor is the breadth of the categories used in classifying the songs. In order to have enough cases to make the differences statistically significant, it has been necessary to include more and more dissimilar songs. But now that certain types of difference seem to stand out as significant, for the total groups of songs,

TABLE VII

Types of Interval and Rhythm for Some Special Groups

Class	No.	Type of interval (percent)			Types of rhythm (percent)		
		'large'	'thirds'	'small'	even	uneven	rough
Derision	7	8	20	72	36	36	27
War-medicine	13	5	9	86	44	38	19
After killing a warrior	8	7	24	69	13	7	80
Death songs	7	12	17	71	20	40	40
Scout songs	6	14	17	69	0	31	69
Disappointment in love	5	32	11	57	14	57	29
Lonesome	8	21	19	60	31	38	31
Parting	5	16	17	67	40	60	0
Absence	7	14	26	60	26	42	32
Death of lover	11	8	18	74	40	36	24
Happy love	14	12	23	65	20	56	24

differences of the same character for small groups of highly similar songs will have more significance. Table VII presents the data with respect to intervals and rhythms for 11 such small, but homogeneous, groups. A similar set of 10 groups has been prepared from an analysis of the folk songs (Table VIII). This last list is very unsatisfactory since there are less than 6 songs represented in more than half the groups. The comparisons that can be drawn are obviously only suggestive. The outstanding characteristics of the data are readily seen. It likewise appears that the agreement between the Indian songs and the folk songs extends only so far as the major classes dedicated to war and love.

TABLE VIII

TYPES OF INTERVAL AND RHYTHM FOR SOME GROUPS OF EUROPEAN FOLK SONGS

Class	No.	Types of interval (percent)			Types of rhythm (percent)		
		'large'	'thirds'	'small'	even	uneven	rough
Prep. for war	5	10	15	75	20	60	20
War marches	5	7	18	75	10	30	60
Victory songs	2	11	32	64	25	0	75
War dirge	2	12	17	71	50	50	0
Death of lover	6	12	17	71	11	78	11
Absence, parting	4	9	15	76	37	50	12
Lonesome, sad	9	12	11	77	56	44	0
Description of, or song to, love	17	9	16	74	22	69	9
Sentimental or serious love	7	6	9	85	50	43	7
Gay, playful	4	13	17	70	57	43	0

CONCLUSION

In spite of all these factors of unreliability, the limitation in the completeness of analysis, and the limitations of analysis itself, fairly straightforward and significant differences stand out between certain classes of songs which doubtless have a different emotional appeal.

In conclusion, it appears that at least the dynamic characteristics of moods can be communicated by song even when the performers or audience have no means of conceptual communication. The dependence of this communication through the common effects of rhythms and tempo upon bodily movements seems apparent; but no such simple explanation seems applicable to the results with respect to distribution of intervals, or of the range or depth of the song. The particular significance of various types of rhythm and interval, range, pitch, depth, and their combinations for one mood or another cannot be clearly and easily deduced from this Indian music since the attitudes and feelings of such peoples in the classified situations are not readily determinable. A determination of the significance of these aspects of music could be made, as it seems to me, on the basis of a statistical analysis such as the one here presented of a body of Western European music whose expression of a particular emotional tone had actually been established by the agreement of a number of auditors.

THE MEASUREMENT OF MUSCULAR TENSION AS A TECHNIQUE FOR THE STUDY OF EMOTIONAL TENDENCIES

By Elizabeth Duffy, Sarah Lawrence College

Experimental investigation of the 'emotions' has shown almost uniform neglect of the highly significant changes in the skeletal muscles which occur under 'emotional' stimulation.[1] Although one of the most prominent theories of the emotions maintains that the prime function of the bodily changes in certain emotional states is to energize the muscles,[2] few investigators have studied directly the relation between skeletal muscle changes and emotional states. Indeed, many investigators, when observing such muscular changes during emotions, have appeared to regard them as entirely incidental and in no way an index of the state itself.

There are, however, a few studies which have been concerned with this problem. Golla has reported an increase in tonus of the arm muscles upon presentation of a 'nocuous' physical stimulus.[3] Johnson has obtained measures of tension in the muscles of the hand during a tapping performance, and in presenting her results has advanced the theory that "temperamental differences are indicated by differences in tension of expressive muscles under differing stimuli."[4] Jacobson has suggested,[5] as a result of experiments with normal and neurotic subjects, that the 'curve of relaxation' (the diminution of the knee jerk during a period of experimentation) is a possible test for nervous hypertension or neurosis. Duffy has found,[6] in an experiment with nursery school children, that individual differences in excitability were definitely correlated with differences in tension in the

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[2] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage*, 1916, 215-216.

[3] F. L. Golla, The objective study of neurosis, *Lancet*, 201, 1921, 218.

[4] Buford Johnson, Changes in muscular tension in coördinated hand movements, *J. Ex. Psychol.*, 11, 1928, 334.

[5] Edmund Jacobson, Differential relaxation during reading, writing, and other activities as tested by the knee-jerk, *Amer. J. Physiol.*, 86, 1928, 693.

[6] Elizabeth Duffy, Tensions and emotional factors in reaction, *Genet. Psychol. Monog.*, 7, 1930 (no. 37), 65.

muscles of the unused hand when the subjects were making discriminative reactions. Luria[7] has reported a study of motor reactions during certain associative processes and has found that the disturbances of the motor curve (obtained by pressing with a finger on a pneumatic appliance) expressed a corresponding affective change evoked by the verbal reactions. A few other investigators, including Morgan[8] and Freeman,[9] have reported increases in muscular tension in situations which might be, but were not usually, classed as emotional.

Experiments in this field are obviously few in number. Of the available studies only one or two have attempted to demonstrate that individual differences in the emotional tendencies of normal subjects may be revealed through differences in tension of the skeletal muscles. Because of the importance of the question it seemed desirable to obtain more conclusive evidence, and the present experiment was undertaken as an extension of the writer's earlier investigation. In this study we obtained measures of tension in the muscles of both the used and the unused hand while our *S*s, 18 nursery school children, were making discriminative reactions to pictures, and again while they were tapping. These measures of tension were related to estimates of excitability and of degree of adjustment to the nursery school. Tension during discriminative reactions was measured for every *S* on 5 separate occasions, affording data on the effect of habituation upon tension, and on the constancy of individual differences in muscular tension.

The plan of the study was analytic and exploratory, emphasizing intensive work with a small number of *S*s. It seems reasonable to suppose, however, that a high degree of consistency of results, when considered from a number of different points of view, indicates reliability of the data comparable to the reliability obtained from a single variety of data based on a larger number of *S*s.

EXPERIMENTAL PROCEDURE

The *S*s were 18 children in the Child Development Institute of Teachers College, Columbia University.[10] Their ages ranged from 2 yr., 11 mo. to 3 yr., 10 mo. (median, 3 yr., 3 mo.)

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[7] A. R. Luria, Die Methode der abbildenden Motorik in der Tatbestandsdiagnostik, *Zsch. f. angew. Psychol.*, 35, 1930, 139-183.

[8] J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 35, 1916, 42-48.

[9] G. L. Freeman, Changes in tonus during completed and interrupted mental work, *J. General Psychol.*, 4, 1930, 309-332.

[10] Two of the *S*s failed to comply with the instructions for the discriminative reactions. Their records were discarded. One *S* could not be obtained for the tapping test. The results for discriminative reactions are based on 16 *S*s, and for tapping, on 17 *S*s.

(*1*) *Discriminative reactions.* The *Ss* were brought to the laboratory individually and seated at a small table in front of a screen on which an automatic lantern projected a series of 40 pictures at precisely 4-sec. intervals. The room was slightly darkened and *S* was instructed to press a key, which sounded a buzzer, whenever a picture of an automobile appeared, but to make no response to other pictures, which were of a horse and of a street car.[11] The slides were presented in approximately random order.[12] The appearance of the pictures and the closing of the key were graphically recorded. The degree of pressure upon the reaction-key was also graphically recorded by a specially constructed device.[13]

Throughout the experiments *S* held in each hand a dynamograph, which gave a graphic record of the pressure of his grip.[14] A large stylus attached to the dynamograph held in the right hand was used for closing the reaction-key. During the interval between successive reactions the stylus was rested upon the base of the key. Arm supports prevented strain. Temperature, light, and sound were held as constant as was feasible. All experiments were conducted between 9:30–11:00 A.M.

After 2 preliminary practice series this experiment was repeated 5 times with every *S* within a period of 4 mo. The order of the pictures was altered for every presentation. The last 3 series of the experiment differed from the first 2 in two respects: (a) a red light was substituted for the buzzer, and (b) between reactions the stylus was rested on a fixed support instead of the key-base.[15]

(*2*) *Tapping.* After the *Ss* had completed the discriminative reaction experiments, they were given the tapping test, which consisted of tapping as rapidly as possible, first for 15 sec. with the right hand, and then for 15 sec. with the left hand. The stylus for tapping was the one attached to the dynamograph for the right hand and previously used for closing the reaction-key. In the unused hand the *S* held the dynamograph held in this hand during the discriminative reactions. Graphic records were obtained of the pressure from the unused hand and the pressure from the used hand, together with 1-sec. time intervals.

RESULTS

(*1*) *Individual differences.* Our results reveal marked differences between individuals in the tension of their muscles as that was measured by the pressure of the hand upon the dynamograph.[16] Table I gives the average tension scores, with their m.v., and the range of dispersion, for each hand for the discriminative reactions and the tapping. The average tension during the discriminative reactions for the unused hand was 16.8 ± 2.1 mm., ranging for the different *S*s from 12.6–21.4 mm.; and for the used hand it was 23.2 ± 3.6 mm., ranging from 17.2–33.8 mm. The average tension during the tapping was: for the unused hand 18.9 ± 4.5 mm., range, 10.0–30.8 mm.; for the used hand 40.3 ± 13.1 mm., range 20.0–73.6 mm.

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[11] The picture of the automobile showed a child crossing the street directly in the path of the car.

[12] Not more than 3 pictures of one kind were presented in succession.

[13] The reaction-key rested on a movable base calibrated for pressure in 50 grm. increments from 150 grm. (the weight of the dynamograph) to 850 grm.

[14] The dynamographs were made of rubber tubing (8 × 1 × 1/8 in.) plugged at both ends with an aluminum drum. They were connected with an air system at atmospheric pressure and were calibrated in mm. of mercury from 0–210 mm., by 10 mm. increments.

[15] The first change was made because the buzzer proved unpleasant to several *Ss*. The second because pressure during the rest period obscured the record of pressure upon the reaction-key. Only pressure records obtained after this change (Series III, IV, and V) could be used in the results.

TABLE I

SHOWING THE AVERAGE TENSION SCORES WITH THEIR M.V., AND THE RANGE IN EACH TEST

(Values in mm. of mercury)

Test	Hand	Av.	M.V.	Range		
				high	low	diff.
Discrimination	unused	16.8	2.1	21.4	12.6	8.8
	used	23.2	3.6	33.8	17.2	16.6
Tapping	unused	18.9	4.5	30.8	10.0	20.8
	used	40.3	13.1	73.6	20.0	53.6

Three questions arise concerning the results. (1) How constant are the individual differences? (2) What is the psychological significance of the differences? and (3) What, within the limits of this experiment, is the best measure of tension?

(*a*) *Constancy during successive performances.* The discriminative experiments, it will be recalled, were given the *S*s 5 times over a period of 4 mo. The data obtained afford information concerning the constancy of the individual differences in tension.

In comparing the tension scores made in these 5 separate experiments there are two disturbing factors which must be taken into account: (1) the change in apparatus and procedure after Series II, which partially invalidates a comparison of Series I and II with the later series; and (2) the fact that 9 of the 16 *S*s developed whooping cough during the period when Series III and IV were being recorded. This suggests that the reliability of these records may have been decreased through the effects of illness and absence. Table II gives the coefficients of correlation[17] for the tension in the used hand[18] during the 5 experiments (Series I–V).

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[16] A tension score was obtained for the highest point reached by the pressure tracing during every 4-sec. interval. An *S*'s score for each hand during a given series of the experiment represents the average of 40 such scores. His final score for each hand during the 5 series of discriminative reactions represents an average of 200 such scores.

TABLE II

COEFFICIENTS OF CORRELATION FOR TENSION IN THE USED HAND DURING DISCRIMINATIVE REACTIONS

Series	I	II	III	IV
II	.74 P.E. .08			
III	.24 P.E. .17	.21 P.E. .17		
IV	.33 P.E. .16	.21 P.E. .17	.45 P.E. 14	
V	.58 P.E. .12	.33 P.E. .16	.34 P.E. .16	.74 P.E. .08

There are two instances of a close correlation between series: that of Series I with Series II (r = 0.74) and that of Series IV with Series V (r = 0.74). Other coefficients of correlation range from 0.21–0.58. Table IV shows that the series which correlate most closely had the smallest median number of days between them. But not only was this true, they had also (with the exception of Series IV) the smallest number of *S*s absent from the nursery school for excessively long periods before the taking of their records. When Series III was recorded, 5 of the *S*s had taken no part in the experiment for 57 or more days, and when Series IV was recorded, 5 of the *S*s had taken no part for 42 or more days. When Series I and II were recorded the epidemic of whooping cough had not occurred, and when Series V was recorded it was decidedly on the wane.

But in spite of specific and of general disturbing factors we obtain consistently positive and, under favorable circumstances, very high coefficients of correlation between tension ranks of the *S*s on different days. Correlations might well have been lowered, not only by errors in measurement, but by a number of factors which doubtless affect tension and which could not be brought under control, *i.e.* state of the *S*'s health, his activity immediately preceding the experiment, experiences of the day, etc. It is to be expected also that adaptation to the repeated presentations of the experimental situation would proceed at a different rate with different *S*s. For these reasons correlation as close as that found between Series I and II or Series IV and V can be accounted for only by assuming (1) that individuals tend to preserve their ranks with respect to tension in a given situation, and (2) that the technique here employed for measuring tension is reliable.

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[17] All coefficients of correlation were obtained by the method of rank differences unless otherwise stated.

[18] The used hand is made the basis for comparison since, in this experiment, it proved to be a better measure of tension than the unused hand.

(*b*) *Constancy during the performance of different tasks.* It is of interest to inquire not only the extent to which individuals preserve their ranks in tension in the *same* situation, but also the extent to which these ranks are preserved in *different* situations. The answer to this question would afford information concerning the extent to which tension is *specific*, or a function of the given situation, and the extent to which it is general, or a characteristic of the individual.

In this investigation, it will be recalled, the *S*s performed two different tasks, (1) making discriminative reactions to pictures, and (2) tapping (under instructions emphasizing speed). Many elements in the two situations were identical, but the differences were sufficient to yield relevant information.

The coefficients of correlation for tension in the corresponding hand during discriminative reactions and tapping were: for the unused hand, 0.15 ± .18; for the used hand, 0.50 ± .14; and for the average of both, 0.58 ± .12.

It is apparent, from the results of the used hand and the average of both, that individuals tend to preserve their respective ranks in tension (though not the absolute value of the tension scores) during the performance of different tasks. It seems safe to conclude, therefore, that the tension manifested by an individual is not entirely specific, or a function of the particular situation, but that it tends to be general, or to characterize the individual in different situations. To what extent situations could be varied without destroying correlations between ranks in tension there are no data for an answer. Our data do indicate, however, that a considerable difference in the tasks assigned does not produce very great variations in the tension ranks of our *S*s. It seems not unlikely that relative degree of tension is a fundamental characteristic of the individual, varying, as all characteristics do, with changes in circumstances, but varying around a central tendency which is different for different individuals.

(*2*) *Significance of individual differences*: (*a*) *Tension and dynamometer scores.* In attempting to determine the significance of individual differences in muscular tension the suggestion occurs that these differences may be due to differences in muscular strength, the stronger individual tending to exert greater pressure upon an object held in the hand. To test this hypothesis the *S*s' strength of grip was measured by means of a dynamometer. Every effort was made to secure maximum pressure. Three trials were made with each hand, and the final dynamometer score was computed as an average of the highest score from each of the hands. Correlation of these scores with average tension in both hands during discriminative reactions yielded a coefficient of −0.03. Consequently, differences in muscular strength cannot account for differences in muscular tension.

(*b*) *Effect of habituation.* The significance of individual differences in tension of the muscles is disclosed in part through an examination of the effect of habituation upon the tension scores. There are two ways in which this effect may be studied: (1) we may compare tension scores during an early and a later part of a day's performance; and (2) we may compare tension scores during the early and later series of the experiment.

Table III gives tension scores in the unused hand[19] during the first and the last 32 sec. of every series of the experiment. The interval separating these periods was 1 min., 36 sec. The sum of the tension scores of every *S* was obtained for the first and for the last period, and from these scores we obtained the median for the group for each of the periods.

TABLE III

TENSION IN THE UNUSED HAND DURING THE FIRST AND THE LAST THIRTY-TWO SECONDS OF THE DAY'S PERFORMANCE

	Median of sums		No. *Ss* showing			No. automobiles pictured*	
Series	for first 32 sec. (mm.)	for last 32 sec. (mm.)	decrease	increase	no change	first 32 sec.	last 32 sec.
I	160	125	9	3	4	4	3
II	100	90	8	3	5	4	3
III	150	85	9	3	4	2	3
IV	160	95	13	2	1	3	3
V	160	80	13	1	2	3	2

*The appearance of an automobile required a response, and therefore might have affected tension.

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[19] The unused hand was made the basis for comparison because it was assumed that tension in this hand would vary less directly with the number of times *S* closed the reaction-key during a given period.

Median tension scores show a decrease from an early to a later part of a day's record, and this decrease appears, not only on the average, but in well over half of the individual cases. It seems probable that, in spite of the short interval separating the two periods compared (1 min., 36 sec.), some adjustment to the situation has been effected by *S*, which is reflected in reduced tension.

The effect of habituation may be observed also through a comparison of tension scores during early and later series of the experiment. But in this comparison we meet again the complicating effects of the epidemic of whooping cough and the changes in the experimental procedure made after the recording of Series II. Again we are led to conclude that Series I and II offer the most valid basis for comparison. Table IV gives median tension scores in the used and the unused hand during each series of the experiment, together with certain facts which may be relevant in interpreting these scores.

TABLE IV

Showing Tension Scores During Every Series and Factors Which May Have Affected Tension

Series	Median tension (mm. of mercury)		Av. no. days between records	No. *Ss* absent during week before record taken	No. *Ss* showing decrease in tension from previous record	
	unused hand	used hand			unused hand	used hand
I	16.8	21.7		3		
II	13.6	17.9	9	4	11	13
III	17.1	23.5	15	8	3	3
IV	17.5	22.0	14.5	10	9	9
V	17.4	25.0	4.5	8	9	6

Series II shows a marked decrease in tension scores in both the median and the individual results. Series III, the point at which changes were made in the apparatus and procedure, shows an increase in tension, in the median and with almost every individual *S*. Here is evidence that habituation decreases tension, and that the introduction of new elements into a situation increases tension; but the evidence for the latter statement is not unequivocal, since the increase noted in Series III may be due to the effects of whooping cough. Series IV and V fail to show the decrease which under ordinary circumstances would be expected.

However, we have unequivocal evidence of a decrease in tension during the latter part of a day's performance and during the latter of the two series recorded before the outbreak of whooping cough. This decrease in tension is due, apparently, to habituation, which involves in all probability greater ease of adjustment, less useless expenditure of energy, less anxious expectancy.

(*c*) *Tension scores and pressure on the reaction-key.* Further data on the significance of individual differences in muscular tension may be obtained through an examination of the relation between tension scores and degree of pressure upon the reaction-key when making responses to the pictures.

It will be recalled that the reaction-key was constructed to yield a measure of differences in degree of pressure exerted upon it. Pressure scores were obtained for Series III, IV, and V for 15 *S*s.[20] The rank of the *S*s in total pressure score (the sum of the scores in the 3 series) was correlated (1) with the average tension in both hands during all series of the experiment, and (2) with the average tension in the *used* hand during Series III, IV, and V. Table V presents the results.

TABLE V

COEFFICIENTS OF CORRELATION BETWEEN TENSION SCORES AND PRESSURE ON THE REACTION-KEY

Both Hands (All series)	Used Hand (Series III, IV, and V)
r .63 P.E. .11	r .24 P.E. .17

Pressure scores are closely related to average tension in both hands during all series of the experiment (r = 0.63), and, surprisingly enough, are only slightly related to tension in the used hand (the hand exerting pressure upon the key) during the particular series for which this pressure was measured. Two interpretations may be suggested. It seems possible that pressure upon the key was an alternative form of expression of tension, so that tense *S*s who pressed the key with great force exerted correspondingly less pressure upon the rubber grip in the used hand, thus reducing the correlation between what might be conceived of as two separate measures of tension. It seems possible, on the other hand, that pressure upon the key, or *force* of reaction, is a habit of response closely connected with habitual tendencies with respect to tension of the muscles, and that these habitual tendencies are indicated more adequately by tension scores from both hands during all series of the experiment than by the more limited sampling of tension represented by scores from the used hand in Series III, IV, and V. The suggested relation between muscular tension and force of reaction finds abundant confirmation in ordinary observation. Tense individuals may be observed to walk with a heavier tread, speak with greater intensity, exert greater pressure in writing and, in general, give evidence of greater energy of response. Occasionally these effects are masked by inhibition; but they are usually observable in greater or less degree. These general observations must be distinguished from the experimental data, which indicate only that high tension scores (when the measures are taken from both hands on 5 separate occasions) correlate rather closely with heavy pressure on the reaction-key.

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[20] One *S* in the discriminative experiments often touched the reaction-key too lightly to close it, because he disliked the sound of the buzzer. This form of response persisted after the buzzer had been removed. For this reason his pressure scores were discarded. For an explanation of the omission of pressure scores for Series I and II see footnote 15, p. 148.

(*d*) *Tension scores and ratings.* Three nursery school teachers in daily contact with the *S*s were asked to rank them in excitability. The instructions were as follows: "Rank the children from highest to lowest in excitability, *i.e.* tendency to become excited. This trait should not necessarily be identified with 'emotionality' or 'instability.' Excitable individuals are often described as 'keyed-up,' 'high-strung,' 'tense,' or 'stirred-up'." After the rating had been completed the teachers were asked to describe as fully as possible the traits they had had in mind in their classification of the *S*s. Traits mentioned were the following: high, shrill voice, screaming, exuberance, nervous tension, intense and frequent manifestations of emotion, type of reaction when asked for any reason to leave the group. The teachers had previously been asked by another experimenter to rate the same group of *S*s on stability under usual and unusual circumstances. When questioned as to whether their ratings on excitability were different from the ratings they would have given on stability, they all replied that they were not. We must conclude, then, that the ratings probably represent a composite of traits variously termed excitability, stability, adjustment to routine, etc., and we should not attempt to interpret them more specifically.

On the basis of adjustment to the nursery school, the physician and the nurse at the Institute separated the *S*s into 3 categories: poor, average, and good.

TABLE VI

Correlation of Average Tension Scores with Ratings on Excitability and School Adjustment

	Ratings	
	Excitability	School Adjustment†
Unused Hand	.29 P.E. .16	.58
Used Hand	.58 P.E. .12	.61
Av. Both*	.52 (.60) P.E. .13 (.11)	.58

*The figures in parentheses represent the correlation with ratings done at *E*'s request. All other figures represent correlations with composite ratings, *i.e.* ratings which include the earlier ratings on stability.

†The maximum coefficient of mean square contingency which can be obtained on the basis of 3 categories is 0.816.

Table VI gives, for tension in the muscles of the hands, coefficients of correlation with the ratings of the teacher, and coefficients of mean square contingency with the ratings of the physician and nurse.

With the exception of the correlation between tension in the unused hand and ratings on excitability,[21] our coefficients of correlation and our coefficients of contingency are uniformly high or moderately high. We have here evidence of a striking tendency for high tension scores to be associated with traits defined as excitability, instability, etc., and with a type of adjustment to the nursery school which is rated 'poor.'

These correlations afford further evidence that the relative degree of tension of the muscles of an individual is not fortuitous, but is highly significant. They indicate definitely the direction in which we are to look for this significance, *i.e* in the individual's degree of adjustment to his environment, his stability, his degree of susceptibility to excitation.

(3) *Relative merit of various measures of muscular tension.* Three different measures of muscular tension have been referred to in the course of the discussion, (1) tension in the used hand, (2) tension in the unused hand, and (3) tension in both hands, or the average of the first two measures. To what extent do these measures correlate with each other, and what are their relative merits as indicators of tension? Table VII gives intercorrelations of the various tension measures.

There is very little correspondence between tension in the used and the unused hand during discriminative reactions. We have a low positive correlation which is statistically unreliable. Tension in the used and tension in the unused hand during tapping yield a significant positive correlation (r = 0.58). The unused hand during discriminative reactions shows a much lower correlation with the average of both

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[21] For an explanation of the low correlation found here see pp. 156-158.

TABLE VII

Coefficients of Correlation Between Various Measures of Tension

Hand	Test		
	Discrimination		Tapping
	Used Hand	Av. of Both	Used Hand
Unused	.13 P.E. .17	.46 P.E. .14	.58 P.E. .11
Used		.92 P.E. .01	

than does the used hand. But this may be explained by the fact that the scores for the used hand were numerically larger and therefore exerted more influence upon the average.

In regard to the relative merit of the various measures of tension we have two scores of information: (1) a comparison of their coefficients of correlation with the ratings, and (2) a comparison of their coefficients of correlation with tension in the corresponding hand (or hands) during tapping.

Table VI gives coefficients of correlation and coefficients of contingency between ratings and various measures of tension. The tension measures which correlate most closely with the ratings are those from the used hand and those which represent an average from both hands (influenced chiefly by the numerically larger scores from the used hand). The unused hand makes a poorer showing, though the coefficient of contingency between the scores from this hand and the ratings on school adjustment is surprisingly large in the light of facts to be presented later.

A comparison of the various coefficients of correlation between tension in corresponding hands during discriminative reactions and tapping indicates that scores from the used hand during these two performances correspond much more closely than do scores from the unused hand. Scores representing the average from both hands correspond more closely than the scores from either hand taken alone. The coefficients of correlation are: unused hand, 0.15 ± 0.18; used hand, 0.50 ± 0.14; average of both, 0.58 ± 0.12.

From this evidence it appears that the scores from the used hand are, in this experiment, a more adequate measure of tension than the scores from the unused hand. It seems possible, though here the data are not clear, that the average of both these scores (which is determined to a greater extent by scores from the used than by scores from the unused hand) is a better measure than scores from either hand taken alone.

But it is important to note that the relative inadequacy of tension scores from the unused hand is more likely to be a result of particular conditions of the experiment than of any fundamental difference between the two hands as a source of tension measurement. The explanation which seems most plausible is that the rubber grip of the dynamograph held in the unused hand was not sufficiently sensitive to differentiate individuals adequately with respect to tension in that hand. It was made of rubber of the same thickness as that of the dynamograph for the used hand, but since pressures from the used hand were greater, the lack of sensitivity of the dynamograph would not operate so disadvantageously in that case. This explanation is supported by the fact that in tapping, where the pressures were uniformly greater, the scores from the used and the unused hand correlate considerably more closely (r = 0.58) than in the discriminative reactions (r = 0.13). That the small range of scores is a possible explanation of the relative inadequacy of tension measures from the unused hand during the discriminative reactions is indicated by Table I. In an earlier experiment in which very thin rubber was used for the dynamograph in the unused hand, tension scores from this hand correlated with ratings to about the same extent as scores from the used hand in the present experiment.[12]

It seems reasonable to conclude that the correlation between tension scores in the used and the unused hand during tapping (r = 0.58) is more representative of the actual relation between tension in the two hands than the lower correlation during discriminative reactions. We have no adequate basis for a conclusion as to whether it is of more value in general to measure tension in a used or a corresponding unused part of the body. It seems probable that a combination of these two measures would be of greatest value, first, because correlations in this experiment were occasionally raised by combining the two measures and, second, because it seems reasonable to suppose that individuals would differ in the degree to which they shunt tension into a used or a corresponding unused part of the body. If this is true, the most adequate measure of tension would include a sampling from both an active and a quiescent region.

SUMMARY

Our *Ss* differed markedly with respect to tension in the muscles of the hands in the same experimental situation. During successive performances of the same task (discriminative reactions) and during the performance of a different task (tapping), they tended to maintain they respective ranks in tension. Apparently tension differences are relatively constant and are not entirely the function of a specific situation.

In attempting to demonstrate the significance of individual differences in muscular tension, we have shown (1) that these differences cannot be explained by differences in muscular strength, (2) that habituation to a situation tends to decrease tension, (3) that individuals with higher tension scores tended to press the reaction-key with greater force, and (4) that individuals with higher tension scores tended to be rated lower on stability and degree of adjustment to the environment.

On the basis of these data it appears that we have in this technique of the measurement of muscular tensions a very promising approach to a study of individual differences in emotional tendencies.

DISCUSSION AND CONCLUSIONS

It seems entirely consistent with psychological fact and theory to conclude that individual differences in tension of the skeletal muscles are associated with differences in emotional tendencies.[23] Increased tension has been observed by many investigators under a variety of circumstances, *e.g.* sensory stimulation, problem solving, the introduction of distractors, and, less often, emotional stimulation.[24] From the evidence at hand it appears possible that increases in tension signify an increase either in excitation in general or in that particular type of excitation called 'effort.' They appear to be associated with striving and expectancy and other states in which the organism is exerting or preparing to exert increased energy. Without question, excited emotional states are among those in which such increases in muscular tension are to be expected; for they are often described as aroused, watchful states, states of increased effort, by which the organism is attempting to meet an emergency. But, as we have pointed out, tension increases have been found not only in excited emotional states, but in other aroused or effortful states, including states of mental work. Why, then, should such increases in tension be considered useful in a study of the emotions?

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[12] Duffy, *op. cit*, 33.

To answer this question we must review briefly our present knowledge of the emotions. Physiological studies of the emotions have shown that certain bodily changes occur during emotional states, but these studies have failed (1) to discover patterns characteristic of particular emotions, and (2) to reveal in any clear-cut way characteristic differences between aroused emotional states and states of mental work. The changes in pulse rate, respiration, galvanic phenomena, etc., found in emotional states have been found also during intense mental effort. It is obvious that active mental and excited emotional states possess a large group of common elements. These common elements, we would maintain, constitute the excited or aroused quality of the states in question; they produce the generally observed dynamogenic effects. It seems probable that our various physiological measures or indices have been, in actuality, indices of the extent to which the organism was *aroused*, the extent of the process of dynamogenesis, and not indices *per se* of an emotional as opposed to a mental state.

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[23] The reference throughout the article is to tensions of the muscles which are assumed to be 'involuntary.'

[24] For a discussion of the effect of sensory stimulation upon the skeletal muscles see Ch. Féré, *Sensation et Mouvement*, 2 éd. rev., 1900, 1-170. Investigations of the effect of emotional stimulation are mentioned in my introduction. For the effect of mental work and certain other conditions see A. G. Bills, Mental work, *Psychol. Bull.*, 28, 1931, 505-529.

But, in practice, these physiological measures have been useful as indices of states called 'emotional.' The reason for this is to be found in the current definition of an emotion. Psychologists have been unable to draw an objective distinction between mental states and emotional states except on two arbitrary bases: (1) degree of disturbance in the organism, and (2) knowledge of the stimulus. Marked degrees of disturbance in the organism, if they are not due to obvious 'physical' causes, are attributed to the presence of 'emotional stimuli,' and the state of the organism is called an 'emotional state.' Thus a student taking an examination would be called *mentally* alert or aroused if his pulse rate showed a slight increase, but he would be called *emotionally* aroused if the increase were very great. Degree of excitation has been the criterion for such determinations. But how great a degree of excitation is necessary to determine a difference in kind, *i.e.* to delimit an emotional from a mental state, has not been established. Consequently, it has been necessary to employ a second standard to meet the deficiencies of the first. To this end psychologists have engaged in a search for the stimulus which gave rise to a particular state. But very often in practice 'kind of stimulus' is decided from 'degree of excitation'—*e.g.* the 'emotional' quality of a stimulus-word is determined from its effect on pulse rate or blood pressure.

Degree of muscular tension affords a very obvious but largely neglected index of degree of excitation and consequent dynamogenic effect. Differences between individuals in tension of the skeletal muscles in the same situation appear to indicate differences in the extent to which the situation has 'excited' or 'aroused' the respective individuals; and habitual differences between individuals in tension of the muscles appear to indicate differences in their general tendency to become 'aroused' or 'excited,' and thus to indicate differences in temperament. These differences in 'dynamogenic tendency' are of great importance and deserve study apart from their rôle in so-called 'mental' or 'emotional' states. Increase in dynamogenic effects within the organism, whenever found, is associated with marked changes in behavior. Among these changes are, in all probability, increased speed and force of reaction, and greater impulsion, or increased difficulty of inhibition. If such facts are established, we may in the future be able to describe certain patterns of reaction which occur in excited states and which, therefore, tend to characterize individuals who become frequently and easily excited. These data would have wide applications, since there are, undoubtedly, certain tasks or vocations for which such individuals would be particularly fitted and, conversely, certain tasks at which they would be handicapped. Great promise is held in this type of approach to the study of temperamental tendencies. It seems possible that it will substitute, for arbitrary classifications of 'key' traits of personality, experimentally established physiological bases for temperamental differences. Such bases would enable us to predict correlations between traits because of the derivation of these traits from a common source. But this does not imply that such a group of traits would not be subject to variation, since changes in general physiological condition or in environment may at any time produce changes in characteristics.

To return to a consideration of dynamogenic tendency, it is unsafe to assume that a given degree of excitation will result always (in either the same individual or in different individuals) in precisely the same effect upon behavior. To illustrate, we may consider the relation between degree of muscular tension (an index of excitation) and efficiency of performance in mental tasks. Several investigators have reported that increased efficiency accompanies certain increases in tension. Our data,[25] to be reported in a subsequent article, indicate that, for our group of subjects as a whole, there was a moderately close relationship between high tension and poor performance. But there were instances, both here and in our earlier study, in which individuals with a relatively high tension score gave an excellent performance. The explanation for this, as suggested in the earlier publication and supported by subsequent data, is that excellence of performance depends not alone upon degree of tension but also upon ability to coördinate reactions. Coördination becomes more difficult under high degrees of tension, and hence we find in the majority of cases (at least with young children) that high tension decreases efficiency. But, when an individual attains good coördination under high tension, his performance may surpass that of his own normal level as well as that of other individuals with a lower degree of tension. We further suggested, and this seems increasingly probable, that differences in ability to coördinate reactions might be revealed by the type of tension tracing with respect to regularity or smoothness of line. Our best performer in the earlier experiment was a subject with a relatively high but smooth tension line. This evidence illustrates the point that, in discovering an individual's tendencies with respect to degree of muscular tension, we obtain extremely valuable information, but not information which may be interpreted without other considerations when we attempt to use it for a prognosis of behavior.

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[25] These data, together with the interpretation outlined below (which suggests an explanation for the lack of consistency of results in this field of experimentation), were first presented by the writer to the New York Branch American Psychological Association, April 18, 1931.

We return now to the original question—the value of the measurement of muscular tensions as a technique for the study of emotional tendencies. It is obvious that increases in tension of the skeletal muscles have a wider occurrence than in states called 'emotional.' But until we formulate a more precise definition of an emotion, degree of muscular tension will constitute an excellent and easily available index of excited 'emotional' states. In common parlance we call those individuals 'excitable' who respond with intensity (or show strong dynamogenic effects) to 'emotional' stimuli which cause little or no response in other individuals. If such reactions occur with relatively great frequency and intensity we call the individuals manifesting them 'unstable' or 'poorly adjusted.' It is not surprising that we found a rather high correlation between average degree of tension of the muscles and ratings on excitability, stability, and degree of adjustment to the nursery school.

Although it may not be possible to offer a final solution for the theoretical problems suggested here, it is practically useful to determine which individuals manifest frequently a highly aroused state. A tendency of this sort might indicate an environment that was uncongenial or threatening, or it might indicate firmly established habits of reaction in the individual, modified only slightly by specific environmental circumstances. Habitually high degrees of tension of the muscles serve to reveal individuals who are living under conditions of strain, effort, or 'over-reactivity.'

RELATIVE EFFECTS OF DETERMINATE AND INDETERMINATE *AUFGABEN*

By D. W. Chapman, Harvard University

The psychologist who recognizes the obligation of describing experience in systematic terms has always, sooner or later, had to embark upon the analysis of certain typical phenomenal fields which he considered to be representative of experience at large. This necessity is as obvious and compelling to the configurationalist as it ever was to the structuralist; but with the passage of time, and with the accumulation of experimental evidence, we can point more confidently to errors which inhere in certain methods of analysis. Indeed, we may consider these errors themselves as proper objects of scientific interest, inasmuch as around them must operate the modern methodological reorientation.

If a given conscious complex was to be analyzed, the older psychology proceeded confidently by coercing its part-contents, a, b, c, d, e, successively into the focus of attention, and by there describing them under favorable conditions of clearness. In symbolic terms, this operation consisted in the establishment (under constant stimulus-conditions) of consciousnesses representable as *Abcde*, *aBcde*, *abCde*, etc., wherein a capital letter indicates the attentional focality of a part-content. The focal terms were then collected in the form $A+B+C+D+E$, and this aggregate was taken to represent the complete analysis of *abcde*.

But this operation is justifiable only if we assume that A and a, B and b, C and c, etc., are identical. Certainly we know that in respect of clearness they are not identical; for it is precisely by a change in clearness-value that they enter or leave the focus of attention. Moreover, we are acquainted with common instances (the analysis of clangs and flavors) where such a shift of attention appears to invite the emergence of actually new qualities, by no means positively identifiable with the just previous experience. Since in the experimental situation such shifts of attention are the effects of

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changes in the *Aufgabe* with which the observer approaches the stimulus-presentation, we may state the problem met by the analytical approach to be the following: *Instead of finding, for a constant stimulus-presentation, one experience over which attention can 'sweep' analytically, we must suppose that for this constant presentation there are* n *possible phenomenal fields, each with its particular attentional structure, and each determined primarily by one of* n *corresponding* Aufgaben. *There is hereby given the possibility that reportable aspects of one field may occur only in vaguely reportable form, or not at all, in another field.*

Of the experimental investigations which have dealt with the demonstration and measurement of this effect, two especially deserve recognition.

The first is that of Külpe[1]—the abstraction experiment—wherein he presented tachistoscopically to his *Os* colored, printed syllables, about which could be reported the number of letters involved, the colors involved, the positions of the colors, or the nature of the total figure which they composed. In some presentations the *Aufgabe* was made indeterminate, and, only after the *O* had reported such characteristics as he chose, was he required to report on the remaining ones; in other presentations the *O* was predetermined for certain characteristics, reported on them, and then subsequently was asked about the others. Külpe found a universally higher reportability for aspects of the stimulus favored by the *Aufgabe* (*i.e.* the preëxposure *Aufgabe*) than for aspects not so favored; and he announced the general law that "the most numerous, most correct, and most definite reports occur in those cases where report and *Aufgabe* correspond [wo die Aussagen mit den Aufgaben zusammenfallen]."[2]

The second study is that of Yokoyama, reported by Boring.[3] Yokoyama presented his *Os* tachistoscopically with pairs of rectangles of light, which he varied in intensity and length; and the *O* was required to make comparative judgments with respect to these attributes. "In one series the *O* was predetermined to observe relative intensity. He was to pay attention to intensity only and to judge the one stimulus with respect to the other. The instruction was vigorous, the sensory difference was small, and the task was continued for many observations. Altogether the *O* may be supposed to have been as definitely predisposed for intensity as he could have been. At irregular intervals in this series, however, and not oftener than once in fifty trials, nor more than twice in a session, the *E* broke in, immediately after the exposure and often before the judgment of intensity could be voiced, with a secondary instruction: 'How about extent?' The *O* was then expected to give, *ex post facto*, a judgment of relative extents. In other series the primary determination was for extent, and intensity was called for at infrequent intervals." The conclusion reached, with respect to the phenomenon of diminution of reportability, was that "it is plain that the original perception under the primary determinant is inadequate to a judgment of any difference other than the attribute upon which the primary determinant is directed."[4]

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[1] O. Külpe, Versuche über Abstraktion, *Ber. ü. d. I. Kongr. f. exper. Psychol.*, 1904, 56-68.

[2] *Ibid.*, 11.

[3] E. G. Boring, Attribute and sensation, this JOURNAL, 35, 1924, 301-304.

These two studies establish the general principle that the phenomenal field initiated by a stimulus-presentation does vary in the adequacy of its several aspects, and that this variation depends upon the predetermination of the *O.* Neither of them, however, appears capable, in its technique, of measuring the phenomenon in a satisfactory way.

In Külpe's experiment, we should observe that the secondary, "surprise" *Aufgabe* was most often given *O* only after he had had a sufficient length of time for a report on the primary *Aufgabe.* This means that if we call for a report upon aspect A in a given phenomenal field, and later call for a report on aspect B of the same field, and our results show that reports on B are less accurate than those on A, we shall not know which of the following cases is true: (1) that B was less distinct than A in the original perceptual field, because B was not predisposed-for; (2) that B was less adequately reported because, between the times of report on A and report on B, forgetting with respect to B had taken place; and (3) that B was less adequately reported because the intervening act of reporting A tended to inhibit or modify surrogates of B.

Külpe took case 1 to be the fact. Rubin,[5] however, has argued case 2; and Wilcocks,[6] following this suggestion, has shown by an experiment, close to Külpe's in detail, that where a question is asked corresponding to the predetermination, but some time after presentation, the correctness of report on this question may be lower than for a question asked more quickly after presentation and not favored by the predetermination. The phenomenon of rapid forgetting appears therefore to cut across the phenomenon which Külpe hoped alone to demonstrate.

Yokoyama's experiment probably avoided this error to a greater extent, for the surprise-*Aufgabe* was given often before the report in respect of the primary *Aufgabe* could be made. But Yokoyama's technique, since it requires such infrequency of secondary *Aufgaben* as genuinely to surprise O, has the disadvantage of affording only two cases in an experimental hour. It does not, in brief, lend itself to quantitative treatment.

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[4] *Ibid.*, 303.

[5] E. Rubin, Bericht über experimentelle Untersuchungen der Abstraktion, *Zsch. f. Psychol.*, 63, 1913, 386.

[6] R. W. Wilcocks, An examination of Külpe's experiments on abstraction, this JOURNAL, 36, 1925, 324-340.

The present experiment sought to reduce these difficulties in the following way.[7] We can present to O stimulus-patterns (arrangements of letters) about which a number of types of report (in this case three: number, names, location) can be made. We can, in one series of presentations, give a definite *Aufgabe* before each exposure, and obtain reports under that condition. We can then present a second series to O in which the definite *Aufgabe* is given him only very quickly after each exposure. His *Aufgabe* in this case (at least in the long run) can be considered as relatively indeterminate at the time of stimulus-exposure. We shall expect, if we score for both series percentages of material correctly reported against numbers of stimuli simultaneously presented, that the results will approximate the $\Phi(\gamma)$ relation, as Fernberger has demonstrated;[8] and we shall be able to obtain a 'cognitive' limen[9] for each of the *Aufgaben* under each of the two conditions of determinate and indeterminate instruction. Comparision of the two sets of limens will then furnish us with a measure of the loss in reportability which the latter type of instruction effects.

This procedure offers the following advantages over the procedures of Külpe and Yokoyama: (1) The interval of time between the exposure and the giving of the surprise-*Aufgabe* can be kept reasonably short and constant.

(2) In no case will the necessity arise of making one report before responding to this surprise-*Aufgabe*.

(3) A sufficient number of cases of report with surprise-*Aufgabe* can be obtained to make their statistical properties comparable with those obtained from *Aufgaben* preceding the exposure.

(4) The factor of surprise involved in the *Aufgabe*-after-exposure will tend to remain constant, since O will act, practically, with knowledge of the experiment, *i.e.*, that one of three tasks is to be required of him.

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[7] The writer wishes to acknowledge his indebtedness to Professor C. C. Pratt, under whose guidance this study was performed.

[8] S. W. Fernberger, Range of visual apprehension, this JOURNAL, 32, 1921, 121-133.

[9] A. D. Glanville and K. M. Dallenbach, Range of attention, *ibid.*, 41, 1929, 207-236.

The experimenter realizes, of course, that in adopting this technique, he sacrificed the possibility of determining what happens to the reportability of characteristics when *O* is set maximally to observe other characteristics. He believes, however, that his procedure enabled him to investigate in a far more reliable way the same effect that Külpe pointed to, namely, the difference between reports which are definitely demanded before the exposure of material, and reports which are definitely demanded only *ex post facto*. This experiment deals, then, with reports made under an *Aufgabe* intermediate between definite determination toward a critical aspect and definite determination away from that aspect.

METHOD

Stimulus-material. The stimulus-cards used in this experiment consisted of squares of white, unglazed drawing paper, 7 in. on a side. A centered rectangle was ruled off on each card in pencil, 3 3/4 in. wide and 3 1/8 in. high; and this rectangle was then ruled into five 3/4-in. columns intersecting five 5/8-in. rows, thus giving 25 small rectangles, 3/4 by 5/8 in. each.

The stimuli—black printed letters, 1/2 in. high, cut from Dennison's gummed alphabet no. 31—were then pasted into various of these rectangles, and the penciled rulings were removed with art gum. The letters were arranged on the cards haphazardly, except for the following rules: (1) all letters used were capitals; (2) in order to prevent the formation of syllables, no vowels were employed, and Y was considered a vowel; (3) M and W, because of their extraordinary breadth in this alphabet, were omitted; (4) the letters were always arranged in a line such that successive members were vertically, horizontally, or diagonally adjacent—this was done in order to minimize subjective sub-grouping; (5) no two cards bore the same arrangement of letters; and (6) all the letters appeared in the course of the experiment with approximately the same frequency.

The preliminary work showed that, for the method of constant stimuli, it would be advisable to work in the range of 4 to 8 stimulus-objects. *E* therefore made 18 cards bearing 4 letters each, 18 cards bearing 5 letters each, and so on, up to and including 8 letters,—a total of 90 cards.

Apparatus. The stimulus-cards were presented in a moderately darkened room by means of a modified Dodge tachistoscope. The tachistoscope was essentially a light-proof box, provided with an observation slit to which *O* held his eyes during exposure. The stimulus-card was held within a dead black frame 18 in. before *O*'s eyes. Between his eyes and the card, a sheet of glass set at 45° to the line of regard reflected a moderately bright white field so as to make it appear coincident with the stimulus-card. This constant field entirely hid the letters on the stimulus-card until the latter was illuminated for an instant (by a beam of light reflected off the glass on the opposite side to the constant field), during which instant the black stimulus-letters stood out on the constant white field. The illuminating mechanism was a rotating-disk episcotister, revolving in front of a projection lantern, and fitted with a variable slit to give exposures of from 10 to 60 σ. Since this episcotister involved a fast and a slow disk, geared together, it gave one flash of light every 1.2 sec., if the projection lantern were continuously illuminated. A single flash could be obtained by turning on the lamp somewhat before the coincidence of the slits, and turning it off immediately after. This was made possible by a timing light which flashed on *E*'s control-desk. The only noise incident to the operation of the apparatus was the gentle humming of the constant-speed motor which drove the episcotister.

The instructions for each exposure were given *O* by means of an instruction-box, the front of which bore three small milk-glass windows, on each of which was printed an *Aufgabe* (count, locate, name). Keys at *E*'s desk enabled him to light a lamp behind the window bearing the instruction for any given exposure. The instruction-box was placed outside the tachistoscope and somewhat to the right of *O*.

A warning signal was furnished by a telegraph sounder.

A narrow board, fixed to the edge of the table in front of the tachistoscope, provided a desk on which *O* could write his report. The desk was illuminated by an overhead light which *E* could switch on immediately after each exposure.

Procedure. The experiment was performed from the beginning of November, 1929, through March, 1930.

The *O*s were 6 in number: Dr. C. L. Goldthwait (*G*), Messrs. W. H. Hunt (*H*), M. H. Lewis (*L*), K. W. Oberlin (*Ob*), and Misses H. Nye (*N*), and J. I. Steinberg (*St*). Excepting *N*, an undergraduate in philosophy with some psychological training, the *O*s were Harvard and Radcliffe students of psychology; the 4 men were graduates.

Each *O* observed for one experimental hour a week. For *G*, *H*, and *L*, the tachistoscope was adjusted to give a length of exposure of 10σ. *St*, however, had difficulty with this length of exposure, and after the practice-series, was given exposures of 30σ, as were also *N* and *Ob*.

During each experimental hour, each *O* was given 90 exposures of the stimulus-cards, arranged in haphazard order with respect to the number of stimuli which each card bore. Forty-five of these exposures formed a 'P-series,' in which the *Aufgaben* were given before each exposure; and the remaining 45 exposures formed an 'F-series,' in which the *Aufgaben* were given immediately following each exposure. To minimize practice-effects, the P-series was given before the F-series in one experimental hour, and after the F-series in the succeeding experimental hour; and this alternation of order was maintained throughout the experiment.

Instructions. Before he observed the P-series, *O* was given the following instructions to read:

> In this experiment you will be presented with a tachistoscopic visual field containing a number of letters of the alphabet. Before each exposure the instruction-box will indicate one of the following three types of report, which you are to give after the exposure:
>
> "Count." In this case you are to report the number of letters seen, by writing that number on the report slip; for example, 7.
>
> "Locate." In this case you are to reproduce with small circles (each circle representing a letter) the relative position of all letters seen; for example,
>
> o o
> o o o o o
> o
>
> "Name." In this case you are to print out in a horizontal line all the letters seen, irrespective of their positions; for example, F X K C L

After you have determined the instruction from the box, place your eyes to the eye-piece and fixate the bright, square field near its center. The exposure will occur 2 sec. after the telegraph sounder clicks.

For the F-series, *O* was given the following instructions:

In this part of the experiment the procedure will be the same, except that you are to take your instructions from the box immediately *following*, instead of preceding, the exposure."

For each exposure in the P-series, the procedure was as follows, the times at the left indicating flashes of the timing light on *E*'s desk:

Time	*Operation*
	O signifies "ready."
0 sec.	Outside light turned off.
	Instruction-box turned on.
2 sec.	Warning signal given.
	Instruction-box turned off.
4 sec.	Exposure occurs.
	Outside light turned on.
	O writes report.

For the F-series, the procedure was as follows:

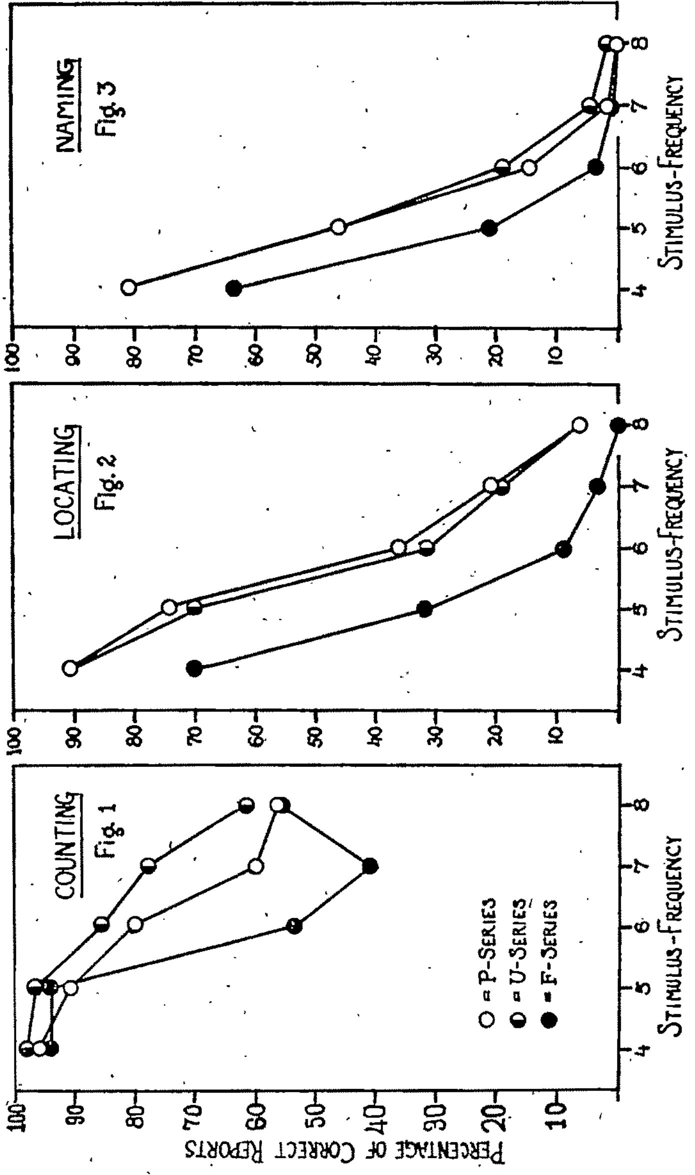
	O signifies "ready."
0 sec.	Outside light turned off.
2 sec.	Warning signal given.
4 sec.	Exposure occurs.
	Instruction-box turned on.
	Outside light turned on.
	O writes report.

In order to determine whether in the P-series the 4-sec. interval between the lighting of the instruction-box and the occurrence of the exposure had been sufficient to allow *O* to establish a firm and definite *Einstellung*, *E* conducted a 'U-series' over 3 more experimental hours. During the first of these hours *O* did nothing but locate the stimuli on 75 cards presented him; in the second hour he did nothing but name, and in the third hour nothing but count. This series was used as a control to determine whether an *Einstellung* maintained over an experimental hour was significantly more firm than one established only shortly before each exposure in the P-series.

RESULTS

The *O*s' reports were scored right or wrong. Table I gives the total number of correct reports made by each *O* (all 5 stimulus-frequencies pooled) for each *Aufgabe*, and arranged according to mode of presentation of *Aufgabe* (columns P, F, and U). Differences between the results of various pairs of series are tabulated in the columns headed D_{F-P}, D_{F-U}, D_{U-P}.

Two generalizations are apparent, and are exhibited in most striking form by the results for locating and naming: (1) the results from the P- and U-series approximate each other; and (2) the number of correct reports in the F-series is distinctly lower than in the P- or U-series. The results from counting are somewhat less univocal, and we shall note the probable reason for this presently.



FIGS. 1-3. SHOWING THE PERCENTAGES OF CORRECT REPORTS FOR EVERY INSTRUCTION AND FOR EVERY EXPERIMENTAL SERIES

TABLE I.

TOTAL NUMBER OF CORRECT REPORTS (OUT OF 75) MADE BY EVERY *O* FOR EACH OF THE P-, F-, AND U-SERIES, FOR EACH *AUFGABE.*

	O	P	F	U	D_{F-P}	D_{F-U}	D_{U-P}
Counting	*G*	56	44	50	−12	− 6	− 6
	H	49	47	51	− 2	− 4	2
	L	54	43	63	−11	−20	9
	N	66	59	70	− 7	−11	4
	Ob	61	53	69	− 8	−16	8
	St	60	59	75	− 1	−16	15
	Total	346	305	378	−41	−73	32
Locating	*G*	28	18	32	−10	−14	4
	H	38	18	27	−20	− 9	−11
	L	30	15	25	−15	−10	− 5
	N	32	12	33	−20	−21	1
	Ob	50	24	42	−26	−18	− 8
	St	29	16	38	−13	−22	9
	Total	207	103	197	−104	−94	−10
Naming	*G*	16	12	11	− 4	1	− 5
	H	18	13	17	− 5	− 4	− 1
	L	14	12	17	− 2	− 5	3
	N	25	15	29	−10	−14	4
	Ob	29	18	27	−11	− 9	− 2
	St	28	10	37	−18	−27	9
	Total	130	80	138	−50	−58	8

The data for all 6 *O*s are pooled and presented graphically in Figs. 1-3, in the form of unsmoothed curves. Here the relative inefficiency of report in the F-series is apparent, as is also the approximation of the P- and U-series for the *Aufgaben,* locating and naming.

Since the curves for locating and naming are free from inversion and strongly suggest the ogive function, their limens were calculated from Urban's tables, and found to be as follows:

		L[10]	*h*
Locating	P-series	5.80	−0.509
	U-series	5.49	−0.506
	F-series	4.54	−0.594
Naming	P-series	4.91	−0.684
	U-series	4.98	−0.555
	F-series	4.28	−0.700

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[10] L is expressed in stimulus-units.

These limens would seem to offer themselves as acceptable measures of the efficiency of report characteristic of the three series.

The curves obtained from counting are not suitable for a calculation of limens, chiefly because of their tendency to inversion at a stimulus-frequency of seven. An analysis of the reports revealed that the *O*s called 6 stimuli 'seven' more often than 'five.' They soon learned that 8 was the terminal frequency in the range, and this tendency to identify the larger groups of stimuli as 'eight' appears to have resulted in an abnormally large percentage of correct reports on 8 stimuli—an artificiality of the experiment which presumably would not have occurred had the range of frequencies been larger.

We may conclude, in fine, from the quantitative data that the efficiency of report for all tasks is lower under an indefinite *Aufgabe* than under a definite instruction. We may also conclude that the efficiency of report under a definite *Aufgabe*, established 2 sec. before the exposure of the stimulus, is approximately the same as the efficiency of report under a definite *Aufgabe* carried throughout an experimental hour.

Introspective reports taken during the final experimental hour show that the better reportability of the P-series partly resides in perceptual differences in the phenomenal field following definite *Aufgaben*:

St: "When carrying the instructions for locating, I attempted to adopt a set for the total configuration of the field. The whole pattern then became stamped in so that I could still see it while reporting. In naming, the set was for a less organized pattern, so that the parts could be seen as lesser units. In counting, the set was to adopt a loose organization, so that the individual letters easily abstracted themselves."

G: "During the exposure it [the phenomenal field] was the same for locating and counting, but for naming the details of the letters stood our more clearly."

N: "With the shorter groups of stimuli, all three characteristics were clear [number, position, and identity]. As for the larger groupings: in counting, the other characteristics were vague at most; in locating, the number of letters was usually clear; and in naming, I concentrated so hard on the individual letters, that the group as a whole was vague."

L: "For the numerical instruction, it [the phenomenal field] had quite a discrete character; for locating, a definite geometrical character; and for naming, it had a combination of the two, with greater differences in the value of the parts."

H and *Ob* found no differences in the structure of the field for differing preceding *Aufgaben*.

Our evidence therefore permits us to say that, in general, an *Aufgabe* given before the presentation of a field tends to emphasize in that field perceptual characteristics which will facilitate the report which it demands. Further introspections add the fact that where this accentuation is found during the exposure of the material, it persists in subsequent surrogative processes, and may even increase, until the report has been made. This effect of a previous *Aufgabe* is therefore common both to the immediate phenomenal field and to its surrogates; and for this reason, we cannot speak of efficiency of report as revealing solely differences of structuring in the original phenomenal field, without admitting the possibility that this efficiency is also governed by structural changes which have taken place in the surrogates intervening between the original field and report.

In the F-series, on the other hand, the typical field and its surrogates have a less univocal structure, corresponding to the neutral *Aufgabe* with which the presentation is approached. When the subsequent definite *Aufgabe* impinges upon this surrogative mass, there is an attempt on the part of *O* to restructure the surrogate along dimensions which are essential for report—and it is apparently the difficulty of this process which is reflected in the lesser accuracy of report in the F-series:

G: "I think the difference [in accuracy of report] was due to the necessity of carrying the mass of unnecessary imagery [in the F-series]. It seemed to be beyond my capacity to hold so much with a clearness anything like that possible when the instruction was given previous to exposure."

St: "In making the report [in the F-series] it was often necessary to change the configuration of the field. Difficulties resulted, in which there was a confusion of the three characteristics with location losing the most strength, and naming and counting following."

N: "I should say that it [the lesser accuracy] was due simply to the confusion resulting from counting, locating, and naming all at the same time."

G: "Considerable tension from moment of exposure until instructions perceived. Confused mass of visual, auditory and voci-kinaesthetic imagery held in suspension. When instructions perceived, a pushing out of unnecessary material and attempt to strengthen wanted material. (I do not think the latter ever became clearer, but there was always the effort to cause it to.)"

The picture which these considerations allow us to draw of the relative functions of *Aufgabe*-before-exposure and *Aufgabe*-after-exposure, may be put as follows: In the P-series, the *Aufgabe* initiates a process whereby perceptual characteristics of the stimulus are emphasized according as they are demanded by the *Aufgabe*. The *Aufgabe* may thus be thought of, figuratively, as providing a preliminary matrix to the structural values of which succeeding experience tends to conform. Its first effect is felt in the perceptual field incident to the exposure of the stimulus. This process of emphasis proceeds during the successive surrogative experience; and the fading out of irrelevant aspects of these images represents an enhancement of the same effect in the original phenomenal field. At whatever temporal stadium in this process the giving of the report begins, it is conditioned by a surrogative mass which shows as its principal aspects precisely those which contribute to the accuracy of report.

In contrast to this, for the F-series, the immediate perceptual field, being preceded by no exclusive *Aufgabe*, tends to exhibit, more or less equally, all the aspects which might be demanded by any of the *Aufgaben* which the *O* knows he may receive. The clearness of these coexistent aspects is diminished by their rivalry. This condition persists in the subsequent surrogates. When the *Aufgabe* is received, there ensues a tendency for this surrogative mass to restructure itself according to the matrix given; but this restructuring is relatively incomplete, and brings with it a decreased liveliness of the surrogate as a whole. The report is then, in this case, conditioned by a less univocal surrogative structure, and loses thereby in accuracy.

CONCLUSIONS

In comparing reports in counting, locating, and naming groups of 4 to 8 stimuli presented tachistoscopically in each of the following two series, P-series, wherein the *Aufgabe* was given before presentation of the stimuli; and F-series, wherein the *Aufgabe* was given immediately after presentation of the stimuli, the experimenter finds:

(1) A lower accuracy of report for the F-series than for the P-series;

(2) That the comparison of cognitive limens gives a convenient and practicable index of this difference of accuracy of report;

(3) That this difference is apparently due to (a) the tendency of both perceptual field and succeeding surrogates in the P-series to accentuate aspects demanded by the *Aufgabe*, and (b) the tendency of both perceptual field and succeeding surrogates in the F-series toward an indifference or confusion of aspects, and a resistance to the accentuation demanded by the *Aufgabe;*

(4) That, since the coöperation of both perceptual field and succeeding surrogates is indicated, (a) the hypothesis that the *Aufgabe* in part determines reportable aspects of phenomenal fields is supported, and (b) the hypothesis that efficiency of report measures exclusively the structure of the original perceptual field, apart from its surrogates, is not sustained.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXXX. POSITION *VS.* INTENSITY AS A DETERMINANT OF THE ATTENTION OF LEFT-HANDED OBSERVERS

By ANNIE MELL WHITE AND KARL M. DALLENBACH

In the various studies on the determinants of attention published from this laboratory,[1] spatial position was found to be an important factor. The position to the left of the point of visual fixation had, for all the *Os* in all of the experiments, a marked attentional advantage. Since all the *Os* were right-handed, the effectiveness of the left-hand position was explained neurologically as follows.[2] Neural excitations aroused in the right retinas of each eye—the areas stimulated by objects occupying positions to the left of fixation—center in the right hemisphere of the brain where they, because right-handed people are left brained, are comparatively free from interference and inhibition by other concomitant processes. As a consequence they possess on the side of consciousness a high degree of attensity, *i.e.* clearness or vividness. Everything else being equal, objects arousing processes in the right brain would enjoy, therefore, an attentional advantage over all others.

This explanation was given added plausibility when Burke and Dallenbach extended the experiments to left-handed *Os*, for they found that the right-hand position had an attentional advantage for the left-handed.[3] They used, however, only 2 *Os*, and only one of these was, according to Rife's classification of dextrality, of the pure LLL type.[4] Their work, therefore, clearly needs confirmation.

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[1] L. G. Meads, Form *vs.* intensity as a determinant of attention, this JOURNAL, 26, 1915, 150-151.

J. N. Curtis and W. S. Foster, Size *vs.* intensity as a determinant of attention, *ibid.*, 28, 1917, 293-296.

A. M. Bowman, Size *vs.* intensity as a determinant of attention, *ibid.*, 31, 1920, 87-90.

K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 205, 213, 229; Position *vs.* intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-286.

Doris Dewey and K. M. Dallenbach, Size *vs.* intensity as a determinant of attention, *ibid.*, 35, 1924, 121-125.

C. L. Friedline and K. M. Dallenbach, Distance from point of fixation *vs.* intensity as a determinant of attention, *ibid.*, 41, 1929, 464-468.

[2] Dallenbach, *op. cit.*, this JOURNAL, 285. Cf. also H. A. Cohn and J. W. Papez, A comparative study of the visuosensory or striate area in the two hemispheres of the human brain, *Amer. J. Phys. Anthrop.*, 14, 1930, 413 f.

[3] R. S. Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left-handed observers, this JOURNAL, 35, 1924, 267-269.

[4] J. M. Rife (Types of dextrality, *Psychol. Rev.*, 29, 1922, 474-480) found 3 distinct types of right-handedness, and 3 corresponding types of left-handedness, making 6 forms of dextrality in all. He determined these types from the use of the hands in unimanual activities, such as writing, sewing, throwing, and hammering,

The object of the present study was to test the neurological theory further by determining the positional preference of other left-handed *Os*. As in the earlier experiments, every condition of attention except intensity and position was eliminated, and the effect of position was measured in terms of intensity.

Observers. The *Os* were Miss Ethel Shaw (*Sh*) and Mr. Benedict Newell (*Ne*), seniors in Cornell University, and Mr. C. W. Fox (*Fx*), graduate assistant in psychology.

We were unable to obtain *Os* of the pure LLL type. *Sh* is left-handed in writing, eating, throwing, hammering, and in using the scissors, bat, ax and spade, but she uses a pitchfork and broom right-handed and is right-eyed. She clearly belongs to the LLR type. *Ne* is left-handed in all the unimanual activities except writing and eating which he now does right-handed as a result of insistent training when young. He plays tennis, bats, and wields an ax left-handed, but uses a pitchfork and shovel right-handed. He is right-eyed. He too, we believe, belongs to the LLR type. *Fx* is left-handed in throwing, hammering, in using the scissors, ax, spade, shovel, pitchfork, and broom. He is left-footed in kicking a football and in jumping, and is left-eyed. He is, however, right-handed in writing, eating, batting, and playing golf. He clearly does not fall within any of Rife's classes. His formula seems to be $^{R}_{L}$RL. He was experienced in psychological observation; the other two *Os* were naïve. All of them, however, served in the experiment without knowledge of the specific problem.

Apparatus. The apparatus was similar to that used in the earlier studies.[5] *O* sat in a light-proof booth with his head held firmly in position by means of a biting-board which was so adjusted that the fixation-point, 1 m. away and 1 mm. in diam., would fall, if projected, midway between his two eyes.

The comparison areas, two circular openings each 2.5 cm. in diam., were exposed in the horizontal position, 12.5 cm. to the right and left of the fixation-point. Only the horizontal position was used in this study. Two series of experiments were performed. In Series A the standard intensity was at the right; in Series B it was at the left. These series are comparable to the experiments in positions 1 and 5 of the earlier studies.[6] The omission of the other positions, the vertical and the diagonals, permitted the inclusion of more individual experiments with every *O* at each of the positions used.

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and in bimanual activities, such as batting, sweeping, spading, and chopping with an ax. Handedness was determined in the bimanual activities by the hand "nearer the business end of the instrument." Some individuals show a bimanual preference opposite that of their unimanual; and others a divided preference. If the right hand is preferred in both unimanual and bimanual activities the individual is classified RRR; if the left hand is preferred he is classified LLL. If the right hand is preferred in unimanual activities, and the left hand in bimanual, the individual is classified RLL. If an opposite preference obtains the formula is LRR. If an individual is right-handed in unimanual activities but has a divided preference in bimanual, *e.g.* bats right-handed but sweeps left-handed, he is classified RRL. If an opposite preference obtains the formula is LLR. Cf. also J. E. Downey, Types of dextrality and their implications, this JOURNAL, 38, 1927, 317-367.

[5] Dallenbach, Some new apparatus, *ibid.*, 34, 1923, 94 f.

[6] Dallenbach, *ibid.*, 283; Burke and Dallenbach, *op. cit.*, 267.

Exposure was made by illuminating the comparison areas from behind. The duration of the exposure was controlled by a gravity tachistoscope, which was set for 1/10 sec., but the actual time used was, as measured by a Hipp chronoscope, 106 ± 1.86 sigma. The light was derived from a 500-w. concentrated filament mazda lamp, which was placed in a projection lantern behind the tachistoscope. The rays of light when uninterrupted by the tachistoscope passed through a light-proof funnel to the posterior frame of a diffusion chamber and thence, through circular windows and connecting tubes, to the ground glass windows in *O*'s booth. The intensity of the illumination was controlled by metal shutters or screens which varied the amount of light allowed to pass through the connecting tubes. One hundred screens, containing from 1 to 100 holes, every one 5 mm. in diam., and concentrated and symmetrically arranged about the center, were used to vary the intensity of the illumination. The number of holes in a screen was used to designate the intensity-value of that screen. The intensity of the standard area was, with the exception of the experiments of Series B for *Fx*, placed at 16, *i.e.* the intensity of illumination from the screen with 16 holes in it. The standard for *Fx* in Series B was 4; this shift in the standard intensity had to be made in order to bring the variable intensities required for this *O* in this series of experiments within the intensity range of our apparatus. The intensity of the variable area could be changed from 1 to 100, *i.e.* the intensity of illumination from screens with 1 to 100 holes in them.

Instructions. The following instructions were given the *O*s at the beginning of every experimental hour.

> At the signal 'Ready,' fixate the spot of light before you. At 'Now,' two areas of light will be shown. Judge which of them, if either is the more attense; that is, which of them catches your attention the more. Give the position as 'right' or 'left,' whichever the case may be. If they are equal, say 'same;' if you are uncertain, report 'doubtful.'

These instructions are similar to those given in the earlier studies. In order to assist the *O*s in establishing criteria of attensity, the following was added.

> Do not base your judgments of attensity upon intensity or form. Report which of the two stimulus-areas catches your attention first, which one stands out, is the more vivid, insistent, or clearer.

Procedure. Two series of preliminary experiments, one for each position of the standard, were made by the method of limits to determine for every *O* the approximate intensity of the variable necessary for a judgment of equality. Upon the basis of the results of these experiments 5 stimulus-values were selected for every *O* for each position of the variable, and the main experiments (Series A and B) were conducted by the method of constant stimulus differences. In each series of experiments 100 observations were taken for every *O* at every stimulus-value. The two series were conducted concomitantly in haphazard order. The *O*s never knew, therefore, which stimulus-area was the standard and which the variable. They had to judge every experiment separately and solely upon the basis of relative attensity or clearness.

When a 'doubtful' judgment was returned—and only a few in spite of the difficulty of the task were returned— the experiment occasioning it was, without *O*'s knowledge, immediately repeated.

Results. The results of this study are shown in Table I which gives for every *O* for each position of the comparison areas the amount of illumination necessary for the variable to be reported equal to the standard in attensity or clearness. These values were obtained after the 'equal' judgment had been divided between the judgments of 'right' and 'left.' Then the 50% point, the point at which *O* was balanced between the judgment 'right' and 'left,' was computed by the constant method from one of the percentile arrays.[7]

TABLE I

SHOWING FOR EVERY *O* AND FOR EACH SERIES OF EXPERIMENTS THE AMOUNT OF ILLUMINATION OF THE VARIABLE NECESSARY FOR THE REPORT OF EQUALITY IN ATTENSITY

O	Series A (left, variable; right, standard)				Series B (left, standard; right, variable)			
	Variable		Standard	Ratio	Standard	Variable		Ratio
	(L)	(h)		(V:S)		(L)	(h)	(S:V)
Fx	2.3	.620	16	2.16	4	61.1	.025	16:244
Ne	76.3	.022	16	67:16	16	2.6	.300	16:3
Sh	34.4	.041	16	34:16	16	0.8	.323	16:1

The data in Table I show that position is effective for all the *O*s, and also that it is effective for them in opposite directions. The intensity of the left stimulus-area is greater for *Ne* and *Sh*, in both series of experiments, than that of the right. This simply means that the left area, because of their predisposition toward the right, must be augmented in order to equal the right in attensity (clearness, vividness). Since both of these *O*s belong to the LLR type of dextrality, *i.e.* are left-handed in the unimanual and in most of the bimanual activities, their results may be regarded as additional evidence for the neurological theory that has been advanced in explanation of the effectiveness of position.

Fx, on the other hand, is predisposed toward the left; the intensity of the right stimulus-area is much greater, in both series of experiments with him, than that of the left. *Fx* is not, however, truly left-handed. He belongs, as we have seen, to an intermediate or mixed ($\mathrm{R_L}$RL) type of dextrality. He is not ambidextrous in the sense that he uses either hand equally well in all activities, but he is ambidextrous in the sense that he performs some uni- and bimanual activities left-handed and some right-handed. His results do not, therefore, stand in opposition to the general conclusion that left-handed people have a preference for the right, for he is not to be numbered among the left-handed. At most, all that his results indicate is that *Os* of the intermediate type exhibit a positional preference similar to that of the right-handed.

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[7] Since the arrays were complementary it did not matter which was used, the same liminal value would be obtained from either. We used, however, in our computations, the array of the 'left' judgments.

The results of this experiment also show that the eyedness of our *Os* does not agree with their handedness, but that it does, curiously enough, agree with their positional preferences. *Ne* and *Sh* are left-handed, right-eyed, and predisposed toward the right; *Fx* is ambidextrous, left-eyed, and predisposed toward the left. Whether the correlation between eyedness and positional preferences, which is perfect for our *Os*, is significant we cannot say. Future experiments alone can determine that. We are, however, of the opinion, in spite of its being perfect for our *Os*, that it is not significant. We come to this conclusion because of the following reasons. (1) Earlier experimenters have found a high correlation between eyedness and handedness[8]—there is a zero correlation for our *Os*. (2) Crossed ocularity types, particularly crossed sinistro-ocularity, are rare, occurring in only 1.7% of Mill's cases[9]—and yet two of our *Os* belong to this type. (3) The eyedness of the *Os* in the earlier experiments performed in this laboratory does not, in so far as we have been able to obtain information concerning it, agree with their positional preferences. (4) Since the retinas of each eye are connected with both hemispheres, it would be difficult upon any known theoretical basis to explain positional preference by eye dominance. We have no right, to be sure, to negate our own results, but here is an instance where the results, we believe, are adventitious; where they are due to an error of sampling.

Summary and conclusions. The results of this study, which corroborate those earlier obtained, may be summarized as follows: (1) All of our *Os* (two are left-handed and the other one ambidextrous or of a mixed, intermediate type) show positional preferences. (2) The position to the right of fixation has an attentional advantage for the left-handed *Os*, and the position to the left of fixation has an attention advantage for the ambidextrous *O*. (3) Eyedness and positional preference agree—the left-handed *Os* are right-eyed and predisposed toward the right, the ambidextrous *O* is left-eyed and predisposed toward the left.

In spite of the high correlation between eyedness and positional preference obtained in this experiment, we do not believe that the relation is significant. It is, we feel, adventitious, being due to an error of sampling. We feel justified, however, upon the basis of the other results of this experiment in concluding: (a) that position is a condition of attensity (clearness, vividness) and a determinant of attention; (b) that the specific positional preference is a matter of the handedness of the *O*—right-handed *Os* being predisposed to the left and left-handed *Os* to the right; and (c) that added plausibility is given to the neurological theory advanced in explanation of positional preference.

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[8] B. S. Parsons, *Left-Handedness*, 1924, 51; Lloyd Mills, Eyedness and handedness, *Amer. J. Opth.*, 8, 1925, 1-9; J. E. Downey, Types of dextrality and their implications, this JOURNAL 38, 1927, 327-333; N. B. Cuff, The interpretation of handedness, *J. Exper. Psychol.*, 11, 1928, 29; Walter Miles, Ocular dominance demonstrated by unconscious sighting, *ibid.*, 12, 1928, 113-126.

[9] Mills, *op. cit.*, 5.

APPARATUS

AN ELECTRICAL PNEUMOGRAPH

By William H. Grubbs and Christian A. Ruckmick, University of Iowa

In connection with our investigations in the field of emotion a need arose for electrically recording changes of breathing. We needed to catch any variations in the breathing processes for two reasons: first, as a check against muscular contractions which also might have left a galvanic record; secondly, because we wished to analyze the breathing curve as an accessory item in the method of expression to run parallel with our introspective accounts. It is well known that even slight movements in the respiratory tract, such as coughing or clearing the throat, leave immediate traces in terms of a galvanometric deflection. This can ordinarily be distinguished because the response is immediate while the galvanic deflection that is due to emotional or affective changes comes only after a latent period. But where a signal has not been recorded for the onset of the affective experience, breathing and other responses have to be checked off from the total record. At present, as far as the authors know, there is no reliable procedure available for photographically recording by electrical means the pneumographic techniques used in conjunction with such an apparatus as the Hathaway device or the Wechsler galvanometer.

We first tried various adaptations of variable wire-wound resistances which could be worn on the chest or abdomen but we soon discarded these even when special types of resistance-wire like small cast bars of nichrome bismuth and other high resistance-metals were used. Such devices either did not slide easily enough or left a jerky record. We finally hit upon the plan of using a carbon button similar to the button found in a telephone transmitter (Fig. 1). This button was mounted inside a fiber case on a thin strip of spring brass (A, A′). The elasticity of the spring tended to increase the ratio of external pressure and the limit of compressibility of the carbon granules (E) inside the button. The diaphragm was formed of a slightly arched disk of celluloid (B) to the center of which was attached a small disk of aluminum (C) with the appropriate electrical connections (F) to complete the circuit within the apparatus. The contact between the carbon button and the diaphragm was softened and made more flexible by a rim of absorbent cotton (D) around the button.

Wires led from this case to a battery containing two 1.5 volt cells and a Wheatstone bridge. From this box wires led to a sensitive milliammeter of 1 milliampere range which for our purposes had the shunt removed and was enclosed inside of the Wechsler photographically recording galvanometer. A small mirror was mounted on the axis of the deflecting pointer in the milliammeter. From this mirror a beam of light was reflected from a small 4.5 volt oscillograph light suitably housed to prevent leakage inside of the galvanometer box.

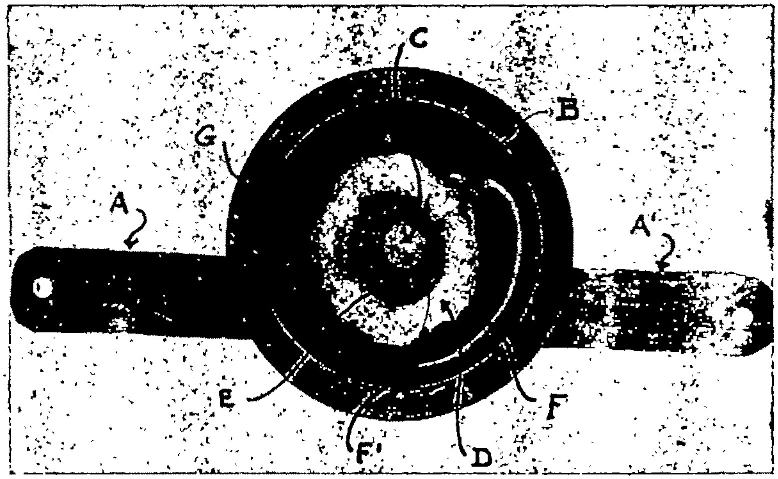

Fig. 1. The Electrical Pneumograph

A,A′, Strips for attaching apparatus around trunk; B, arched disk of celluloid; C, small disk of aluminum; D, rim of absorbent cotton; E, carbon granules; F,F′, wires for electrical connection, coiled at G for flexibility.

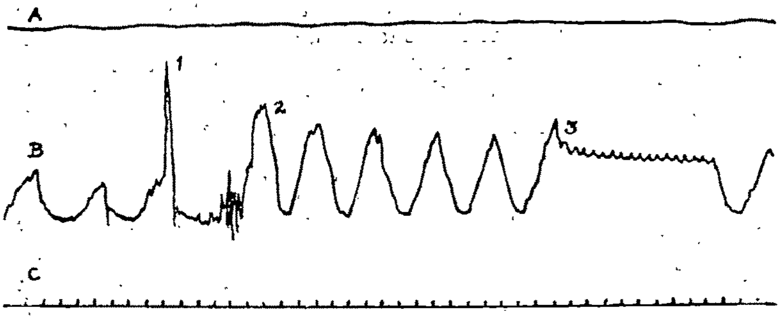

Fig. 2. Photographic Record from the Electrical Pneumograph

A, galvanometer line; B, respiration record: 1, exciting stimulus presented; 2, heart beat showing at crest of wave; 3, heart beat showing while breath is voluntarily held; C, time line in sec.

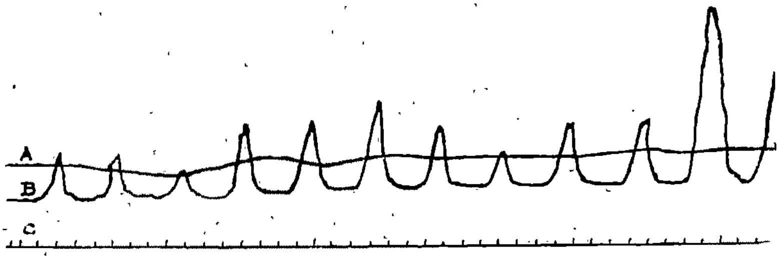

Fig. 3. Changes in Breathing and Galvanic Reflex

A, galvanometer line; B, respiration record; C, time line in sec.

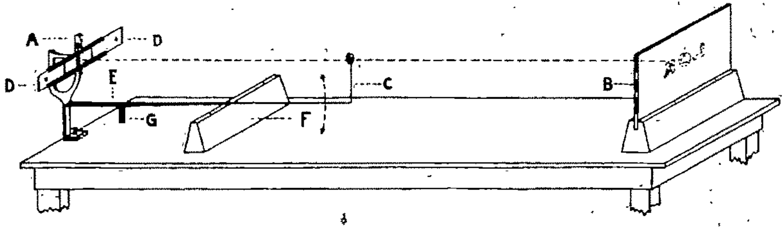

FIG. 4. THE MONOPTOMETER

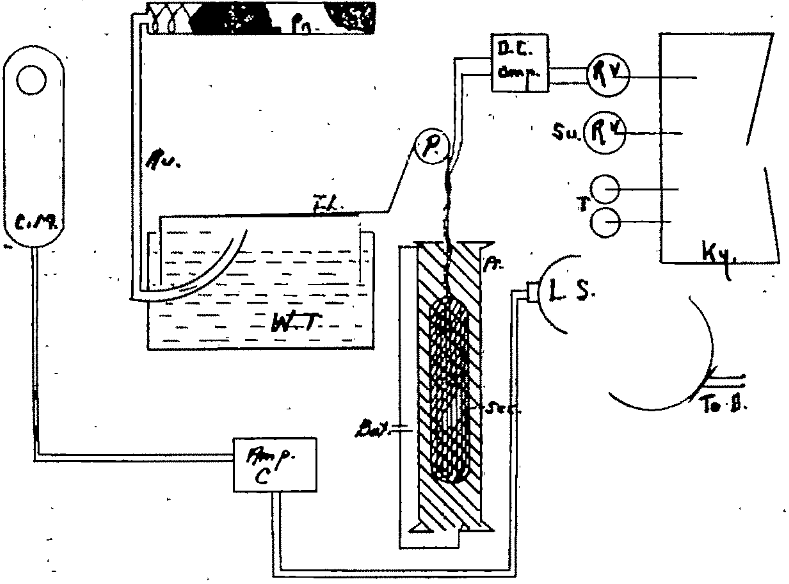

FIG. 5. DIAGRAM OF RECORDING APPARATUS

C.M., condenser microphone; Amp. C., amplifier control; L.S., loud speaker; Pn., pneumograph; Ru., rubber tubing; W.T., water tambour; T.L., tambour lever; P., pulley; Pr., primary; Sec., secondary; Bat., battery; D.C. amp., D.C. amplifier; R.V., recording voltmeter; Su., recording voltmeter to be used in circuit with subject; T., second and minute timers; Ky., kymograph; To D., to dictaphone.

At present we are standardizing the deflection so that the recorded curve is comparable with the ordinary pneumatic principle usually used with existing pneumographs. But our records so far indicate a degree of sensitivity which exceeds that obtained from the present standard equipment (Figs. 2 and 3). We are able indeed to record pulsations from the circulatory system at the crest of each wave (Fig. 2, B 2) and to get a much longer record of heart pulsations when breathing is held (B 3). The instrument can be placed on almost any part of the trunk although the curves differ for the several positions used. Some of our best results, however, were made when it was placed on the floating ribs of the left side.

The entire arrangement is compact, easily portable and is especially adapted to use where the *O* is advantageously removed to a room far from the recording apparatus. For simple arrangements in the drill laboratory and for demonstrational purposes the standard pneumograph will still have points of merit but where careful research is planned and especially where accurate photographic records are desired as happens in so many of our investigations the electric pneumograph ought to find a place in our laboratories. The whole equipment ought not to cost more than $15, not including the Wheatstone bridge.

THE MONOPTOMETER: A NEW DEVICE FOR MEASURING EYE-DOMINANCE

By F. H. LUND, Temple University

The fact of eyedness, *i.e.* dominance of one eye over the other in visually directed movements, seems to have been recognized long before any attempt was made to measure or determine it objectively. As early as 1861, Humphrey referred to "the correspondence of function between hand and eye."[1] Again, in 1881, LeConte called attention to the relation which exists between eyedness and handedness.[2] Since then a number of other writers and investigators have dealt with the subject.

The first attempt to introduce quantitative measures was made by Parson, in 1924, who reported the results of tests in which he used the manoptoscope as his measure of eyedness.[3] This instrument, devised by Parson, consists of a conically-shaped sighting tube and an exposing device which is placed at a distance of about 2 ft. from the *S*. In the center of the exposing device is placed a fixation-disk, with an *R* to the right of the disk and an *L* to the left. If the *S* is right-eyed and fixates the disk with the right eye he will, with the left eye, see the *R*. The *L*, on the other hand, will be seen with the right eye if the *S* fixates the disk with the left eye.

In 1930, Cuff published an account of the manoptometer,[4] which is an improvement upon Parson's manoptoscope, in that the new apparatus permits (a) of a more accurate regulation of the distance between the *S* and the exposing device, and (b) of the regulation of the distance between the fixation-disk and the objects seen by the non-dominant eye. In as much as the sighting tube is retained in its original form, it does not, however, seem to us that Cuff has obviated the chief objections to Parson's device. These objections are (1) the conditions under which the *S* is required to fixate are highly artificial, since vision is limited to what may be seen through the circular aperture, and the *S* cannot be guided—as he normally would be—by objects seen in indirect vision; (2) with vision thus restricted, it becomes largely a chance matter as to which eye first sights, or gets into line with, the object of regard—this being the case, there is a good deal of room for chance errors, particularly in the case of *S*s manifesting mild dominance; and (3) the eyes of the *S* being shielded from view by the sighting tube *E* is unable to tell whether the *S* is following instructions. Moreover, in determining the form of dominance present, the experimenter must depend almost entirely upon the *S*'s own report.

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[1] G. M. Humphrey, *The Human Hand and the Human Foot*, 1861, 8.
[2] J. LeConte, *Sight*, 1881, 107.
[3] B. S. Parson, *Lefthandedness*, 1924, 7 ff.
[4] N. B. Cuff, A manoptometer, this JOURNAL, 42, 1930, 639.

The *monoptometer* devised by us (see Fig. 4) was constructed with a view to meeting these objections. It will be seen from the figure that the circle (C) mounted upon the bar (E) extending from the head-rest (A) is on a level with the slides (D) and the fixation-disk (B). Thus, the *S*, seated at the head-rest, may be instructed to bring the circle into line with the fixation-disk. In doing so the *S* is required to keep both eyes open, and to manipulate the bar (E), and thus the circle (C), by the handle (G), which is grasped with the favored hand. The experimenter standing opposite the circle, is able to follow the operations of the *S*, and can tell immediately, from the point at which the extension bar (E) comes to rest, which is the dominant eye. If it stops on the right side of the median line (a line determined beforehand and marked off on the bar rest (F)), the *S* is right-eyed. If it stops on the left side, the *S* is left-eyed. Through repeated trials in which the *S* is required to bring the mounted circle into line from a position well to the right, or well to the left—the procedure alternating—it is possible to determine both the *kind* and *degree* of eye-dominance possessed by the *S*. If dominance is of the less decided or milder form there is apt to be less consistency in the point at which the bar is brought to rest in successive trials.

Since the apparatus is less a "measure of handedness and eyedness" than it is a "measure of one-eyedness," the term, *monoptometer*, seemed more appropriate than the term, *manoptometer*, used by Cuff.

The slides, (D), are not essential to the experiment. They are introduced merely as a convenient means of demonstrating to the *S* which eye is used in sighting. In the absence of these slides, a book, or card, may be held in front of the dominant eye, so that the *S* can see for himself which eye he is using.

In order to secure a comparative measure of the monoptometer and the manoptoscope, 526 *S*s were tested for eyedness, using first the one instrument and then the other. The monoptometer tests yielded 69.8% right-eyed, 25.5% left-eyed, and 4.6% binocular and ambiguous. The manoptoscope tests yielded 63.7% right-eyed, 20.4% left-eyed, and 15.9% binocular and ambiguous. In the case of the latter, the decrease in the number of right-eyed and left-eyed, and the decided increase in the number of binocular and ambiguous, may be attributed to the special conditions of testing when the manoptoscope is used. Also, to the fact that, with this instrument, it is largely a chance matter as to which eye first gets into line with the disk. Then, again, the size of the circular aperture in the sighting tube, and the distance at which it is held, permits the *S* to bring together the images of the two eyes, or, at least, to cause an overlapping of the images within the limits of the disk.

In view of the facts we have considered, the monoptometer would seem to be superior to other devices for measuring eye-dominance, first, in that the test conditions are more objective, less artificial, and more nearly in accord with the requirements of ordinary vision; secondly, in that the form of dominance can be determined without the aid of the *S*'s report; and, thirdly, in that it is possible to secure quantitative results, not only with respect to *kind* of dominance, but also with respect to *degree* of dominance.

COMBINING THE BREATHING UNDAE OF SPEAKER AND LISTENER WITH THE DICTAPHONE RECORD OF THE SPEECH

By LYMAN SPICER JUDSON, Alabama Polytechnic Institute

The writer has pointed out that the breathing of the listener is affected by the stimuli of the heard words.[1] It has also been noted by MacDougall,[2] Gamble,[3] Wycozoikowska,[4] Clark,[5] Golla and Antonovitch,[6] and others that the breathing of the speaker not only varies during thinking, silent reading, whispering, and overt speech, but that it probably varies with different kinds of thinking, reading, and speaking.

The apparatus described provides an objective means of recording the breathing of the speaker and listener, and of furnishing an accurate identification of the word-breathing relationship. Essentially, the equipment makes it possible to record every breathing movement of the thorax (and abdomen, if required) and to record at the same time the word or words which accompanied the breathing movements. The two are closely correlated by identical timing devices.

Fig. 5 shows the spring pneumograph worn by the speaker.[7] This impinges a column of air directly to a knife-edge, water tambour. The tambour lever operates, over a pulley system, the moving coil of the Electrodynamic Recorder. This latter unit consists of a light movable coil cutting lines of force in the field of an energized primary helix. The induced current set up is amplified and controlled through a D.C. amplifier for convenient operation of a zero-center, dead-beat recording voltmeter. The voltmeter lever writes directly on the kymograph paper. Two of these combined air-recorder units may be used in obtaining both thoracic and abdominal records. Any convenient number of these combinations may be used to obtain the breathing undae of the speaker and one or more listening *Ss*. For long speeches a switch in the recorder circuit between the amplifier and the voltmeter permits almost instantaneous change from one kymograph to a fresh kymograph record.

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[1] L. S. Judson, Objective studies of the influence of the speaker on the listener, *J. Expression*, 4, 1929, 1-11.

[2] R. MacDougall, The physical characteristics of attention, *Psychol. Rev.*, 3, 1896, 158-180.

[3] E. A. McC. Gamble, Attention and thoracic breathing, this JOURNAL, 16, 1905, 261-292.

[4] A. Wycozoikowska, Theoretical and experimental studies in the mechanism of speech, *Psychol. Rev.*, 20, 1913, 448-458.

[5] R. S. Clark, An experimental study of silent thinking, *Arch. Psychol.*, 7, 1922, (No. 48), 1-101.

[6] F. L. Golla and S. Antonovitch, The respiratory rhythm in its relation to the mechanism of thought, *Brain*, 52, 1929, 491-509.

[7] The Electrodynamic Recorder may be used directly with the air-pressure system of Golla and Antonovitch.

The speech to be recorded is passed through a condenser microphone and amplifier to a loud speaker. The loud speaker is in close proximity to the time-markers indicating seconds, minutes, or any other convenient interval. In addition to marking the kymograph record, these timers are adjusted to click loudly or to ring a small bell. These audible time-markings are received, superimposed on the speech, by the dictaphone. Thus the breathing of the speaker, as recorded on the kymograph, is timed by the identical timer which times the speech on the dictaphone record. This provides an objective record of the cause-and-effect relationship of speaking and breathing, which may be studied with the assurance that it shows the original correlation of the two factors.

For long speeches, continuity of the dictaphone record may be assured by using two dictaphones and over-lapping the records.

The use of this apparatus makes it possible to apply objective measurements to changes in breathing on the part of the speaker, due to (a) internal conditions and (b) external conditions (*e.g.* applause, hissing, which can be directed by accomplices of the experimenter), and to changes in breathing on the part of one or more listeners, due to the influence of the speaker or of the crowd. The utilization of distant recording devices, such as the Electrodynamic Recorder and the microphone, makes it possible to measure changes in the actual audience-situation with laboratory accuracy. The recording may be done at any convenient distance from the audience.

With the close coöperation of the speaker, and by having the listeners under observation scattered in the audience, it is possible very nearly to duplicate the normal audience-situation.

APPARATUS NOTE

A Simple Device for Producing an Illusion of Seen Movement

The device described here is essentially a modification of the well-known barber-pole illusion.[1] A cylinder of any stiff material is spirally striped or grooved to produce the effect of a barber-pole. A large disk or washer is inserted tightly over the cylinder and fastened at about the center of its length. The dimensions of the parts depend on the use of the device. For demonstration purposes a rod 18 in. in length and 2 in. in diam. gives satisfactory results. This is inserted through a washer 1 in. thick with an external diameter of 4 in. and a hole cut as close as possible to the diameter of the cylinder. Rotation of the device results in an apparent movement of the washer along the axis of the cylinder. This movement can be eliminated only by establishing visual points of anchorage on a stationary background.

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[1] Cf. J. P. Guilford, Illusory movement from a rotating barber pole, this Journal, 41, 1929, 686 f.

NOTES AND DISCUSSIONS

A Case of Apparent Blue-Blindness

R is a young woman 19 years of age and next to the eldest of a family of 6 children consisting of 4 girls and 2 boys. She has had, as far back as she can remember, difficulty in the perception of certain colors. So far as she knows no one else in her family has had such trouble, nor has she herself had any accident affecting her eyes. Her eyes are grayish blue in color and appear normal to the ordinary observer. When asked her favorite color she replied, "blue." She has never needed to wear glasses.

R was first given the Nagel test.[1] In this test she named all colors correctly except in the case of Plate B-1, in which she called the yellowish brown spots green and the brown spots a darker green. Since this test showed *R* not to be a case of ordinary red-green blindness, an attempt was made to determine whether she was a case of so-called blue-blindness. This was done by means of tests made especially for the purpose:

TABLE I

Showing *R*'s Ability to Distinguish Letters in a Color-Confusion Test

Plate	Color	Letter	Response
1	Y-O St. and V-R St.	P	+
2	G-Y St. and G St.	H	+
3	Ro-V St. and G-Y St.	O	+
4	G-B St. and B-G St.	V	–
5	Y-G St. and B-V Tint 1	F	+
6	Y Tint 1 and G-B Tint 1	E	+

Plates were prepared of bits of paper of different colors stuck on a white background of ordinary drawing paper. A letter was made up of particles of one color surrounded by particles of another color interwoven about the letter. The letter could easily be discerned by a person of normal color vision. The colors used in making up the plates were Milton Bradley standardized colors. The bits of color were about 1/8 in. across and the letters about 1 1/2 in. high. The plates were held about 15 in. from *R*.

The construction of the plates used in the first tests for blue-blindness is shown in Table I. In this table the colors and the letters used in the various plates are shown in the second and third columns, while *R*'s responses are indicated in the fourth. The colors are abbreviated as follows: Y-O means Yellow-Orange; G-Y means Green-Yellow; Ro-V, Rose Violet; G, Green; G-B, Green-Blue; B-G, Blue-Green; Y-G, Yellow-Green; B-V, Blue-Violet; Y, Yellow; and St. means standard. Every plate was first presented to a normal *S* (who indicated by a nod of the head if she perceived the letter) and then to *R*, who gave an oral response naming the letter or else indicating that she could perceive no letter. A plus sign in the fourth column indicates that *R* was able to distinguish the letter; a minus sign that she could distinguish no letter. The tests were presented in good daylight. The results show that the letter V in G-B St. on a B-G St. background alone passed unnoticed.

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[1] W. Nagel: Tafeln zur Untersuchung des Farbenunterscheidungs-Vermögen, 9th ed., J. F. Bergman, Weisbaden, Germany.

Further tests were constructed on a similar plan for the purpose of finding out whether *R* could distinguish between shades of blue and gray. Five blues (1–5) and 6 grays (A–F) were used. In Table II correct designations for each of the blues (taken from the Milton Bradley Standard Color Chart) appear horizontally across the top of the table, while names of the grays according to the same chart are given in the vertical column at the extreme left. A plate composed of Gray A and Blue 1 will be designated as A (1), one composed of Gray B and Blue 2 as B(2) etc. The letter used in each plate is shown in the first vertical column under each caption; the responses to the various plates as they were presented to *R*, in the next two columns. A plus sign in the second column indicates that *R* was able to distinguish the letter in the plate and a minus sign that she was unable to do so. In the third column a plus sign indicates that *R* distinguished the colors and a minus sign that she did not. (When *R* was able to distinguish the letter she was asked if the letter and background were the same color). Reading under (1) we should have then the following facts shown: in Plate A(1), *R* was able to distinguish the letter (H) and did not confuse the colors which appeared on the plate; in Plate B(3) *R* was able to distinguish the letter (M) but she confused the colors. The responses marked by one minus sign show that *R* was able to perceive the letter but was unable to distinguish between the colors. The responses marked by two minus signs show that she was unable to perceive the letter and hence could not distinguish between the two colors as to hue or brightness. The table shows that 19 out of 30 plates (63.3%) gave a confusion of colors, while 14 out of 30 responses (46.6%) showed confusion of colors but ability to perceive the letter, indicating confusion of colors but not of brightness. Five out of 30 responses (16.6%) show inability to distinguish the letter, *i.e.* confusion of both color and brightness.

TABLE II

Showing *R*'s Ability to Distinguish Letters and Colors in a Color-Brightness Confusion Test

	Blues														
Grays	(1) Shade 1			(2) Tint 1			(3) Tint 2			(4) G-B Tint			(5) G-B St.		
A. Very dark, almost black	H	+	+	X	+	+	L	+	−	U	+	−	A	+	+
B. Dark, cool	P	+	−	K	+	−	M	+	−	L	−	−	A	+	+
C. Light, warm	E	+	+	S	+	+	T	+	−	J	+	−	O	+	−
D. Light, neutral	N	+	+	R	+	+	A	−	−	A	+	−	E	−	−
E. Green-gray, light	N	+	+	U	+	−	H	+	−	V	+	−	L	+	+
F. Light, cool	F	+	−	Y	+	−	S	−	−	U	+	+	R	−	−
Failure, letters		0			0			2			1			2	
Failure, colors			2			3			6			5			3
Failure, brightness									2			1			2

Further tests were made to study *R*'s reactions to shades of green and yellow and to blue and green. Table III shows the construction of the plates and *R*'s responses. The second and third columns show the colors of which each plate is composed and the letter used. Columns 4 and 5 give the responses, showing *R*'s success or failure in distinguishing the letters and the colors. Thus, reading from the table, we see that in Plate 1 made up of G St. and G-Y St. *R* was unable to distinguish the letter J which appeared on the plate, hence the two colors appeared of the same hue and brightness. In two cases she was unable to perceive the letter and in three cases the colors appeared the same. In one case *R* was able to distinguish the letter but confused the colors, showing that she distinguished the letter by difference in brightness.

TABLE III

SHOWING *R*'S ABILITY TO DISTINGUISH LETTERS AND COLORS IN A COLOR-BRIGHTNESS CONFUSION TEST INVOLVING CERTAIN SHADES OF GREEN AND YELLOW

Plate	Colors	Letter	Ability to Distinguish Letters	Ability to Distinguish Colors
1	G and G-Y	J	–	–
2	G-Y and Y-G	H	+	–
3	Y and G	N	–	–
4	G-B and G	L	+	+

Discussion. The first test for what is called blue-blindness (Table I) shows that, in all but Plate 4, *R* is able to distinguish the letter. Failure on Plate 4 indicates confusion as to both color and brightness between G-B St. and B-G St.; but the extent of the confusion of colors is not clear from this test. Table II, however, shows most clearly the nature of *R*'s difficulty. In nearly half of the responses she could distinguish between grays and blues as to brightness but not as to hue. In no case was she able to distinguish the hue of Blue Tint 2 from any of the grays nor could she distinguish it either by hue or brightness from light neutral gray or light cool gray.

She had less difficulty, to be sure, with Blue Shade 1, and Green-Blue St. Blue Shade 1 she could distinguish from dark cool gray or light cool gray as to brightness, though not as to hue. In no case did she confuse Blue Shade 1 with a gray both as to hue and brightness.

In nearly two-thirds of all cases, however, *R* confused the hues of the blues and grays, showing the nature of her case clearly. She is, as we believe, a case of true partial color-blindness and not just of color weakness. She was unable to distinguish either by hue or brightness Blue Tint 2 from light neutral gray or from light cool gray; Green-Blue Tint 1 from dark cool gray; Green-Blue St. from light neutral gray or light cool gray.

It will be noted that 3 of the 5 confusions involving both brightness and hue occur when the blues were greenish in color. This probably has some significance. It will be noted also that 4 of the 5 confusions involve light neutral gray or light cool grays. This shows a tendency to confuse the lighter grays and the greenish blues both as to hue and brightness. Every gray, however, had its share of confusion with some one or other of the blues. Very dark, almost black gray was confused as to hue with the least number of blues.

The results in Table III shows that *R* has difficulty also in distinguishing between certain shades of green and yellow. She can see no difference in brightness nor hue between Green St. and Green-Yellow St. or between Yellow St. and Green St., whereas Green-Yellow St. and Yellow-Green St. appear the same as to hue but not as to brightness.

It may be inferred from the results of these tests that *R*'s case is a peculiar one of partial color-blindness. She is unable to distinguish between many shades of gray and blue, and between certain blues; nor can she distinguish between certain greens and yellows. The spectrum probably appears to her as different saturations and brightnesses of red and green, most yellows appearing greenish and the blues greenish or grayish. It is difficult to give a theoretical explanation over and above the suggestion that there is some abnormality in the rods and cones of the retina which abnormality has no doubt been inherited. There could well be further profitable study of *R*'s case and the writer would be glad to put any investigator in touch with her.

Columbia University. C. E. HUNTINGTON.

AN UNUSUAL CASE OF LOSS OF HABIT

A pair of gray squirrels which came under my observation regularly performed two successive responses on hickory nuts, each of which indicated discrimination of the apical end. Experimentation with these responses resulted in the loss of one and the retention of the other by each squirrel. So far as I know, this is a new phenomenon, and not without interest.

The squirrels, a male and a female, were received from a collecting station in Texas when about half grown, and kept under laboratory conditions. They never became gentle enough for handling. Except where otherwise noted, they were caged together.[1]

From the time the squirrels were first observed, they turned nuts over and over before opening them. During this examination the apical projection was bitten off, while the basal projection was not touched.

The opening of the hickory nuts was also found to proceed in a manner which required discrimination of the apical end. The squirrels split the nuts into halves by gnawing the flattened sides at the base of the apical projection. The kernels were then easily procured by chipping away the thin shell at the basal end. All the empty shells, which were in almost perfect halves, showed gnawing at the apical end and chipping at the basal end. An examination of the shells of 85 consecutive nuts revealed not a single exception.

To determine whether the discrimination of the apical end was visual, the squirrels were given hickory nuts in a dark room. They opened the nuts without hesitation in the same manner as before.

The animals were returned to daylight and nuts were presented from which the projections had been filed off so that both ends were uniformly smooth. This destroyed the habit; the squirrels seldom found the apical end; they gnawed at random on the surface of the nuts. Of a total of 23 filed nuts, only 2, or 8.7%, were split into halves as before. The empty shells of the other nuts were very much gnawed, showing that much more than ordinary time and energy had been required to open them. The sensory control of the discrimination must therefore have been either tactual or olfactory or both.

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[1] I am indebted to Ohio State University for the original purchase of the squirrels and for permitting them to be forwarded to Michigan State College, where the observations were made.

When fed normal nuts the following day, the squirrels bit off the apical projection as before. This they continued to do throughout the period of experimentation. Although the animals were not always observed in the act, examination of the shells showed that the apical projections had been removed from nearly all opened nuts, while the basal end was not scratched. In many cases the squirrels were observed in the performance of the response.

However, the squirrels did not resume their old habit of splitting the nuts, but gnawed the surface of the nuts indiscriminately as they had done with those that had been filed. Often a large section of a nut was gnawed away, and frequently even after such exertions not all of the meat was secured. The few cases in which the apical end was found seem to have been the result of chance. The habit was completely lost.

For over four months the animals were fed the same kind of hickory nuts as before. The only other kinds of food given were some other nuts mentioned later, and some vegetables. The shells of the opened hickory nuts were examined at intervals to determine the percentage of the nuts which had been split into halves by gnawing at the apical end only.

From March 21, the day after the sensory control tests were made, to April 15, 215 nuts were opened, of which 8.7% were opened "correctly". From April 15 to June 6, 309 were opened with 7.1% correct. From June 6 to June 22 the squirrels were fed no nuts whatsoever. Following this rest, from June 22 to July 5, 172 nuts were opened and 10.4% were correct. Of the 226 nuts opened from July 5 to July 30, 13.7% were correct. The experiment had to be discontinued at this time.

From June 22 onward the two squirrels were kept in separate cages. The individual records may be of interest in addition to the totals given above. The male opened 145 nuts, of which 9.7% were correctly opened; the female opened 253, of which 13.8% were correct.

It will be recalled that before the filed nuts were presented the squirrels had opened 100% of the nuts correctly. Of the 23 filed nuts, 8.7% were correct. When 932 normal nuts had been opened after the filed nuts, the correctness was only 13.7%. This practice covered a period of over four months. The original habit must have been acquired much more rapidly, for the squirrels could have been little more than four months of age when they were obtained.

Before the filed nuts were given, forty each of hazel nuts and of almonds had been presented to the squirrels, and this was repeated after the former manner of opening hickory nuts had been lost. In both cases the nuts were invariably opened from the apical end, which was the sharper end. Nuts with apical ends less different from the basal, such as English walnuts and acorns, were usually but not uniformly opened from the apical end, both before and after the presentation of the filed hickory nuts. The loss of habit was therefore specific to hickory nuts.

In order to throw some light upon the distribution of the apical method of opening nuts, 180 hickory nutshells, apparently opened by squirrels, were gathered at random in a forest. Only 34.7% of these were split in half. Either this is a learned response acquired by relatively few squirrels or it is an instinctive response lost by a large number of squirrels. The first alternative seems the more probable.

The two squirrels here studied accepted and opened wormy nuts of any kind as readily as good ones. This fact supports an earlier observation of the author that the rejection of wormy nuts is very probably a learned response.[2]

It is a matter of common knowledge that when the difference between two stimuli is reduced until the discrimination disappears, the animal may subsequently fail to respond accurately to stimuli which were formerly differentiated very easily. In this experiment that is what happened to the "opening" response when the two ends of the nut were made indistinguishable. However, in the present case, discrimination of the end of the nut was indicated by the removal of the projection as well as by the act of opening the nut. The habit of removing the apical projection was not lost. The experiment shows that when discrimination responses break down the ability to perceive the difference between the two stimuli is not necessarily lost, for the persistence of this ability is witnessed by the second discrimination response. But the difference no longer leads to the customary response. The change has been associational rather than sensory.

University of Virginia Wayne Dennis

A Summary of Certain Factors in Current Investigations on Learning and Memory

This note represents an attempt to summarize certain trends of experimentation on learning and memory reported during the years 1925-1930 inclusive. Slightly more than 7% of the total number of pages issued during the period by 22 psychological publications has been devoted to studies of this type.[1] A survey of this material brought out certain facts pertaining to (*1*) the size of the groups employed, (*2*) the kinds of subjects experimented upon, and (*3*) the age of the human subjects used. In obtaining the data for the following tables, the journals cited were read and any reports of an experimental nature, *i.e.* those in which factors were systematically varied in an attempt to determine their effect upon learning and memory, were included. There was a decided tendency for studies to be titled 'experimental' when they were quite obviously statistical.

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[2] W. Dennis, The rejection of wormy nuts by squirrels, *J. Mammalogy*, 2, 1930, 195-201.

[1] The journals examined totaled 53,671 pages of psychological literature of which number 3,937 pages dealt with experimental studies of learning and memory. The following journals were included in this summary: (1) *Amer. J. Psychol.*; (2) *Arch. Psychol.*; (3) *Australasian J. Philos. and Psychol.*; (4) *Brit. J. Psychol.*; (5) *Brit. Psychol. Monog.*;(6) *Univ. Calif. Pub. Psychol.*; (7) *Genet. Psychol. Monog.*; (8) *Univ. Iowa Stud. in Psychol.*; (9) *J. Abnorm. Psychol.*; (10) *Psychol. Clinic*; (11) *J. Comp. Psychol.*; (12) *J. App. Psychol.*; (13) *J. Exper. Psychol.*; (14) *J. General Psychol.*; (15) *J. Educ. Psychol.*; (16) *Mind*; (17) *Mental Hygiene*; (18) *Ped. Sem.*; (19) *Psychoanalytic Rev.*; (20) *Psychol. Rev.*; (21) *Psychol. Bull.*; and (22) *Psychol. Monog.*

Such reports were not included in the present summary, nor were a few lengthy theoretical articles supported by meager and incidental experimentation.

(*1*) *Size of groups.* The total number of *S*s used in any experimental program is of little consequence, the important consideration is the number of *S*s used per experimental group. Table I is, therefore, based upon the number of *S*s used per group, and gives for every year of the 6-yr. period the percentage of the total number of groups used which falls within certain designated class limits.[3] More than 23% of the groups numbered less than 11 *S*s. It would seem that such studies are worse than useless for comparative purposes. In a number of investigations college classes were used for research purposes with the apparent implication that any two of them were sufficiently equated for experimental comparisons. This assumption is gratuitous. The equation of experimental and control groups is quite unsettled, especially in the field of infra-human study. Researchers using animal *S*s rarely make an attempt toward preliminary equation of groups. Labeling conclusions tentative does not seem to be sufficient justification for the use of groups which are compared without being subjected to some sort of preliminary equation.

TABLE I

PERCENTAGE OF THE TOTAL NUMBER OF GROUPS USED EACH YEAR WHICH FALL WITHIN CERTAIN CLASS LIMITS

Year	1-10	11-20	21-30	31-40	41-50	51-60	61-70	71-80	81+
1925	4	22	23	7	6	8	1	9	20
1926	25	31	19	6	1	10	1	0	7
1927	20	23	8	8	9	1	2	7	22
1928	30	12	14	6	1	7	6	2	22
1929	34	15	13	6	12	2	2	3	13
1930	29	27	24	5	6	3	1	1	4

(*2*) *Kinds of S*s. It is of interest that so few studies have been reported upon *S*s other than vertebrates. One who wishes to approach the problems of learning and retention phylogenetically must progress very sketchily until he reaches the fishes. Table II partially summarizes the present status of affairs with respect to this factor by giving the percentage of the total number of learning and memory studies which was concerned with humans, with rats and mice, and with all other animals. The major part of the work dealt with humans to the almost entire neglect of all phyla except the vertebrate. Less than 1% of the investigations reported involved the use of invertebrates.

(*3*) *Age of the human S*s. Theoretically, the problems of learning at any one developmental level are neither more nor less important than those at any other. For an all inclusive and systematic psychology of the growth of human learning ability it is essential that there be at least a large number of studies for each age. That this situation does not prevail has frequently been noted. There are many studies of the learning and retention of college and graduate students, but experimental investigations of the ability of pre-school and primary grade children are relatively scarce. Table III gives the percentages of the total number of experimental studies concerned with humans at the different age levels. The limits of the age groups cited are arbitrary, but are nevertheless most consistent with the designations used in the studies reported. Over 62% of the studies conducted were performed on adults.

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[3] The percentages in Table I involve more groups than there were studies reported upon. This is due to the fact that many single investigations involved two or more groups.

TABLE II

Percentage of the Total Number of Learning and Memory Studies Devoted to Different Animals

Ss	1925	1926	1927	1928	1929	1930	Mean for 6 years
Human	68	51	56	50	56	71	58.7
Rats and mice	23	19	33	34	40	25	29.0
All others	9	30	11	16	4	4	12.3

TABLE III

Percentage of the Total Number of Human Studies Performed on Different Age Groups

Group	1925	1926	1927	1928	1929	1930	Mean
Pre-school	11	12	16	6	11	9	10.8
Grades I-IV	6	8	5	12	4	2	6.1
Grades V-VIII	21	5	10	21	6	7	11.7
High school	3	13	4	11	15	10	9.3
Adults	59	62	65	50	63	71	62.1

The following three statements summarize briefly the above tables: (1) Of all the groups used in experiments on learning and memory, 23.7% numbered less than 11; (2) 58.7% of such studies were performed on humans, with less than 1% being devoted to invertebrates; (3) 62.1% of the studies involving humans were performed on adults, with but 17.8% concerning themselves with school children.

De Pauw University Stephen M. Corey

A Note on "A Neglected Possibility in Frequency Theories of Hearing"

In a recent number of this Journal Kreezer brings forward what he regards as a "neglected possibility" in auditory theory.[1] It happens, however, that his proposal is no more nor less than a statement of the volley theory, and as such is far from neglected in present-day discussions.

Kreezer throws aside our suggestions regarding the volley theory because, as he asserts, we assume nerve-fiber response inevitably to occur at a particular phase in the wave of stimulation. We have made no such assumption. On the contrary, in Fig. 2 of our article where the volley theory is represented,[2] we have pictured the fibers as showing variability in their response and hence producing by a piling up of discharges a form of distribution curve with a maximum corresponding to the point of maximum stimulation.

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[1] G. Kreezer, A neglected possibility in frequency theories of hearing, this Journal, 43, 1931, 659-664.

[2] E. G. Wever and C. W. Bray, Present possibilities for auditory theory, *Psychol. Rev.*, 37, 1930, 365-380, esp. 376 ff.

The assumption of this type of distribution of discharges is necessary if the volley theory (or the telephone theory either for that matter) is to account for the results that we have obtained experimentally, for, contrary to Kreezer's supposition, our amplifier and telephone receiver system will not distort to the extent of giving a sine-wave type of response to a signal that departs at all widely from it. His suggestion that the nerve-discharges are distributed uniformly throughout one half-cycle of each wave is therefore not possible, for such a "square wave" type of response would be distinctly noisy in its acoustic effect, and would not be adequate to the clear transmission of speech. The use of high-frequency oscillographs, which he proposes for testing this point, is thus hardly necessary; yet if he had examined our reports carefully he would have learned that we have made use of the cathode ray oscillograph in these experiments, though primarily in measurements of intensity where such instruments indeed excel the telephone receiver.[3] We may say here, in order to leave no chance for misunderstanding, that our oscillographic studies check with the work with the telephone receiver in excluding such a type of response as Kreezer suggests.

Incidentally we add that Gasser did not "develop" the cathode-ray oscillograph, nor did Rosenberg develop the bifilar oscillograph, though each deserves the credit of using these instruments in nerve studies; and that the discussion of quanta of intensity, which Kreezer enters into (with certain arithmetical peculiarities), is pointless in the utter absence of experimental results indicating such quanta in this or any other sense-field. For, indeed, it is a serious stumbling-block of "straight telephone" theories, along with the simple place theories, that they require that quanta, either of intensity or of pitch, be assumed despite the absence of experimental indications of them;[4] and it is a particular merit of the volley theory that it leads to no such assumption.

Princeton University

E. G. WEVER
C. W. BRAY

THE THIRTY-NINTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The thirty-ninth annual meeting of the American Psychological Association was held at the University of Toronto, Toronto, Ontario, on Thursday, Friday, and Saturday, September 10, 11, and 12, 1931. Registration figures show a total of 448 persons in attendance classified as follows: 128 members, 166 associate members, 29 newly-elected members, and 125 persons not affiliated with the Association.

At the business meeting, 5 new members and 264 associate members were elected. The Secretary announced that the mail ballot on the question of alternating Annual Meetings east and west of the Appalachian Mountains showed 655 favoring such policy and 161 opposing, and that the mail ballot on the question of September *vs.* December as the time of meeting showed 581 favoring September and 238 favoring December. The By-Laws were amended to provide for holding future Annual Meetings during the first two weeks of September. Another change in the By-Laws moves the date for closing applications for Associate Membership from March 15 to July 1. It is to be noted that the date for closing applications for members and for transfers from associate to member remains March 15. It was voted to accept the invitation of Cornell University to hold the fortieth annual meeting at Ithaca, N. Y. on September 8, 9, and 10, 1932 and to appoint Professor Madison Bentley as a member of the Executive Committee for 1931-32. The Council of Directors was authorized to make arrangements for a meeting in Chicago in September 1933. Newly-elected officers are: President, Walter R. Miles; Directors, Calvin P. Stone and Edward C. Tolman; Secretary, Donald G. Paterson; Division of Anthropology and Psychology, N.R.C., K. S. Lashley and Joseph Peterson; S.S.R.C., Carl C. Brigham; A.A.A.S. Harvey A. Carr and Walter F. Dearborn. The appointment of Herbert S. Langfeld as editor of the Psychological Monographs was ratified. Leonard T. Troland, Clarence E. Ferree, and A. T. Poffenberger were appointed to represent the Association on the Inter-Society Color Council. J. McKeen Cattell was appointed as the Association's representative at the Seventh American Scientific Congress to be held in Mexico City, Mexico, in February 1932.

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[3] Wever and Bray, The nature of acoustic response, *J. Exper. Psychol.*, 13, 1930, 380.

[4] For a discussion of this matter of quanta, see E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 160.

The program represented a departure from previous practice in so far as no scheduled Round Tables were included. Ten sessions for formal papers were scheduled, designated as A or B programs, and seven additional sessions, designated as C programs, were scheduled for briefer reports, titles only listed in the Abstracts. Forty-eight papers were presented by members and associates in the A and B programs and fifty-seven ten-minute reports were presented in the C programs. Without making a detailed quantitative analysis of the program in comparison with previous programs, one gains the impression that the bulk of present-day experimentation, as reflected in the Toronto papers, is concerned with what Woodworth designates as the "psychology of performance," the subjects being rats, primates, new-born infants, pre-school children, college students representing adults, and the senescent. There were very few papers on applied or technological psychology.

On Thursday morning the Section of Clinical Psychology held its annual meeting followed by presentation of six papers. The newly-elected officers are: Chairman, Fred Kuhlmann; Secretary, Luton Ackerson; and other members of the Executive Committee, T. C. Hegge, M. R. Trabue, and F. N. Maxfield.

On Thursday afternoon at 4:00 p. m. an informal conference on studies related to the broadcasting of psychology was sponsored by the Committee on Psychology appointed by the American Psychological Association to coöperate with the National Advisory Council on Radio in Education. The attendance and discussion clearly indicated wide-spread interest in the educational and research opportunities afforded by the radio.

The University of Toronto was host at a delightful tea held in the Quadrangle of Hart House on Friday afternoon. The tea was followed by an open house affording delegates an opportunity to visit all departments of Hart House which is a "cultural center" in the broadest sense of the term.

On Friday evening, the Reverend Dr. H. J. Cody of the Board of Governors of the University of Toronto cordially welcomed the Association Membership and gave a brief history of psychology at Toronto, including an account of its present status as a subject of instruction, research, and state service. This was followed by the Presidential Address, "The Psychological Study of Behavior," by Dr. Walter S. Hunter in which it was argued that psychology seeks to describe and explain extrinsic behavior whereas physiology deals with intrinsic behavior. Extrinsic behavior is not dependent upon a specific organ for its manifestation nor upon a fixed mode of response in any given organ but primarily involves adaptation to the external environment including the latter's social phases. Attention was also directed to the difficulties inherent in classifying psychological forms of behavior because of the character of the behavior studied. These difficulties are not present in physiology since the latter science studies behavior which can be classified in terms of the structures involved.

The facilities for the meeting were uniformly excellent as regards dormitories, dining arrangements, and size and location of the rooms and auditoriums used for the various programs. Professor Bott and his associates proved themselves to be ideal hosts.

University of Minnesota DONALD G. PATERSON

THE NATIONAL RESEARCH COUNCIL'S FUND TO SUPPORT RESEARCH

The National Research Council has been given the administration of a limited fund from which grants can be made in comparatively small amounts toward the support of the research of American investigators in the natural sciences. The field includes Psychology.

The Committee in charge of the Fund has adopted the following administrative policy:

(1) Grants will be made to cover such expenses as apparatus, materials and supplies, technical assistance, and, in special cases, field expenses.

(2) In general, grants will not be given for personal salaries or fellowship stipends, for expenses of publication, for the purchase of books, or for travel to attend scientific meetings.

(3) Preference will ordinarily be given to the support of investigations (a) which can be completed with the aid of the grant, (b) toward which the institution to which the applicant is attached also contributes financially or through special support, and (3) for which a grant of not more than $1,000 is requested.

(4) A report of progress, with items of expenditure, should be made by the grantee to the Secretary of the Committee at least twice a year, as of December 31 and June 30.

(5) The title to property purchased from grants will remain with the National Research Council until ultimate disposition is made by the Council.

Applications should be made as early as possible.

Grants made during the past year of particular interest to Psychology are as follows:

L. W. Max	The physiological basis of thinking
Elsie Murray	Investigations in color-vision

H. C. Sanborn	Heritability of the songs of birds
R. C. Travis	Speed and characteristics of reflex and voluntary eye-movements as indicators of the adequacy of adaptive behavior
K. M. Dallenbach	Studies in neural regeneration
E. C. Tolman	The inheritance of maze-learning ability in rats
F. Fearing	The functions of the non-acoustic portion of the labyrinth in pigeons
J. A. McGeoch	The influence of the time-interval and of the point of interpolation upon degree of retroactive inhibition
Sophie De Aberle	Growth and development of southwestern Indian children

Address Grants-in-aid, National Research Council, 2101 Constitution Ave., Washington, D. C.

M.B.

Exchange of Odd Volumes and Partial Sets of Psychological Journals

Most psychologists possess incomplete sets of some of our American and European periodicals. The National Research Council has thought it worth while to offer its services as a clearing house for the completion, exchange, and possible sale of these partial sets. The recent publication of *A Volume-Year Check List of Psychological and Allied Journals* makes this time favorable. Professor E. S. Conklin has been good enough to list here by volume and year a large number of our periodicals. (Vol. 1, No. 2, *Psychol. Ser.*, University of Oregon.) The pamphlet sells for fifty cents a copy.

If you are sufficiently interested in disposing of, or in completing, your partial sets, drop a card or note to the Council saying that you will pay for a copy of the check list. Your order will be forwarded to the University of Oregon Press, and the check list with bill sent you directly. Upon its receipt you may indicate upon it by the word "buy" any volumes you desire to purchase, and by the word "sell" (adding price by lot or volume) any volumes which you would be willing to dispose of. Then send the check list to this office; and if a sufficient number of returns are received they will be consolidated and you will be informed of opportunities for sale or exchange. The Council will act only as a center of information. Actual selling and buying negotiations should be made directly between seller and purchaser.

If you prefer to type your list (indicating each year, volume and issue of each journal), you may send this list instead of the Oregon list to Division of Anthropology and Psychology, National Research Council, 2101 Constitution Avenue, Washington, D. C.

M. B.

A Correction

In my note on a "Neglected Possibility in Frequency Theories of Hearing" in the October 1931 number, the figure for the refractory period, on the basis of which the calculations are made (pp. 660 f.), should read 0.005, not 0.02 sec.

University of Berlin GEORGE KREEZER

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Social Psychology: An Analysis of Social Behavior. By KIMBALL YOUNG. New York, Alfred A. Knopf, 1930. Pp. xii, 674.

Starting with the definition "Social Psychology deals with the study of personality as it develops in relation to social environment," the author divides his subject into five parts as follows: (1) on the social setting of human behavior, including also a brief introductory chapter and also a discussion of group life of animals and men and the nature of culture; (2) on the psychology of individual behavior, differing only slightly from the usual general introductory psychology; (3) on personality and group participation, covering the topics of language and social interaction, early conditioning, and influence on the personality of primary and secondary group contacts, occupational attitudes, leadership, authority and prestige; (4) on personality and subjective patterns as symbolized in language and exhibited in group life especially in myths and legends, and also on various types of race, national, religious and occupational personalities and prejudices; and (5) on the crowd and the public as groups in relation to personality, ending with a discussion of the psychology of the audience. There is an index of names and of subjects. At the close of each chapter suggestions and references are given for the student. The book is planned to be used as a text in connection with the *Source Book for Social Psychology* by the same author.

The style is clear, and interest is insured by the use of many concrete incidents and facts. It is up-to-date, and as objectively scientific, and nearly as well organized as the present state of the science makes possible. It will undoubtedly take a high place among works in this field, and be very useful as a text. For the latter purpose Part Two could easily be omitted for students who have already had courses in psychology. It would probably have been better to omit technical details, especially physiological psychology, and to make this portion a briefer introduction to the data of general and individual psychology, necessary to the understanding of social psychology proper.

It is always difficult in organizing a new body of scientific knowledge to determine the chief center of interest and fix the bounds of the science. It was an important change in the development of the science of social psychology when Allport demolished the indefinite, unfruitful idea of a "group mind" and centered interest on the individual, but in the opinion of the reviewer, another shift is needed. If we transpose Young's definition thus: Social Psychology deals with social environment as it develops in relation to personalities—we have a change in the center of interest that will be useful in the future development of the science. Ultimately it will be necessary to recognize that the center of interest for social psychology is neither personality as such, nor social environment as such; but these two factors interacting under certain physical conditions in determining group conduct and in developing human institutions. The latter aspect of social psychology is not ignored by the author, but the development of individuals rather than of institutions is stressed, although perhaps not more than is justified in the present state of knowledge of the social sciences.

Leominster, Massachusetts E. A. KIRKPATRICK

The Language Development of the Preschool Child. By DOROTHEA A. McCARTHY. Minneapolis, The University of Minnesota Press, 1930. Pp. xiii, 174.

This monograph begins with an historical sketch of the main contributions in this line by previous investigators. The study was carried out on a total of 140 children, 20 divided evenly as to sex representing each of the following ages: 18, 24, 30, 36, 42, 48 and 54 months. The observation on each child was made within a month and a half of its classified age, and the subjects were so selected as to be fair samples of the preschool population of Minneapolis. For this selection the occupation of the father was used as criterion, and the Barr-Taussig Six-Fold Classification of Occupations was applied to the adult male population of Minneapolis between the ages of twenty-one and thirty-five years, the percentage of each type of population being calculated from the 1920 census report. The mental test records of the children, by the Kuhlmann Revision of the Binet Scale, showed the mean I. Q. to be slightly above 100 at each age-level represented, and the girls were slightly superior to the boys. Despite very careful procedure it was, however, not possible exactly to equalize throughout all groups such factors as the coöperation of parents, the interest of parents in the free clinics and the greater stability of the population from which the samplings were chosen. Each child was observed individually in familiar environment for a period during which fifty consecutive verbal responses were recorded under carefully controlled conditions. Any response marked off from the preceding and succeeding remarks by a pause was considered as a separate unit.

An analysis of the results reveals certain recognizable stages of linguistic development and changes in the relative proportion of the different "parts of speech." It is observed that words as used by the child hardly fit into the different parts of speech as outlined by grammarians. Often a word may represent a complete sentence, and its classification by adults is difficult. In early stages of development the number of incomprehensible responses was large, and with advance of age it decreased faster in the case of girls than in that of the boys. The same order of development was also found through changes from simple to complex sentences, showing, in conformity with the results of intelligence tests, greater precocity in the girls. The details of changes in emphasis on different parts of speech as age advanced were not significantly different from those already well known. Though the psychologist's contributions to the knowledge of language development has in the past been small, it is evident that his part in the future scientific studies of this kind is likely to be considerable. The main contribution of the present study seems to be the greater precautions as to representative samplings, larger number of subjects and impartial recording of linguistic responses. It is obvious that much is yet to be done in recording objectively these responses through a long period of time. The children of the upper classes in general showed more rapid development than those from lower social strata. It is interesting to note that the child who is barely making a beginning in language at 18 months gains proficiency in his vernacular, learning several thousand words for use in various more or less complex sentence relationships, in the short period of three years. J. P.

Statistical Résumé of Spearman's Two Factor Theory. By KARL J. HOLZINGER. Chicago, University of Chicago Press, 1930. Pp. 43.

In this forty-three page monograph, Holzinger has given a more clear and readable presentation of the statistical aspects of Spearman's two-factor theory than has appeared in any one publication.

The résumé includes the derivation of the tetrad difference function, an account of the probable error of the tetrad difference, and also the best approximation formulas for the probable errors. A list of all the possible tetrad differences of five variables and their probable errors is given along with a sample problem showing how these formulae are used.

Finally, there is a discussion of other factor patterns. Such patterns, while of theoretical importance perhaps, do not seem to fit most of the empirical data any better than the simpler two factor pattern of Spearman. In addition, the statistical procedures necessary to an investigation of group factors are much longer and more clumsy to handle.

If one wishes to make use of the technique of the tetrad difference, this monograph will prove very valuable.

Ohio State University HAROLD A. EDGERTON

Pleasure and Instinct: A Study of the Psychology of Human Action. By A. H. BURLTON ALLEN. New York, Harcourt, Brace & Co., 1930. Pp. ix, 336.

Following a brief sketch of theories of pleasure and 'unpleasure,' or feeling, regarded both as affective and as akin to sensations, the author takes the position that "Pleasure is the mental element which accompanies the sensations of the bodily processes when they proceed in a normal fashion" (p. 60). With strong stimulation pleasure may be enhanced. Unpleasure occurs with conflicts among the vital impulses as affected either by under- or over-stimulation. But what does this have to do with the control of human action? The reader who asks this question critically with an idea of getting an insight into the methods of control of the vital processes finds nothing of a scientific nature, but only unverified assumptions or implications of causation by subjective phenomena which are associated with activity (probably only different aspects of it). The results are not markedly different from attempts to explain a man's life activities from the movements of his shadows. 'Conation' plays an important part in the general speculations of this volume. Many of the British psychologists and philosophers would seemingly profit from a thorough and critical analysis of the implications of their concept of conation which plays no part in American psychology.

A discussion of some of "the human instincts," largely according to McDougall's early conception, including the 'impulse to power,' altruism (also treated as an instinct), and others, as they are related to conation and the general struggle to maintain the basic nutritive processes, occupies over 130 pages. The inheritance of acquired tendencies is assumed without regard to current biological data to the contrary. Self-consciousness is regarded as playing a part in a rudimentary form in such 'instincts' especially as fear and anger, and in the higher 'conative' developments they emerge as the more explicit consciousness of the unity of personality. But even this higher stage, fortunately, is found to come into being as a result of our mental constitution and not by direct 'individual choice.' Later chapters deal with relations of feeling to instincts and to sensations, aesthetic experiences, ideomotor action, and 'the psychology of values.' No positive contribution to psychology is made in the book, which follows the older arm-chair, reflective methods with not a touch of scientific procedure. The volume is of little value to the individual who is attempting by verifiable hypotheses and experiments either to extend our knowledge of psychology or to learn about its established results. It is indeed unfortunate that books of this kind apparently consume so much of the study-time of serious-minded students. There are, of course, points of view from which the book is interesting.

J. P.

Encyclopaedia of the Social Sciences, Vol. I. Edwin R. A. Seligman, editor-in-chief; Alvin Johnson, associate editor. New York, The Macmillan Co., 1930. Pp. xxvii, 646.

For the psychologist, the appearance of the first volume of the *Encyclopaedia* raises the interesting question of the relation between psychology and the so-called social sciences. The editors designate psychology as one of the semi-social sciences, along with ethics, education, and philosophy; the social sciences proper are politics, economics, law, anthropology, sociology, penology, and social work. In the case of the semi-social sciences those topics are selected of which the social aspects are acquiring increasing significance (preface, p. xx).

"Psychology is . . . becoming social in a double sense. We must interpret the individual mechanism of mental processes, in part at least, in terms of a social environment; and secondly we have to deal with the thought processes of the group as such, that is, of the individual not simply as unconsciously influenced by others but as purposively coöperating with others" (Introduction, What are the Social Sciences? Seligman, p. 7).

One can always quarrel with classifications. It would seem rather arbitrary to regard education as only semi-social when anthropology, with a large content on the physical side allying it closely with the biological sciences, is designated as one of the social sciences properly so called. As for psychology, the decision as to whether it should be more properly allied with the social or with the biological sciences must wait upon the determination of the extent to which individual mental mechanisms are the product of the social environment. In that connection Seligman seems to have chosen his words very happily when he speaks of psychology as "becoming" social. The extent of this "becoming" remains to be determined.

Rather more than half of this first volume is introductory. There is an historical account of "The Development of Social Thought and Institutions," sketched in broad outline from the time of the Greeks until the period of War and Reorientation; it is perhaps inevitable, in view of the range and compass of the outline, that much of it should be somewhat obscure except to the initiated.

The account of "The Social Sciences as Disciplines," which constitutes Part Two of the Introduction, describes the status, past and present, of the Social Sciences in the various European countries, Latin America, Japan, and the United States. Psychologists will undoubtedly wonder at the very rare and inadequate references to their science throughout Part Two. In the section on Great Britain are at least offered the explanation that psychology there has claimed affinity with the natural rather than with the social sciences; in the other sections, including that on the United States (L. L. Bernard) psychology is all but ignored. This is all the more surprising in view of the statement in Part One that "no summary of the definable influences of psychology can give an adequate account of its real importance for the social sciences in the post-war period. Especially in the latter part of the period psychology was 'in the air.' No social scientist not altogether impervious to the general movement of thought could fail to pay it tribute in the laying out of his problem, in his critical judgment of his own methods" (Section on War and Reorientation, p. 197, written by Editorial Staff). The writers of Part Two evidently did not make quite the same appraisal.

In the *Encyclopaedia* proper (Aaronson-Allegiance) there are a number of articles of definitely psychological interest. Jastrow writes on abnormal psychology and gives an impartial presentation of a wide range of topics,—hysteria, neurasthenia, Freudianism, extraversion-introversion, Kretschmer's types, endocrines, the autonomic nervous system, intelligence testing, maladjustment in children, crime, etc. One may question the possibility of giving any adequate idea of Kretschmer in one paragraph, or of Freud in three; certainly it would not be easy to understand these paragraphs without a background in the subject, but limitations of space perhaps made that inevitable. More surprising, in view of the nature of the *Encyclopaedia*, is the inadequate treatment of the social implications and applications of our knowledge of abnormal mechanisms, for example in certain religious manifestations, in mob phenomena, etc.

Miriam van Waters writes a good review of the topic of adolescence; L. S. Lyon treats advertising with hardly a word about the contributions of experimental psychology; Raymond Pearl discusses alcohol in its biological aspects, and comes to the significant conclusion that its moderate use is not harmful, and possibly beneficial; W. A. White defines alienist, and Kimball Young discusses adjustment.

In connection with the individual articles, the probability is that the psychologist who reads wisely will find the *Encyclopaedia* useful and interesting because of what it can tell him about the other social sciences, rather than about his own. He will hardly learn much from short articles, however well written, on topics in his own field. On the other hand when he reads—to take only one example—in the articles by Lowie on primitive adoption, of some of the interesting modifications and variations of what we have been accustomed to call the "parental instinct," he can get a hint from anthropology on the manner in which cultural factors affect some of our apparently fundamental psychological mechanisms. The function of the *Encyclopaedia* seems to be to make the knowledge of related sciences, rather than of our own, accessible to us. And in this connection it should be particularly valuable to psychologists, who have on the whole paid less attention to the other social sciences than they have paid to psychology.

On the Editorial Staff, representing psychology, are Floyd H. Allport, who is Advisory Editor; Georgina S. Gates and Mark A. May for the American Psychological Association; Edward L. Thorndike for the National Education Association. Margaret F. Washburn is on the Board of Directors, and a number of other psychologists act as Editorial Consultants.

The *Encyclopaedia* as a whole represents a notable effort at coördination, which promises, in spite of certain inevitable difficulties, to be a highly significant contribution to the development of the social sciences. The remaining volumes (fifteen in all are projected) will be eagerly awaited.

Columbia University OTTO KLINEBERG

Direction Orientation in Maze Running by the White Rat. By JOHN F. DASHIELL. Comparative Psychology Monographs, Vol. 7, No. 2. Baltimore, The Johns Hopkins Press, 1930. Pp. 71.

This monograph is an experimental study of the commonly observed tendency of the white rat to exhibit a general direction-orientation in maze running usually toward the exit and more or less independent of specific and local stimulation. In preliminary experiments this showed itself as a tendency to make errors in blind alleys that happened to open up in the general direction of the food box much more than in those that opened up in the opposite direction, regardless of the direction of the true pathway at such points—a finding which agrees with the earlier results. The problem of determining the nature of this reaction is studied extensively in the present monograph. Exteroceptive sources of guidance (visual, auditory, and olfactory cues both from within and without the maze) were eliminated by the usual methods of rotating the maze, changing the materials used in the construction of the maze, and using air currents from false food boxes, the experimenter, etc. By adopting specific modes of inserting animals it was found that animals tend to follow out their initial orientation to a much greater extent than could be accounted for on a chance basis. On the theoretical side, the author suggests that the rat on entering the maze takes on some sort of kinesthetic or organic posturing set developed from previous trials and directly related to the animal's position when proceeding up the entrance passage-way. This general factor persists and reasserts itself when the animal temporarily turns aside from the true pathway, and thus operates to facilitate movements consonant with this orientation and to inhibit antagonistic responses. This explanation may perhaps be regarded as a special application of the 'completeness of response' principle of Peterson to the particular conditions investigated.

Columbia University C. J. WARDEN

The Experimental Production of Dreams during Hypnosis. By DAVID BALLIN KLEIN. Austin, University of Texas Press, 1930. Pp. 71.

"I want you to watch very carefully for any dreams you might have and as soon as the dream is over tell me all about it." With these instructions to (mainly) eight hypnotized subjects and with a 10-second stimulus of various kinds, including a pinch on the hand, a whiff of perfume, sound of a tuning fork, handling a wax candle, strokes on the hand with absorbent cotton and near the genital region with a ruler, 171 hypnotic dreams were recorded—only 40 of which are printed in this monograph. Rearranged and reworded, the author's main conclusions are: (1) Hypnotic dreams are essentially like sleeping dreams, and differ from associations that result from asking waking subjects to dream (a control run with three subjects) in being "real," "present," and in having a *Gestalt*-like pattern, to which the stimulus is assimilated and lost. (2) The average hypnotic dream—records for 84 dreams—ran for 32.5 seconds. (3) Sex dreams were rare, only 9% of the total; the Freudian distortions non-evident. (4) The existence of typical dreams in stereotyped situations, (*e.g.*, the dream of falling at tilting the bed, of smothering at covering the face) and of recurrent dreams by a given person with the same stimuli, was confirmed. (5) Dreamless sleep may be merely amnesic sleep. (6) The results bear out Hollingworth's idea of the redintegrative nature of thinking—the purpose underlying the series of experiments.

The method and the procedure are admirable and the results and conclusions are very suggestive. It occurs to the reviewer, however, that contrary to the author's findings, many hypnotic subjects do dream spontaneously, being *en voyage;* and that the very suggestion to dream and to report the dream as soon as it is over may constitute not only a fictitious setting, but also an artificial beginning and an arbitrary ending of the dream. The patterns of the hypnotic dreams may be smaller, of shorter duration, less complex and much more definite, and even qualitatively different because the antecedents in hypnosis—including the instructions—are different from the antecedents in sleep. Perhaps the very nature of the dream itself is changed by being on the alert for a dream, instead of being intent on sleeping. But even though it were shown that the results of these experiments could not be predicated of normal dreaming, they would still be interesting and revolutionary.

Louisiana State University PAUL C. YOUNG

The Child of Circumstance. By ALBERT WILSON. New York, William Wood & Co., 1929. Pp. xx, 420.

This book represents, the reader is blandly told, a surgeon's matured reflections on crime and his scientific determination of its organic and structural concomitants. It is difficult to treat the elaborate pretensions of this work seriously, for everywhere the psychological naïveté of the writer is painfully manifest. Such a work may be ascribed to the curious and pathetic belief, held less strongly at present, that medical training qualifies one for pronouncement on anything, especially if the thing could be dubbed 'scientific.' The reader is dismayed by the enviable ease with which 'The Psychology of the Criminal' is disposed of in two pages; the equally facile classification into 'types;' and finally the assured reduction of all criminal behavior to defects in the structure of the brain.

As criminology, psychology, and biology the claims of this book are very dubious, indeed. But as a significant document on the aberrations of the scientific mind it may have some value perhaps, for it shows that a scientific technician, like a surgeon, may easily live in two hermetically sealed worlds: one in which all the safeguards of scientific procedure are enforced, and another in which only the terms, the symbolic forms, of science are present without meaning.

University of Louisville ELLIS FREEMAN

Teaching in College and University: A Survey of Problems and Literature in Higher Education. By C. V. Wood. Baltimore, Warwick & York, Inc., 1929. Pp. xi, 557.

This book, as the sub-title indicates, is a general and necessarily superficial survey of the problems and literature in higher education; it also records mere opinions, several of which are unimportant and are given by individuals who themselves have done no experimental or other important work on the problems. This is probably a necessity for the sake of completeness of such a survey, and because problems and opinions are not easily differentiated. The two chapters dealing with the psychology of learning and with measurement and guidance touch most directly the work of psychologists but they omit many valuable contributions and contribute nothing specifically. It is true, of course, that several psychologists are now doing administrative work of importance. The book should be valuable to all workers in higher educational institutions, chiefly because of its immense classified bibliographies, those in the Appendix alone covering 83 pages; and yet the reviewer finds the bibliographies of the decade covered (1918-1927) very deficient so far as the more serious experimental investigations are concerned. Much useful psychological material and important scientific techniques have been omitted. It is probably such experimental procedures which ultimately will be invoked to settle most of the problems considered. The professional educator has yet a long way to go to bring his problems under experimental control and handling.

J. P.

Methods of Private Religious Living. By Henry Nelson Wieman. New York, Macmillan Co., 1929. Pp. 219.

The chapters of this book first appeared under the title of "Experiments in Personal Religion" during the year 1927-8 in the monthly bulletin of The Institute of Sacred Literature published at the University of Chicago. The book lends itself to classification as a phase of applied psychology. It constitutes an attempt to apply certain methods to a definite sort of human activity.

The theme of the book as a whole is that of progressive integration. There are nine chapters, eight of which are devoted each to a particular method, while one, the ninth, considers several methods. These methods are offered as guides "in personal experimental ventures of religious living." Of the various activities employed in making these methods effective, the following are illustrative: social adaptation and organic association; relaxation; self-analysis; auto-suggestion; the acquisition, evaluation, and organization of facts; developing habits of alertness, plasticity, and fearlessness; developing habits of experiencing beauty; holding fixedly before the mind some belief or ideal until it exercises "a powerful influence upon the individual;" and problem-solving by "exposing oneself to the stimulus of a problematical situation with a mind freed of all bias and preconception, and waiting in this state, or returning periodically to it, until there dawns upon the mind that integration which will solve the problem."

The book is excellently organized, and should be of interest to psychologists as indicating an effort to apply psychological principles, by one who is not primarily a psychologist, to a definite form of human activity.

Syracuse University Ernst Thelin

Psychologische Untersuchungen über das Stiefmutterproblem: Die Konfliktmöglichkeiten in der Stiefmutterfamilie und ihre Bedeuting für die Verwahrlösung des Stiefkindes. By HANNA KÜHN. Hamburger Untersuchungen zur Jugend- und Sozialpsychologie, No. 1. Leipzig, J. A. Barth, 1929. Pp. viii, 162.

A new series of which this is the initial study takes for its field the important facts connected with the personality of the adolescent and his behavior in social situations. The present investigation belongs, both in its conception of problem and its method of investigation, to the last decade of the nineteenth century. The effort is made to isolate the single factor, the presence of a stepmother in a home, as a source of conflicts, and to determine the significance of such conflicts on the personality and the behavior of the stepchild. The answer to this two-fold relationship is sought in the replies of children to questions, in compositions on the general theme under consideration, in rather complete documents from a juvenile court, in biographies, literary references, personal interviews with children, and the like. In each case the reader is given only quotations to support an interpretation.

The scientific world has outgrown the method here used as a source of dependable knowledge. The study makes its most important contributions when it lifts into relief problems that require a precise formulation and an objective method for their solution and this careful survey of what people think and believe abounds in such problems. The extent to which popular opinion regarding stepmothers affects the behavior of every member of the new family group and the growth of personality under conflict situations are examples of such problems.

Florida State College for Women P. F. FINNER

The Neuroses. By ISRAEL S. WECHSLER. Philadelphia, W. B. Saunders Co., 1929. Pp. 330.

This book was written primarily for the medical student and the practitioner. It is of considerable value, however, to the psychologist whose interests are in this specific field of abnormal psychology.

The author discusses the various schools, but he always has a psychoanalytic leaning. In fact, Freudian designates his point of view better than psychoanalytic. He says that more than 90% of what constitutes genuine psychoanalysis was evolved by the originator himself.

In the chapter on etiology, which is one of the most interesting, we get the author's methods as well as his leaning. He says: "It is true that psychoanalysis furnishes at present the best insight into etiology of the neuroses, but there are a number of other theories, each of which has more than a grain of truth" (p. 89). He then proceeds to discuss and, to some extent, to evaluate the following theories: the somatic or pathological, the physiological and the behavioristic theory, the glands of internal secretion theory, the vegetative nervous system theory, the suggestion, the social or biologic, and the psychoanalytic theory.

The book is well written, interesting, and stimulating, but it is likely to be of little value to the person not well grounded in the various fields of psychology. The different theories described are not criticised and evaluated sufficiently, and the book assumes too much of the nature of an encyclopedia.

University of New Mexico B. F. HAUGHT

An Anthology of Recent Philosophy: Selections for Beginners from the Writings of the Greatest 20th Century Philosophers. By D. S. ROBINSON. New York, Thomas Y. Crowell Co., 1929. Pp. xii, 674.

The author has attempted in this volume to bring together for the beginner in philosophy—and also for non-professionals who wish to know something of what philosophers are saying—a representative selection of "the most readable" recent philosophical literature. The reader who takes the term 'anthology' too strictly will soon become aware that he is in for a serious piece of work, but he will doubtless find it interesting if he is naturally inclined toward philosophizing. First he is given on a single page a conspectus of the various "isms" and other types of modern philosophies—idealisms, realisms, pragmatisms, mechanisms, vitalisms,—a veritable "flower gathering" of the products of 'emergent' philosophers. It is freely admitted by the author that because of this selection according to types many worthy philosophers not exactly fitting these types are omitted. It appears reasonable, once you turn your attention to these 'emergents,' that in philosophy as in psychology (witness *Psychologies of 1925*, and our recent 'philosophical' discussions filling hundreds of pages in modern psychological books and journals) only the more distinguishing 'types' shall receive attention and analysis. A great service has been done for the reader by Robinson. Preceding every excerpt is found an excellent short and clear analysis or résumé of the selection, and after the latter are a number of questions which seem to be, in general, rather good to bring out a thoughtful consideration of the contributions of the selection as well as the point of view and the classification of the author (philosopher) who has contributed it. In this latter task the reader is helped by the first selection in the book, James' interesting and suggestive Philosophy and Temperament, *Pragmatism*, pp. 1-13 and 33-40. The author of the *Anthology* has made a real contribution in these features of a preceding analysis and really helpful questions, and the reader is asked to do his part in turn by reading each analysis and selection thrice,—first simply for general interest; secondly, to correlate analysis with excerpt and to try his hand at writing out a good analysis himself; and, thirdly, "to find out the answers to the questions at the end and to form his own opinions of what the author says." This last reading will, of course, help the reader also to see that any author's questions are more or less from his own point of view, total independence from such a more or less personal or 'school' slant being impossible to any author (especially the author of any 'philosophy').

Part I gives a general orientation; Part II exhibits the idealisms or "tender-minded" philosophies, to use James' characterization, and they do ring somewhat true to this characterization in the view of a humble layman like the reviewer; Part III presents, in the framework for tough-minded analysis already described, the realisms; and in Part IV we find the pragmatisms. It is well that the tender-minded get the stage first, for most of the later "isms" are exhibitions of more or less "tough-mindedness." To this statement, however, some exceptions must be made, for the reader will find in Part V, which is too recent, and perhaps too 'terrible,' to be characterized in a single term (here, by the way, is work for some future 'emergent'), along with the wicked and surely tough-minded "mechanisms," such selections as Emergent Evolution by Driesch, The Unknowable by Spencer, Fictionalism from *The Philosophy of "As If"* by Vaihinger, and the so-called New Scholasticism and Its Contributions to Modern thought, by Ryan.

The reviewer has added this hyphenated word "so-called," because he has found in the book nothing particularly new except that it appears in this 20th century, but he may not have delved deeply enough, not being a philosopher.

An additional valuable feature of the book is an appendix of biographical and bibliographical sketches of the philosophers from whose writings excerpts have been taken. The author seems to have done a creditable piece of work, and his *Anthology* will doubtless appeal to many readers other than beginning students in philosophy. Many scientists will doubtless find recreation in going over its pages.

J. P.

Fundamentals of Educational Psychology. By I. M. Gast and H. C. Skinner. New York, Benj. H. Sanborn & Co., 1929. Pp. xiii, 354.

The material of this text is presented under the conventional chapter headings and every chapter is organized under a number of sub-headings. Every sub-topic is in most cases a unit in itself, and often there is little attempt to make a transition from one sub-topic to another. This gives the impression that the book is a series of disjointed sub-topics rather than a collection of developed chapters.

The authors do not hesitate to include material which bears upon a given chapter but which is more fully treated elsewhere. Thus, the first chapter includes not only a description of original nature but also a discussion of methods of modifying instinctive tendencies and a treatment of imitative and volitional action. Material belonging primarily to the chapter on transfer of training is presented in the chapter on habit. "Problem of attention" appears in the chapter on the learning process. Material on instincts and individual differences is introduced into many chapters. This breaking down of the logical barriers between chapters is perhaps advantageous from a pedogogical standpoint. On the other hand, the overlapping between the chapters on habit, the learning process, and memory seems to the reviewer hardly justifiable.

The book cannot be classified as to its central point of view. This is as the authors intended, for one of their aims has been to place "greater emphasis upon the significant contributions of the many psychologists regardless of viewpoint and less emphasis upon particular viewpoints." It seems safe to say, however, that they regard the individual as a product of 'will' as well as of heredity and environment. Although they assert that heredity and environment cannot operate independently, they place the emphasis decidedly upon heredity. Instincts are treated as drives, and the assertion is made that "they serve as a basis for all education." The treatment of learning follows that of Thorndike rather closely.

Another of the authors' aims has been to give "a frank expression of sane opinion concerning certain topics that have been misinterpreted or overemphasized in certain quarters during recent years, *e.g.* intelligence tests, the significance of test results, new-type classroom tests, endocrinology, heredity, and behavioristic psychology." The chapters on intelligence and educational measurements, which appeal to the reviewer as the strongest, fulfill this aim very well for the first three of the above examples but why the last three should have been mentioned is not clear. Heredity receives greater emphasis than is customary even in texts on educational psychology and endocrinology and behavioristic psychology are scarcely mentioned.

The outstanding feature of the book is the special attention given to the social and applied aspects of the subject. This has led to a great deal of generalization, quotation of opinion, and moralization. The result is that any psychologist will find much with which to disagree, the student will perhaps wish for greater concreteness in many of the applications, and if he is not inclined to be credulous, he may desire more support for many conclusions. Nevertheless the authors have, in the reviewer's opinion, taken a long step in the right direction in some respects and have achieved a practicality greater than that which is characteristic of many other texts in the field.

Stephens College CARL N. REXROAD

The Intelligible World. By WILBUR MARSHALL URBAN. New York, Macmillan Co., 1929. Pp. 471.

The author develops the theme that philosophy is intelligent discourse about the world and that the only intelligible language is the idiom of the Great Tradition. The secret of philosophical wisdom is in the intellectual orientation toward the fine minds of all time. Any concept of totality that shall satisfy the demands of philosophy must possess meaning and value, as well as reality. In fact every problem of metaphysics resolves itself into one of value. The only tolerable conception is of the world in the form of "idealism," that is, spiritual content is the fundamental reality. The traditional conception of time and space pictures them as phenomena of a deeper non-temporal, non-spatial order of meanings and values. "The One Spirit's plastic stress is the only element of unity or universality that can be found in the world process. Evolution *makes the world intelligible to us only* (italics mine) when we conceive it, either consciously or unconsciously, as evolution of a divine idea, as a gradual revelation in time of that which was present from the 'foundation of the world.' "

Teleology, finality, and purpose, properly understood are a necessary part of the world. Evolution and progress are in truth transcendental or metaphysical, or they are nothing. A decrease in the amount of available energy in the world entropy the author interprets as a problem not of metaphysical but of cosmic order, being nothing more than the cosmic application of the significance of death. Entropy, he contends, does not hold true for life and mind. Immortality has always been a symbol for the conservation of value. It is in this sense only that endless life has been a necessary postulate of morals and values. The only world possible to intelligent reflection has as its constituent parts Matter, Life, Mind, and Spirit.

Salient conclusions from the speculations afford comfort to those in sympathy with the dualistic point of view in psychology. The arguments depend considerably on experience for their validity, though the point of view is not scientific. Noteworthy spiritualizations of experiences are the timeless-present, 'instinct of philosophy,' immanence, and omnipresence. The book should appeal to the vitalist as a scholarly return to the idealism of the past in the light of the present. In contrast, to the modern critic it should appear, perhaps, as a learned examination and organization of many types of human conduct, making value stand out as one of the great present day problems.

University of Utah M. C. BARLOW

The Art of Interrogation. By E. R. HAMILTON (with an introduction by C. Spearman). International Library of Psychology and Philosophy and Scientific Method. New York, Harcourt, Brace & Co., 1929. Pp. xii, 174.

As the title of the book suggests, interrogation, in the form of tests of intelligence or of subject matter, is an art as well as a science, though a very young and imperfect science. The author has selected for discussion three general topics: namely, mental tests, examinations of the old style and new, and the questioning conducted by the classroom teacher.

The book is quite free from technical terms and altogether free of statistical devices, being intended, evidently, for the non-technical reader; especially, it appears, for the teacher. It is the author's purpose to consider the basic psychological principles of interpretation of tests; for, in his own words, "though examinations are of such long standing and intimately affect so many people, a critical survey of the principles governing their use has but seldom been attempted." The author develops his thesis chiefly by means of presenting some of the main principles of test construction, such as the determination and control of selected mental processes, selection of representative items, correlation of processes, consistency of conditions, quantification of results, and so on.

In connection with the remaining two devices—old and new type examinations and questioning—it is the author's thesis that examining is an art, having at the same time "a scientific basis which is imperfectly understood." He expects psychological research to make this understanding less imperfect as more is revealed concerning learning and expression, knowledge and ability, examiner and examinee.

The non-technical reader will find in this volume a clear statement of some of the important psychological principles underlying various types of testing, and teachers of all grades will find suggestive principles and criticisms of interrogation, both oral and written.

Cornell University F. S. FREEMAN

Cornell Studies in English XVI, Gianfrancesco Pico della Mirandola: On the Imagination. The Latin text with an introduction, an English translation and notes. By HARRY CAPLAN. New Haven, Yale University Press, 1930. Pp. 102.

Pico's work on the imagination is an essay on a psychological subject, which is here fittingly presented by a student of the classics in a series of English studies. This combination of interests matches the complex nature of the original work. Pico was a humanist, and the humanist of his period (1470-1533 A.D.) was a man interested in the classical tradition, given to scholarly contemplation, and not so far penetrated by the new sciences as to ignore the work of 'supernatural agencies.' All these factors appear in this little work.

In the history of psychological literature imagination or phantasy occupies an important place. It mediates between sense and reason, for its material is given by the senses, but it is itself an inner activity. As the images survive the moment of sensation they become objects of reflection, partly dominating the acts of reason and partly used by reason to embody its more abstract visions. Desire is allied with imagination, and for that reason the subject leads into questions of conduct. The physiological part is supplied by the ancient doctrine of temperaments, against which the reason sets the control of ideals in the form of imagined good or evil. Pico is obviously humanistic in the sense that he aims to make his psychology the basis of a practical ethics.

The work naturally appeals more to the historian of ideas than to the modernist, who cares only for the latest fruits of science. Pico thinks the variety of imaginations depends on the temperament of the body, the sense affections, the judgment and the ministration of the good and bad angels. The last source effectively dates the work. Pico thinks disorders of the body must be cured by treatment of the body. The others he would treat by what we may call forms of sublimation: like some modern psychologists, he relies on religious emotion to achieve the most effective integration of life. His views on the use of heaven and hell, that children may be "delighted by the one and saddened and terrified by the other," are tempered with a flash of the experimental method when he says: "I have discovered by experience with my son that stories of this nature have had this effect," and he anticipated Bacon's views that in education concrete pictures have more power than abstract reasoning. Ignoring the severe standards of modern science, let us acknowledge the urbanity and charm of Pico's essay. The work of the translator has been well done. The notes are valuable and show competent knowledge of the sources and the subtle transitions of mediaeval thought from the purely classical to the mixed Neo-platonic, Arabic and Christian teaching. The printing is exceptionally accurate and the whole production does credit to the industry and scholarship of the author.

University of Toronto G. S. BRETT

Twins: Heredity and Environment. By NATHANIEL D. M. HIRSCH. Cambridge, Harvard University Press, 1930. Pp. 159.

The author has written a treatise on twins which will interest both the scientist and the layman. There are brought together in the first chapters the salient biological and psychological facts concerning twins. A chapter is devoted to a review of most of the important experimental studies upon twins. With this introduction Hirsch launches into a 'fresh' attack, which, far from yielding simple descriptive data, is to disentangle the effects of heredity and environment. The method is simply that of experimental selection, surely nothing new and a procedure to be followed with the greatest caution.

Data are gathered upon dissimilar twins living in a similar environment (58 pairs), similar twins living in a similar environment (38 pairs), and similar twins living apart (12 pairs). Obviously criteria of similarity and dissimilarity were required. For the twins, these criteria were provided by physical and mental measures. These seem quite adequate. For the environments, similarity was thought to obtain when the twins lived under the same roof. This selection is questionable.

Each pair, having been selected, is described with respect to certain anthropometric measures (height, weight, cephalic index, etc.) and mental measures. In addition there are often given data concerning handedness and other traits. Much of the descriptive material is subjective and much is drawn from the estimates of school teachers and others. It is doubtful whether this material is to be trusted. One is impressed either with the similarity or the dissimilarity of twins.

In the former case there is doubtless an unintentional stress, on the part of teachers, social workers, and friends, on likenesses; in the latter instance contrasts are readily drawn though differences may not actually be very great. The subjective estimates of character, temperament, and emotionality are thus driven to extreme statements. However, this kind of material is not of such a nature as to make general conclusions possible. The author has concerned himself therefore with those traits capable of quantitative expression and interpretation. After summing up the measures certain contrasts are made. Comparisons, showing little statistical reliability, yield the sweeping conclusion that heredity is several times as important as environment in determining the extent of the differences found between members of a twin pair. Obviously the yield is a function of the selection process and this will be found upon examination to be not above criticism.

In the last chapter the author makes an impassioned plea for change in the social order.

University of Virginia FRANK A. GELDARD

King Mob: A Study of the Present-Day Mind. By FRANK K. NOTCH (pseudonym). New York, Harcourt, Brace & Co., 1930. Pp. 226.

This volume, like a telegram, is an urgent, brief communication. Much must be read between the lines. The author recognizes a consuming struggle between individuality and collectivity, between nature and spirit; individuality and spirit being the things consumed. He fears that nature and its counterpart, collectivity, are reducing culture to a meaningless mechanism. The holocaust now impending has its historical precedents. But, in consequence of modern instruments of communication and the perfected mechanics of exploitation, the present-day mind represents a complete surrender to the Moloch of "a thrilling awareness of number."

One lays down this important book with a loss of respect for democracy: the voice of the people is not the voice of God, but the meaningless urgings of some as yet unknown "elementary law of nature." It might reasonably be concluded that this elementary law of nature is an interpretative rather than an explanatory principle: "The enmity between the Mob and the individual thinker . . . [is] a war in which the weapons of the moment are fortuitous, but in which the opposing energies flow from the inexplicable dualism of the world" (p. 13).

In the light of this general principle the author surveys the American mind in respect to literature, science, commerce, and national and international politics. And there he finds nothing but a reservoir of stupidity, ignorance, and self-deception. With pardonable dignity he represents himself as a prophet. His inferences are drawn from a wide and reflective observation of recent social tendencies. The reviewer surmises, however, that the author is not entirely unprejudiced in his attitude toward certain modern popularizers of our evolving cultural products.

The volume makes no pretense of being a contribution to social psychology as science. It does, however, represent a clever combination of the philosophies of Tarde, Le Bon, and Durkheim. Drawing upon them (perhaps unconsciously), the author portrays our contemporary America as a product of universal repetition, collective stupidity, and group coercion. His style is lucid, his reasoning generally consistent. The reviewer is, however, unconvinced of the validity of his premises.

May it not be that the alleged universal war on individuality by the mob is something other than the author finds, perhaps the very opposite of it? May not this collective 'taking to the new' really mean a collective search for individuality? Those who refuse to be drawn into intellectual stampedes about social movement generally will be inclined to answer these questions in the affirmative.

University of Maryland J. W. SPROWLS

The Vibratory Sense and Other Lectures. By DAVID KATZ. Orono, Maine University Press, 1930. Pp. 163.

These lectures, written in semi-popular form, are based mainly on the works of Katz and his students at the Rostock Psychological Institute. Experimental and observational studies, mostly with animal subjects, indicate that while the physiological state of the organism conditions hunger to some extent, hunger depends to a high degree on the external circumstances in the immediate environment.

Children are classed as "voluntary vegetarian" and "voluntary carnivorous" on the basis of expressed preferences for different kinds of food. However, since no history of the development of individual food habits appears to have been considered, this classification can have little meaning.

In studying facial characteristics a composite photograph of ten intelligent and of another of ten unintelligent children were made and compared in various ways. It is quite possible that this technique is not valid. The results obtained in various investigations on judging intelligence and personality traits from photographs suggest the possible hazards involved in an analysis from composite pictures of facial characteristics which accompany intellectual traits. The method is, of course, not new.

Although the author warns the reader against Darwinian anthropomorphism, he rather uncritically describes the character of hens in such terms as "she was very envious," and "she did not like to be the subject of examination," and even tends to approve of anthropomorphic interpretation of animal behavior.

Other topics considered are (1) localization of sound by dogs, cats, and hens; (2) the "vibratory sensation;" and (3) conversations of children.

The book as a whole contains much information and stimulating observation. Psychologists and others will find it an interesting and valuable treatise.

University of Minnesota MILES A. TINKER

Folkways in China. By LEWIS HODOUS. London, Arthur Probsthain, 1929. Pp. vi, 248.

This volume is a collection of material on folklore and customs gathered from personal observation by the author during long residence in China, from conversations with Chinese and other residents, and from "wide reading" in Chinese sources, *e.g.*, the *Book of Rites*. The thirty-seven short chapters cover a great variety of topics dealing with ceremonies and feasts, observances relating to celestial phenomena and seasonal changes, to agriculture, great alleged historical events, war, sacrifice, etc. There are no specific references and the reader is given no means of evaluating the data as to validity. The book, though interesting to students of human nature, is not scientific. General references and a list of Chinese names are given at the end of the book. J. P.

Great Experiments in Psychology. By Henry E. Garrett. New York, The Century Co., 1930. Pp. xvii, 337.

The book has been planned as a second text for the elementary course. It aims to supplement standard texts by presenting concretely the experimental foundations of psychology.

The "great experiments" include the work of Binet, Terman and other testers, the army alpha, Ebbinghaus' studies in memory, Pavlov's investigations upon the conditioned reflex, Thorndike's animal experiments and laws of learning, the work of Thorndike and Woodworth upon transfer of training and formal discipline, Watson's experiments with infants, Galton's studies of individual differences, Cattell's researches on reaction time, the experiments of Cannon and others upon emotion, Köhler's investigations in perception and learning, the laws of Weber and Fechner, studies in space perception, and the work of Franz and Lashley upon brain function during learning. Each field of experimentation is treated broadly and with a wide historical perspective. Earlier and later work is brought into the same picture with excellent balance and arrangement.

The treatment throughout is accurate and dependable. A great deal of material has been organized in a way which will inspire the alert and industrious student. Each chapter closes with a few carefully selected references. There are eleven plates portraying the psychologists whose work is considered in the text; and thirty-five figures.

This book should give the elementary student a sound and thorough introduction to psychology *via* experimentation, and leave him with a wholesome respect for the subject. The reviewer is delighted with the volume and hopes it will be widely used. In addition to other merits of the book, the style is lucid and very readable.

University of Illinois — Paul Thomas Young

University of Iowa Studies in Psychology, No. XIII. Ed. by C. A. Ruckmick. Psychological Monographs, Vol. XL, No. 181. Princeton, Psychological Review Co., 1930. Pp. vi, 214.

Space does not permit giving both full abstracts and detailed criticism of each of the papers in this monograph; nor does the importance of most of the papers warrant any deviation from this rule. The reviewer will therefore limit himself to brief remarks, allocating them to their proper fields in psychology and attempting to rate their relative excellencies.

Harold R. Fossler. Disturbances in Breathing During Stuttering (pp. 1-32): The presentation of this paper is excellent both in clearness of style and ordering of the material. The importance of the paper consists chiefly in an improvement in technique. Previous experimenters in this field have caused their subjects to stutter by making loud sounds and bringing about other emotional disturbances, all of which adds irrelevant deflections in the graphic records. Fossler requests his subjects to talk freely about some past experience and takes his stuttering as it comes. The only criticism of the method is that these past experiences are sure to have different emotional contexts, which somewhat defeats the improvement in technique. Furthermore, no complete record of this free exposition is taken, as could easily be arranged with a dictaphone. Even though the author finds that special words do not bring on stuttering, the breathing curves mean very little without some correlation with the *S*'s phraseology and ideas expressed.

William Severt Larson. Measurement of Musical Talent for the Prediction of Success in Instrumental Music (pp. 33-73). The abilities tested by the Seashore tests may well be necessary in varying degrees to performers whose instruments demand special acuity in certain fields as supplements to their instruments, but they are by no means criteria of musical talent. The latter lies more on the conceptual level, and does not demand anything exceptional in the way of acuity. A deaf Beethoven and a Clara Schumann distinctly hard of hearing can compose or perform superlatively, but many a piano tuner, with well-nigh perfect pitch, can do neither. This analysis makes understandable the fairly high correlations found by Larson and other experimenters in this field, for naturally a performer on a stringed instrument has to have a good ear or he will fail, no matter how great his endowment of musical talent may be. But more important still, it explains why even in the most proficient group tested by Larson there are students rated by the tests in the last two percentiles of all the groups, who apparently are doing satisfactory work in music. These students would be weeded out, if Larson's recommendations were followed, long before they had reached this stage. This would be manifestly unfair to them and it would perhaps render music a decided disservice.

J. T. Nielsen. A Study in the Seashore Motor-Rhythm Test (pp. 74-83). The paper is very unclear—no statement even of the rhythms used is made. The author finds that both musically trained and untrained *S*'s improve but slightly during the first three days in their ability to tap out a rhythm sounded in the ear. Considering that the music students improve somewhat more than the others in spite of their previous training, it may be inferred that this period is largely spent in getting used to the experimental situation and the problem set. All except one of the music students rate higher in adeptness than the others, so that the author feels that another criterion of musical talent has been found. The reviewer is not so confident.

Emily Patterson. A Qualitative and Quantitative Study of the Emotion of Surprise (pp. 85-108). The qualitative parts of the paper may prove of value, but the quantitative sections, since they are derived by a galvanometric technique, for which there is as yet no accepted explanation, must remain open to question. The procedure is doubtful in one particular, at any rate. The *E* leaves *O* alone in the experimental room to set the Wheatstone bridge and to read his own "normal resistance." But *O*'s emotional state during this period is usually far from normal, ranging in different *O*s from sleep to terror; and since the value thus obtained is apparently used in later computations, a large source of error may have entered with it.

Alfred R. Root. Pitch-Patterns and Tonal Movement in Speech (pp. 109-159). To the reviewer, this paper is by far the most important in the volume, for it is not only a contribution to phonetics, but should give psychologists in general much food for reflection, in that it compares the physical frequencies applied to the ear with the phenomenal experience, and finds many striking disparities.

The implications for phenomenal psychologies are wide. The method should surely be carried over into music psychology to see how many of the characteristic slides and glides found by Metfessel are actually perceived as such.

Arnold Henry Wagner. An Experimental Study in Control of the Vocal Vibrato (pp. 160–212). The paper should be addressed to singing teachers rather than to psychologists. Its aim is to discover whether 'artistic vocal vibrato' may be taught. It may be.

Cornell University R. S. Hill

Studies in the Organization of Character. By Hugh Hartshorne, Mark A. May and Frank K. Shuttleworth. New York, Macmillan Co., 1930. Pp. xvi, 503.

This is Part III of *Studies in the Nature of Character* by the Character Education Inquiry, Teachers College, Columbia University, in coöperation with The Institute of Social and Religious Research, and it concerns itself with many of the same traits, situations, and tests as the preceding parts. Additional tests of inner traits and attitudes are given and treated in the same careful, objective, scientific, statistical manner that characterized the earlier studies. In this volume, however, the object is not to study and measure traits and situations themselves, but to discover and measure their relations to each other in the structure of character. The authors remain committed to the idea that all their tests are of particular ideas, acts, and situations, and valuable chiefly because of variety of sampling. Their final selection of a battery of twenty-three tests as a basis for a total character score, with directions for administering and scoring, are not included in this volume, but are published separately by the Association Press, 347 Madison Avenue, New York City. The theoretical validity of the group of tests, as computed, is 0.95. Detailed tables, formulas, and bibliography are given in the appendices. The index is for this volume only.

The authors discuss the relation of social intelligence and social attitudes as indicated by tests and they seek to determine the sources of moral knowledge and the influence of concomitants such as age and social intelligence as well as of environmental influences, such as home, school, Sunday school, and clubs. They investigate the interrelation of these factors, and reach the conclusion that conduct is more a matter of group standards than of individual traits. The components of character as revealed by reputation scores, foresight tests, combined pen portrait tests, and the relations of these to actual functioning in work and play are considered.

In studying the nature and significance of integration, especially in relation to the honesty tests, the authors find that only about 40% of children show integration as measured by consistency in the various honesty situations tested, and these in only a slight degree.

In the opinion of the reviewer, the small amount of consistency revealed by the test is due in part to adult lack of consistency in applying standards, which is reflected in the scoring of acts as more or less moral. The authors find a close, though small, correlation of indices of consistency with the total honesty score. They do not, however, find evidence that children consciously generalize from particular situations, and integrate in accordance with ideal standards. The claim of the authors that the integrations are specific may be allowed without accepting the view that children do not try to apply general standards to particular cases. They, as well as adults, try to do so, but are likely to succeed only in lines where group practices guide them in deciding what acts are to be regarded as consistent with the general ideal.

The chief conclusions regarding character education are that present methods of moral training may do more harm than good, when ideals and the factors influencing conduct in practical situations are contradictory; and that the principal difficulty in character education of children, is the conflict of the standard set with the factors in the situation which naturally control the child's action. If the child were continually a member of a group observing standards of general social usefulness, the problem of education would be greatly simplified. It would then be necessary only to provide for integration of individual character by guiding children through a series of experiences graduated as to opportunity for acting in accordance with standards, and of graduated temptation to diverge from them.

This book completes one of the most notable researches in character education of recent times. The foundation is laid for a science of moral education, and well standardized tests are provided for further research. Probably no one, in this time of chaos of traditional and reasoned moral ideals and practices, could have performed the task attempted better than the authors have done, but when our mixed codes of morals have been revised in the development of a science of ethics, there will probably need to be a complete revision of all these tests of moral character.

Leominster, Mass. E. A. KIRKPATRICK

Formen des Gemeinschaftslebens jugendlicher Mädchen: Sozialpsychologische Untersuchungen in einem Fürsorgeerziehungsheim. By GERTRUD HEERMANN. Hamburger Untersuchungen zur Jugend- und Sozialpsychologie, No. 2. Leipzig, J. A. Barth, 1929. Pp. viii, 160.

This monograph describes the social structure that developed in an institution of 55 asocial young women, ranging in age from 14 to 20 years. The most inclusive social unit is that based on rank or influence in the group; rivalry for position and power ranges the members into a series of zones ranging from the most to the least effective. Within this totality minor social units appear: associations of two or more friends, the clique, groups for some purpose, groups stimulated by outside leaders, and others as they are commonly described by the social psychologist. These groupings, as well as the social behavior of the individuals, are interpreted as the normal expression of the persons in the particular environment that is provided. The characterization of each individual gives, in the main, the essential facts for tracing the resulting behavior and might well be read by those who are giving us large masses of undigested and often meaningless facts on problem children.

Students of social psychology will want to use this investigation to see the operation of the principles of social behavior in a group form not described in earlier literature. With other studies that examine carefully and minutely a small section of life, it will tend to lend precision to the large generalizations that constitute the substance of many treatises on the subject. To profit by the study it will not be necessary to accept the presuppositions of the writer. Stern's view is apparent throughout. There is an unnecessary stress on examining the individual as a whole, and refraining from analysis and the investigation of aspects of the person. In fact, the discussion frequently bases conclusions on views that can come only from highly specialized experimental enquiry. For example, the statements on the respective nature of two individuals who will become friends in a given situation give the occasion for an experiment on an aspect of behavior rather than undifferentiated observation. The relation of drives and motives to the behavior studied can hardly be regarded as fundamental. We shall not advance our insight into human life by basing interpretations on the generalization that there is a struggle for existence in adolescent years or that "the longing for the understanding and like-minded person . . . . can be regarded as the leading drive in social formation."

Florida State College for Women P. F. FINNER

The Psychology of Religious Awakening. By ELMER T. CLARK. New York, Macmillan Co., 1929. Pp. 170.

Much as those questionary studies of conversion, made thirty years ago, have been condemned for their crudity of research technique, attempts to improve upon them have been rare indeed. That they should be early repeated was apparently believed by some at least of those who made them. With the passing of the years reasons for their repetition have accumulated. Lehman and Witty have shown the great change in collecting habits over the same interval, and the question has been properly raised if there may not have been like changes in the frequency and modal age of religious conversions. Clark's re-study here presented is therefore most welcome.

The method used was again that of the questionary. The questions show some effort to profit by the experience of those who made the early studies, but they manifest little influence of the extensive development of questioning technique which has come in the last decade. Returns were available from 2174 people, mostly students in small colleges. The data are excellently presented both in tables and in graphic forms. None of them seem to have been omitted. They are all there for future reference and comparison, a feature which will appeal strongly to those who have tried to do much with the earlier studies. The age curves verify that indication of a drop in the modal age of religious awakening already brought out by the Indiana Survey. But they do more. The returns are classified into types—definite crisis, emotional stimulus, and gradual. By tabulating the returns according to each of these forms separately the drop in the modal age is revealed to be characteristic of the last two only; the modal age for the crisis variety of religious conversion remains about where it was when Starbuck and Leuba and Coe and others made their studies. That there is a conspicuous decline in the frequency of the crisis variety is not surprising, but its definite determination is valuable. The relationship of different kinds of religious training, especially the conservative versus the modernistic, to the different forms of conversion is carefully worked out and given in detail. The treatment in general is cautious and convincing. Much as the statistically minded might wish for a more elaborate treatment of the data, or for data collected in a manner permitting a more elaborate treatment, the reviewer is inclined to the opinion that this book must be accepted as the most important contribution to our knowledge of the conversion phenomenon that has appeared in a generation at least.

Psychologists who use the book will soon discover that the author is a clergyman rather than a psychologist. He speaks the language of religion more easily than that of psychology, and this leads to the usage of such phrases as "the sense of joy, peace, and happiness" and "the sense of increased responsibility" which are likely to irritate the trained psychologist. Perhaps if he had been a psychologist first of all the author might have sought to discover relationships to mental ability and academic achievement, to measures of social adjustment and fair-mindedness, or to measures of emotional stability (topics which the book will make every psychologist wonder about); but then again, had he been a psychologist he might not have done the job at all.

University of Oregon — Edmund S. Conklin

Psychologie der Waarneming. Een Studie over Gezichtsbedrog. F. J. T. Rutten. Nijmegen, N. V. Dekker & Van de Vegten, J. W. Van Leeuwen, 1929. Pp. 154.

This monograph was presented as a doctor's thesis to the University of Utrecht. The classical Müller-Lyer illusion was chosen for experimental exploration with a view also to the formulation of a general theory of perception. A rather complete account of the types or variety of figures used in earlier investigations (sketches of which from pages 6, 8, and 9 may well be copied for demonstration material) is rendered in the introductory chapters together with a critical historical review of earlier studies and theories. The technique of the experimental set-up is described in minute detail. It involved two lanterns focused toward the middle of a white screen 1 meter square in a dark room; the exposure-time was controlled by motor and rotating cut-out disks which allowed variation in two ways. Great care was taken to consider and to rule out all disturbing or variable factors, an endeavor being made to keep brightness, and size of figure, duration of fixation, and distance, under perfect control. The two original figures of the Müller-Lyer illusion were used in turn with length of accessory lines in 2/5 relation of the main line and with angles of the accessory lines of 30, 45, and 90°. (Three other variants of the figure were also tried.) The size of the figure could be varied from 4.5–100 cm. The distance of *O*, who was seated between the lantern and the fixation-point on screen, along the middle axis, varied from 20 to 350 cm. A table gives angles of vision from 2 to 78°. Twelve different exposure-times from 10 to 1000 sigma were tried. The figures were projected both horizontally and vertically in varying order of sequence of main and accessory lines. Two trained and seven untrained *Os* served during the experiments. The general duration of one sitting was one hour, with rest periods intervening. The instructions were as follows: "Lines will be projected in front of you; you will see them together for an instant. Tell me then if you perceive a complete figure and describe it, *i.e.*, how everything appears, behaves, and disappears." These instructions appear highly suggestive to the reviewer, and they may even partly explain the results obtained as due in no mean measure to the influence of the *Aufgabe*, bearing in mind especially the 7 untrained *Os*. It might be that if the instructions had been: "Something will appear in the visual field; you are to describe your experience in detail," the results would have conformed more to the *Gestalt* theory of perception or would have been near to those effected on the basis of Bentley's theory of organization. The study may, in this regard, serve to illustrate the all-importance for the formulation of any theory of perception of the attitude and the *Einstellung* instilled in the *O*. This crucial point has hitherto hardly been given its due attention. Under the conditions described above the author finds four phases of the formation of the figures: At the exposure of the figure and its parts, movements similar to those discussed by the Gestalt school were observed. It seems, however, somewhat arbitrary and dangerous to divide these movements into the two classes, (1) phenomena which are the consequence of the tachistoscopic exposure of an object, and (2) form-phenomena which must be considered as a result of the tendency to formation of a new whole from collective data, *i.e.*, as the *Durchstructurierung* of the observed complex. There is, in point of fact, no possible criterion for such a discrimination. The author's explanation of the Müller-Lyer illusion, as well as his theory of perception, finally ebbs out in an organization of 'meanings.' With its careful technique, its almost complete history of the illusion, the study will take its permanent place in this classical field.

Wittenberg College M. L. Reymert

The Growth of Reason. By Frank Lorimer. New York, Harcourt Brace & Co., 1929. Pp. xii, 231.

The sub-title of this book is: A study of the Rôle of Verbal Activity in the Growth of the Structure of the Human Mind. The volume makes a distinct contribution toward placing the psychology of language and of reasoning upon a scientific basis. Scientific 'approaches' are offered to studies in symbolism, in reasoning and in social psychology. Fortunately the author knows biology, psychology, and philosophy, and apparently enjoys the art of keeping an even keel while steering for his goal. New and difficult terms are defined with unusual clearness. The book abounds with new insights and new points of view without being radical or strained. The mind-body conundrum, the symbol-meaning bugbear, language and higher thought processes are among the problems admirably handled.

The contents appear by chapters and are expressed in analytic form, and the bibliography, also arranged by chapters, is placed at the end of the text.

Skidmore College Linus Kline

Principles of Experimental Psychology. By H. Piéron. Translated by J. B. Miner. New York, Harcourt Brace & Co., 1929. Pp. viii, 190.

The latest French edition of Piéron's book appeared in 1928, and was reviewed in this Journal (41, 1929, 124). We now have it translated into English by Miner. Thus it is available for general student use as a text or for reference. Miner's translation is both adequate and pleasing. The sense of the French text has been rendered with utmost accuracy into English which retains the style of the original without an 'accent.'

Hobart College Forest Lee Dimmick

The Growing Boy. By PAUL HANLEY FURFEY. New York, Macmillan Co., 1930. Pp. viii, 192.

In *The Growing Boy* Furfey presents a very simple and readable account of the social characteristics and play interests of boys between the ages of six and sixteen years. Chapters I, II, III, and VIII deal with growth in general, the concept of developmental age, development during the first six years, and adolescence. The remaining six chapters are given up chiefly to a series of brief case studies of the play behavior of boys of different ages. Throughout the book, the fact that social development is a continuous process which is not marked off by any very sudden or discrete steps is emphasized. Developmental age, which is the writer's term for "the progressively increasing maturity of behavior which shows itself in the child's changing interests and in his whole behavior" shows an age-overlapping which is quite similar to that found for stature or mental age.

The value of the book is much enhanced by the well-selected list of references at the end of each chapter. It should form a useful addition to the library of the elementary teacher or the lay reader who is interested in child development.

Institute of Child Welfare FLORENCE L. GOODENOUGH
University of Minnesota

A Syllabus Work-Book for a Two-Level Plan of Instruction in Elementary Psychology. By H. H. REMMERS and G. C. BRANDENBURG. New York, Macmillan Co., 1930. Pp. 110.

This work-book contains a series of assignments in Gates' *Elementary Psychology* and Dashiell's *Fundamentals of Objective Psychology*, with supplementary questions and readings from other authors. There is also a list of exercises or more specific questions to be answered in writing. For the more able or more ambitious students optional assignments are suggested in the way of reports on additional readings, observations or exercises. These are more general in nature.

While the work is an attempt to take care of the more able students in the same classes with the less able, the method may not prove the most advantageous one, however, as there is little recognition that the able student knows much of the work at the start and may actually need a more advanced course rather than additional assignments beyond the usual ones.

University of Minnesota M. J. VAN WAGENEN

Sleep: Why We Need It and How to Get It. By DONALD A. LAIRD and CHARLES G. MULLER. New York, John Day, 1930. Pp. xi, 214, 13 charts, 2 tables.

This is an interesting and amusing book, popularly and carelessly written in semi-journalistic style, and resplendent with anecdotes. It is apparently of little scientific value since experiments and procedures are not described so that they could be repeated, and exact data are omitted. It has an appendix giving a brief 'Description of Typical Experiments' and a bibliography of 81 titles which are not referred to. There is no index.

University of Minnesota C. R. GARVEY

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COMPETITION IN CHILDREN: AN EXPERIMENTAL STUDY

By PEARL J. GREENBERG, University of Vienna

The literature on competition is plentiful. The importance of this human inclination is everywhere acknowledged, and it is everywhere exalted as one of the prime motive powers in the accomplishment of the world's work.[1] The *nature* of competition is perhaps the subject of an equal amount of discussion, and here agreement is not so universal. There is a tendency among psychologists to try to determine whether competition is a primary instinct, or merely a human tendency that can be developed by nurture and directed by social relationships. The argument depends much on terminology and the terms vary with schools of thought. A decision between the contestants has no especial significance for this investigation. For the purpose of clarity of thought, however, it seems advisable to take a definite stand in the matter of definition. So, throughout the following pages, competition will be understood to mean that human tendency which consists of *a desire to excel, an impulse to do better* than our rivals.

Some studies in rivalry have already been made, although the field has not been extensively investigated. Our present research is in the field of child psychology, and we shall report particularly our findings on work in the field of experimentation with children. The experiments of Moede are those which first come to mind.[2] His comments on rivalry are based on a series of experiments in which a child tried to attain a good result on a dynamometer. The child tried with all his might; but when a second child was brought into the situation, his power in-

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[1] This work was carried out under Dr. Charlotte Bühler, Director of Research of the Psychological Institute of Vienna. The author wishes to thank her, and her assistant, Dr. Hildegard Hetzer, for their valuable suggestions.

[2] W. Moede, *Experimentelle Massenpsychologie,* 1920, 1-73.

creased and he did better. Moede proceeds to enlarge on the effect of the contest on the contestant and on the group.

In her study of the popular or folk plays of children, Hetzer discusses the competitive plays of children. She found that before the age of 3 yr. rivalry or competitive plays were absent. Between the ages of 3 and 6 yr., 8% of all the child's play is competitive. She says in part:

> In the plays of early childhood blind chance and the age of those playing together decide the social position of the individual child. The distribution of the various 'offices' of the game is determined, for the most part, by the rules of the game. Individuals have no opportunity to change their social position by personal merit. Contests of this type are found first in the plays of later childhood, in the group plays of comrades of equal age. After the sixth year of life the contest plays take on an ever widening aspect. Bodily skill above all, but mental skill as well, help to win. The ability characteristic of the ideal leader of certain age-groups is preëminent here. With boys at the age of puberty, bodily power, pluck, and courage come first; with girls success depends on drawing, singing, roller skating. Before the tenth year this sex difference is lacking. In early childhood 'rivalry plays,' and in later childhood 'neutral companion plays,' move into the background.[3]

A very careful and exact study of despotism and rivalry in babies has been made by Charlotte Bühler.[4] It is concerned with the behavior of children from 4 to 22 mo. of age, and is a detailed study of how children of this age seek to dominate the situation and thereby display their despotism. In this situation two children of varying age relationships were placed together and a toy given to one of them. Bühler says that if you expect an immediate battle for the toy you will be disappointed; "the child who does not receive the plaything is satisfied to look on, to see what the other child does with the toy, for at this time the looking gives him as much, or even more, pleasure than grasping. Also, when an older child takes away a plaything from the younger, the younger is so completely occupied with looking that ceasing to have and grasp the thing does not yet cause him to suffer and seems in no way to injure him."[5] She continues, "Despotism between children springs from other causes and events than prevail between the child and the grown-up. The play situation between two children coming together for the first time is almost completely neutral. When there is only 'looking contact,' one can observe the neutrality of the relationship. It is with grasping that the 'collision' begins."[6]

She traces the development of despotism in these youngsters, the attempt on the part of one child to impose his superiority on the other by the strength of his activity. Her findings are as follows: (1) The child of the first half-year does not yet enter into a relationship of despotism and rivalry. At this age rivalry is incidental to the object and despotism of the older child. (2) In the second half-year rivalry and despotism are found vigorously developed. (3) Age relationship plays a very definite part in the stages of development of rivalry and despotism. Only children of approximately the same age (maximum difference 2½ mo.) rival each other, and the older child is almost always the despot.

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[3] H. Hetzer, Das volkstümliche Kinderspiel, *Wiener Arbeit. f. päd. Psychol.*, 6, 1927, 75.

[4] C. Bühler, H. Hetzer, and B. Tudor-Hart, *Soziologische und psychologische Studien über das erste Lebenjahr*, 1927, 1-102.

[5] *Ibid.*, 55.

[6] *Ibid.*, 56.

American psychologists likewise have interested themselves in the subject of rivalry, and their investigations throw light also on this hitherto obscure but important subject. Hurlock investigated the effect of incentive on performance, and reports that children do better after either praise or reproof than after mere repetition.[7] Ross has shown that motivation is greater for bright children than for dull ones.[8] Both studies corroborate the empirical psychological statement that competition is an energizing and stimulating human impulse under whose influence the best work is often accomplished.

To sum up the preceding opinions: they seem to indicate that competition is an extremely important human impulse, playing a vital part in life's work; that competition directly influences and vitalizes the quality of this work; that competition and pugnacity or despotism are very closely related, though not identical; and that pugnacity appears to precede competition.

OBJECT

The object of the present investigation was to study a competitive situation, and to determine: (a) whether, in a given situation, competition always exists; (b) when, in that situation, the tendency first occurs; (c) whether, if present, it exists always in the same degree; (d) how it manifests itself; and (e) what causes it to vary.

This report falls into two parts: (I) An evaluation of the degree of competition displayed in the child's performance when he uses concrete material in a specific situation. (II) An analysis of the factors causing the variations in the manifestation of competition. This analysis includes a discussion of competition as an inclination developing normally and in definite relation to certain laws of growth; a consideration of the part education plays in the growth of competition in childhood, and an evaluation of the part that personality and individual differences play in the exhibition of competition.

METHOD AND PROCEDURE

The greater part of this experiment was carried out in the department of Kleinkinder of the Kinderübernahmsstelle in Vienna, although children in two other kindergartens were also observed. Children from the age of 2–7 yr. were included. The experimenter entered the room where the children ordinarily lived, and choosing according to her needs the ones with which she wished to work, asked them if they would like to build. In practically 100% of the cases the answer was, "Yes." If not, that child was not urged against his will to go with *E*. Two children were then taken into a smaller room, where they would be less disturbed by others. Here they found a small improvised table, a rather wobbly one, and on the table a pile of building blocks. These blocks were smaller and more varied in size and in shape than the ones with which they were familiar.

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[7] E. B. Hurlock, The effect of incentives upon the constancy of the I.Q., *Ped. Sem.*, 32, 1925, 422-434.

[8] C. C. Ross, An experiment in motivation, *J. Educ. Psychol.*, 18, 1927, 337-346.

The material offered the interest and challenge of novelty and of a little more difficulty than that of the material already familiar. On the other hand, it was of a sort with which any child could do something, even if only to handle it. The factor of uneasiness with *E* was absent because of her frequent previous contact with the children. Even the very little children always came eagerly to build.

The time, the place, the material, and the *E* were constant in this experiment. We were not, of course, constantly dealing with the same children from week to week, since they remained in the institution only about three weeks. From one point of view, however, the *type* of child was 'constant' and represented in most cases the milieu often designated as proletarian. But the children's parentage comprised every possible mixture of type and quality, from the point of view both of heredity and of environment. Most of the children came from poor homes, but a cross-section of their homes represents exceedingly well a cross-section of society in general.

The children, therefore, of varying ages and of varied home background, were brought two at a time into a small but very pleasant room, and seated on their own little stools opposite each other at a table with building blocks. The blocks were piled in the middle of the table and were not divided between the children; each child could reach out and take as many as he needed or wanted. For the most part they were glad to play with the blocks or build with them, which suited *E*'s purpose very well. Without instructions beyond being encouraged to go ahead and make something, they were permitted to build freely at will. The construction was considered finished when either child said, "I have finished," or when he had used up all the stones. When both children had finished they were asked to come and stand by *E*'s side and to look carefully at each other's construction. Then they were asked, "Which is prettier?" This first construction, carried out without an assignment, and inclusive of the child's answer to the question, "Whose is prettier?" was considered the first part of the experiment.

After they had answered the question the children were permitted to build again, but this time they were given an assignment. *E* pushed all the blocks into the center of the table again, and looking at both children said, "I would like to see who can build prettier this time?" or "Would you like to see who can build prettier?" In some cases the children built a third time, the assignment then being, "Let me see who can build bigger."

The activities, the remarks, the gestures, the whole mood of the situation, were recorded in the protocols as completely as possible. The name, sex, and age of each child were registered and a record kept of all data concerned with the experiment. In the 2–3 yr.-old group there were 2 boys and 6 girls; in the 3–4 yr.-old group 8 boys and 7 girls; in the 4–5 yr.-old group 5 boys and 6 girls; in the 5–6 yr.-old group 11 boys and 9 girls, and in the 6–7 yr. old group 7 boys and 4 girls.

I. BEHAVIOR IN COMPETITION

The constructive situation of the experiments has two basic elements or components out of which the problem of competition arises. The child cannot escape from some sort of contact with either the other child or the material. He has been placed in a social situation wherein he builds with another child, and he has been given blocks as material with which to work. Competition is concerned with both, because out of the child's relationship toward the other child develops the inclination we are here calling competition. This statement may be reversed: the child indicates the degree of competition which he experiences by the way he behaves toward the material and toward the other child. In this section of the paper, therefore, we shall devote ourselves to an examination of the child's behavior in these two respects, and from this examination we hope to gain criteria by which we may evaluate the degree of competition the child is exhibiting.

(*A*) *Relation to material.* The child's relation to the material is not constant; his reactions change not only as the experiment is repeated or varied but also during the progress of any one building. He comes to the situation for the first time, fresh, and his behavior at the very first contact with the material seems to convey a definite significance. To keep this reaction unadulterated *E* gave him no directions. She simply asked him, "Would you like to build with some blocks?" When he answered "Yes," she brought the two children into the room and into the situation already described.

(*a*) *Early behavior.* What did the child do when he sat down? At least one of several quite different things: he (*1*) merely *played* with the blocks, or (*2*) did *nothing* with them, or (*3*) began at once to *build.*

(*1*) *Played.* When first confronted by the situation, some children began immediately to play with the blocks. This is an exceedingly characteristic and unmistakable type of behavior. Such a child did not hesitate for a moment. He was scarcely seated when he touched the blocks, drew them toward himself, pushed them away again, lifted some up, let them drop, twirled them around in little circles, etc. These movements were mostly arm and shoulder movements, at times involving the hand, but almost never employing the smaller muscles of the wrist or fingers. The child's attention in these cases was given to the material not as something with which to work, but merely as material with which to do something, and from the constructive standpoint his movements were undirected and non-specific. In 50% of the cases this mere manipulating continued throughout the experiment; in the other half, the child passed from such undirected functioning into a more constructive handling which at times closely resembled building. But even when the child did eventually build, his behavior could be characterized by the word 'uncertain.' Not 1% of the children at this stage were found to give these uncertain constructions a name.

(*2*) *Did nothing.* To the group just described belong the children between the ages of 2–3 yr. There seems to follow in the slightly older child (3–4) a period wherein he does not attack the material at the very start, but delays handling the stones and gives his attention to his surroundings. He looks around the room, makes remarks about things or persons in the room, or else merely sits still and does nothing. He appears not to see the blocks as a special object of attention, and does not begin to touch the stones until the other child has begun to build. Take for example the following protocol:

> Boy, H.E., age 3·3. While companion builds steadily, H.E. looks around, makes remarks on the toys in the room. Hears some water running and turns toward it. Listens to it, keeping his head turned in that direction. Knocks over other child's building (unintentional, occurred through movement). Coughs. Talks about stones. Yet when asked whose is prettier he said, "Mine."

(3) *Built.* In the third type of behavior the child goes immediately to the blocks and handles them constructively, making something out of them. These cases were very numerous and appeared to increase with chronological age.

Table I and Fig. 1 show the relation between age and the frequency of the three types of behavior just described. It is not hard to understand why the child of 2 or nearly 3 years of age begins at once to manipulate the stones. The infant made his first random and undirected movements without any relation to an outside object. As the child develops and comes into contact with material, this progression from non-specific to specific reaction is repeated with each new material. Consequently the young child to whom these building stones are a new material, and whose building experience is very limited, makes toward that material undirected and non-specific movements. The 3-yr.-old has discovered that manipulation has an end-result, and that out of the material he can make 'a thing.' This interest in the material leads the child to see what he can do with it and he builds. The figures show a very interesting rise in the percentage of children whose attention to building grows, starting with 37.5% in the child of 2 yr. and reaching 90% in the 7-yr.-old. When, however, the child of 3 yr. is arriving at a phase of development wherein he is interested in effort with the material, he is also approaching the social age. He appears more interested in his

TABLE I

RELATION BETWEEN AGE AND TYPE OF BEHAVIOR

Age-Group (yr.)	Percentage who do nothing	play	build
2–3	0.0	62.5	37.5
3–4	31.6	21.5	46.9
4–5	28.7	14.3	67.0
5–6	0.0	22.7	78.3
6–7	10.0	0.0	90.0

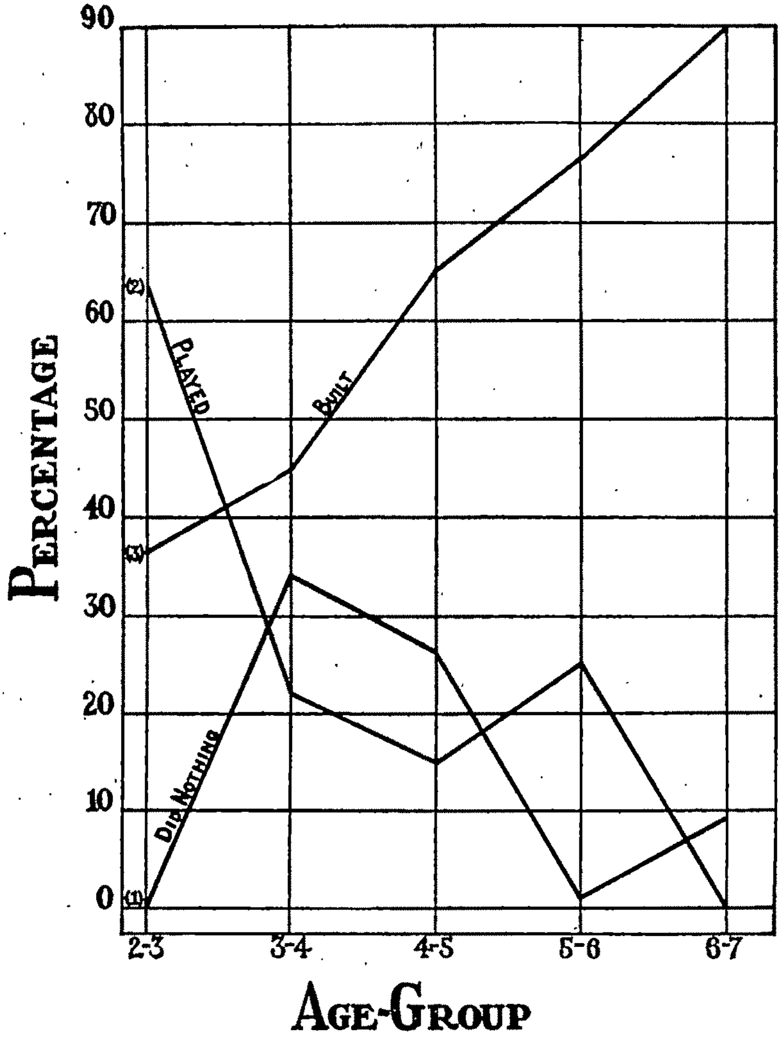

FIG. 1. EARLY BEHAVIOR WITH THE BUILDING MATERIAL

surroundings, in the other child, in other people, in the other things in the room, than in functioning through the blocks alone. The percentage of children who sit before the blocks and do nothing with them rises from 0% in the 2-yr.-old to 31.6% in the 3-yr.-old. The percentage falls when the child reaches the age of 5 yr. and is apparently familiar with the material. Then he wishes to begin work at once with it.

The child's behavior at the very start, then, is integral with his understanding of the problem. The child of 2 yr. does not see the blocks as material; he sees them as things with which to function. The child of 3–5 yr. already recognizes the material as stuff for work, but his interest in the other child is perhaps the stronger influence. He does definitely recognize the blocks as a creative medium, and passes rapidly from a functioning to a working behavior, all the time being even more aware of the other child in the situation. Thus, as has been said, at the very beginning of the problem the child's behavior falls into three types: functioning with the material; doing nothing with the material (at the start); working with the material. The correlation between the child's *mastery of the material* and his recognition of the *idea of excelling* will be discussed later.

(*b*) *Behavior during building.* The child's behavior with the material does not, however, confine itself to what he does at the beginning of the experiment. The experiment continues, and by a study of what he does later in its course we can obtain some further idea of the progress of his mastery of the material and the correlation between mastery and competition. The casual observer might say that surely if the child builds at all he merely uses the material. But close observation distinguished some interesting variations in the way he handles the material. The reference is not to the skill with which the child uses the blocks but rather to the manner in which he *reaches out* for them. Muscular behavior seemed to have a direct and rather close relation with the feeling-tone of competition, as Table II indicates.

(*1*) *Used as needed.* There were a large number of children who simply used as much of the material as they needed, reaching out for blocks one or two at a time, either taking those nearest at hand or choosing the ones which seemed best fitted to the construction they were making. This kind of reaching is itself varied, of course, and depends on the child's development and skill; but it is mentioned here as one type of behavior towards the stones, in which the child "using the material for his needs" is responding to the *need for stones* and not to the *social situation* or to the impulse of competition. He is fulfilling his needs in the interest of the work.

But this type of action does not always obtain. Without attempting at this point to analyze the cause of these variations, but merely to describe them, it may be said that while many children simply used the material as they needed it, others very definitely exhibited one or two other kinds of action: 'grabbing,' or 'giving.'

(2) *Grabbed.* The child who 'grabbed' reached and took what lay on the other child's side of the table. There can be no doubt that such a child was exhibiting something different from a need for material. It was indeed a need for certain material, but it was a need attended both by a disregard for what belonged to the other child and at the same time by an anxious regard for what the other child would say in response. This high-handed disregard was demonstrated by such a startling act as the child's standing up, reaching right into the middle of the other child's construction and taking what he wanted. At other times the child took stones surreptitiously (say from a little pile the second child had gathered ready for use), and watched all the time to see if he was observed. There were cases where he took many stones and hid them slyly in his lap, and also where the recognition of grabbing led to out-and-out quarreling. The close relationship between the competitive impulse and the instinct of pugnacity was dramatically clear.

TABLE II

BEHAVIOR WITH BLOCKS DURING BUILDING

Age-Group (yr.)	Percentage who grabbed	gave	used as needed
2–3	0.0	16.6	83.4
3–4	0.0	5.2	94.7
4–5	30.7	0.0	69.3
5–6	36.4	13.6	50.0
6–7	27.3	18.2	54.5

(3) *Gave.* A more subtle piece of behavior and one a little more difficult to analyze was the 'giving' of stones. At times this giving was in response to a request for stones. But oftener it was a spontaneous giving, apparently divisible into two types. One was a mere social giving; another child was in the situation and the child thrust stones toward him. The second was in response to a recognition of the other child's need of the stones, a recognition which is involved in an understanding of the situation. At times the child offering the stones would observe the second child searching or looking about. Then he would say, "Do you need this?", handing him at the same time a stone. Generous, however, as this act appears to be, it is closely connected with the child's recognition of the problem, and is bound up with the impulse of competition. For while the 2-yr.-old stage does show in the following table a greater number of children giving stones than appears from 3 to 4, it must be remembered that this giving is largely a muscular movement, just a shoving of the stones toward the other child. In the next age-group, when the child shows many other signs of social interest, that is, looks more at the other child, talks to him, etc., he does not yet give him stones. The reason for this will be discussed in the next section of this paper. Let it suffice to say here that while he is interested in the other child he does not yet recognize him as part of the building situation.

There is one other comment to be made on the way the child behaves with the material. The child of 2 yr. at no time asks for stones; while the child of 6 does so with a very clear indication of conscious action. This very simple observation has, however, a deeper significance, and gives a general view of the whole development. The child passes from a mere stimulus-response reaction and equally non-

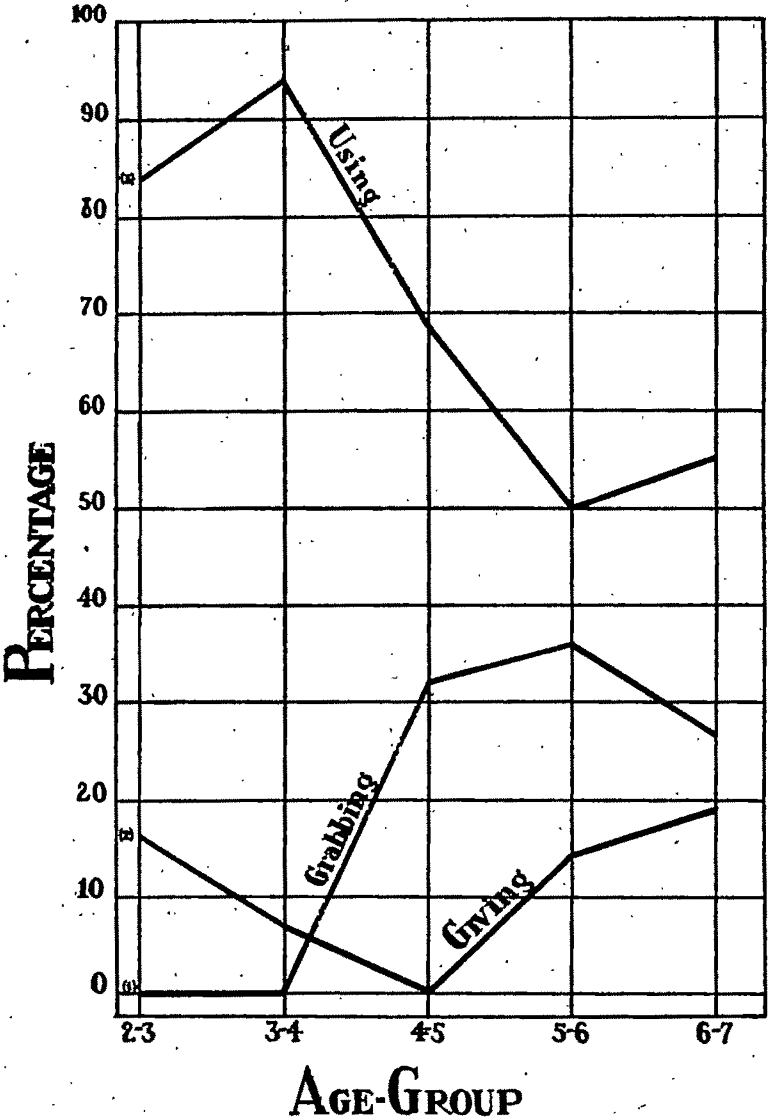

FIG. 2. BEHAVIOR WITH THE MATERIAL DURING BUILDING

thoughtful giving through the steps of (a) neither giving nor taking. (b) then a decided grabbing, (c) finally a stage where he does take what he wants, but also asks for what he needs, and thus exhibits fully an understanding of the problem.

(c) *Interest in work.* We may include as indicative of the degree of competition the child's attitude toward his own work. This is, of course, the most definite of the indices, but at the same time the most subtle. For while it must be described in terms as concrete and definite as possible, still it involves what is called by some psychologists 'a feeling-tone.' That is, cases frequently occurred where *E* 'felt' competition more keenly than she could describe it. She was finally able, however, to isolate and organize some of the phenomena that suggested 'competition.' These phenomena were *(1)* degree of attention, *(2)* physical signs of interest, *(3)* remarks about own and other's work, and *(4)* child's judgment concerning the work.

(1) Attention. It seems logical to regard the degree of the child's attention as a measure of his interest. In order to determine, therefore, whether a child was exhibiting interest in the building process, we used the quality of his attention as one means of measurement. The child was said to be attentive if he directed himself to the blocks and either manipulated them or built with them throughout the experiment. Such a child attacked the problem with eagerness and at times even with force, and remained at his task until he had completed it. Sometimes he gave passing attention to extraneous sounds or intruding persons, but such attention was momentary and relatively superficial. His conversation, too, concerned itself with the work in hand and not with his social life or other interests. That is, of course, an extreme picture; there were many children who could not measure up to this high standard and yet gave the observer the impression that they were interested. I shall give here two protocols to make my meaning clear:

TABLE III

Percentage of Children Showing Positive Attention to the Blocks

	Age-Group				
	2–3	3–4	4–5	5–6	6–7
Attentive	75	40	71.5	81	90
Inattentive	25	60	28.5	19	10

An inattentive child.

Girl, E.J., age 4·0. Does not build. Does not play with the stones. Gazes around the room. Looks at Fritz. Wiggles in her seat, looks out into the hall. *E* asks her, "Can you build?" She answered in a slow sleepy voice, "Oh, yes!" *E*, "What are you building?" Answer, "A house."

The other child pointed to E.J.'s stones and said, "That isn't a house." She answered, "Yes, that is a house." At the question, "Whose is prettier," she did not answer, merely pointed to the other child's construction.

An attentive child.

Boy, H.H., age 3·6. He says: "There! I am making a train." Talks continually to himself as he works, but keeps on working and does not deviate at all from the building. "So, now I lay something here. Oh, Oh!" Puts a stone on the construction. Goes on building without looking up. Seems to have his train finished. Says, "Express train. An express train. Well! Here's a bridge." (Points to it.) Talks to himself as he builds, "A boat is sailing under the bridge." *E* asks, "Are you finished?" Answer, "Not yet." In a few moments he says, "Now I am finished!" Gets up and comes to side of *E*. At question, "Whose is prettier?" he looks at both and finally points to his own.

Let us regard this from the quantitative standpoint and see how the percentage of attentive responses varies with the age-groups of the children.

It can be readily seen from Table III that the child of 2–3 yr. of age was attentive to the blocks. It should be noted that he did not build with the blocks, but rather manipulated them and busied himself with them. He however gave his attention to the blocks. The slightly older child, the child from 3–4, was distracted by outside sounds and sights. He gave attention readily to any passing disturbance. From then on attention increases until the child of 6–7 exhibits 90% interest. The existence of even this 10% of disinterest was doubtful, and must be considered again when we look for the more personal causes of variation.

(2) *Physical signs.* A little easier to observe were those physical signs by which the child seemed to show unmistakably when he was especially interested. We shall try to show the relation between the physical manifestations and the feeling-tone accompanying competition. As soon as a child became aware of the real meaning of the situation, these physical signs showed themselves very clearly and sometimes a child who when building for the first time (that is, without an assignment) had been quiet and relaxed, became restless and intense under the excitement of competition. Sometimes rather slow movements changed into rapid ones, and relaxed muscles stiffened. The child experiencing the competitive impulse sat more erect in his chair, sometimes on the edge of his seat. He reached out with short, jerky, motions. Color increased and often the eyes brightened, although quite often the lids came down over the eyes, either to increase the surreptitiousness of his glance toward the other child or because of the tenseness of the eye-lid muscles. Children exhibiting this competitive impulse at times rose out of their seats—two actually exchanged seats.

Looking through the protocols I find many phrases such as the following:

[Protocol 6] Laughs, puts tongue between lips. [47] Body tense. Jumps up and down. Claps hands, sings. [55] Every added stone brings a glow to his face. [56] Child cries and says his building is "horrid." [79] Increases speed. Doesn't stop till all the stones are used up. [80] Makes many restless movements of the body. [112] Can scarcely wait to begin. Eyes sparkle. [124] Jumps up and down. Begins to get excited. Talks very loud.

Thus in many physical ways the child proclaims to the onlooker that he is not passively manipulating the blocks, nor even merely using them as a material means to fulfill his building interests, but that he is aware of a social situation and has a desire to excel his partner, a will to succeed. Not all of these physical movements need be consciously directed toward the goal, but one does feel that a child acting in this manner is bending all his energies to the purpose of surpassing a competitor.

(3) *Child's remarks.* Many children showed by their remarks that they were quite aware of the problem, that they understood the situation, and that, further, they wished to excel.

A few examples must suffice. For the sake of clarity I have organized them into groups, but they all seem to have one underlying theme, self-exaltation, with all that this implies:

(a) Remarks about the need of certain materials so as to continue the work.

"I need two big stones."—"I want a longer one."—"Have you some more of these very little stones?"—"You have so many, and I have so few!"—"I need many, many, many."

(b) Remarks about the sort of thing they are making.

"I am making a train."—"I like to make only towers."—"I can also make a garden."—"So! Here is a window."

(c) Remarks on their own work showing pride or desire to excel.

"A big house!"—"Look! Isn't mine pretty?"—"Look at me!"—"I'm not finished. I must work very hard."—"Bigger! Bigger! bigger! bigger!"—"My stones don't fall over, I give attention."

(d) Extraneous remarks reflecting glory on themselves.

"My mother is going to get me when she has money."

Table IV shows the percentages in which these different classes of remarks occurred.

TABLE IV

PERCENTAGE OF REMARKS ABOUT OWN WORK

	Age-Group				
	2–3	3–4	4–5	5–6	6–7
Positive	0	10	26.6	43.4	70
Negative	0	0	0.0	0.0	10
None	100	90	74.4	56.6	20

An analysis of the causes of these variations will be made later. The development proceeds, of course, from the absolutely uncritical attitude of the child of 2 yrs. of age to the much more critical attitude of the child of 7. The 7-yr.-old includes some negative remarks about his own work.

The tendency to make remarks has a direct bearing on the display of competition. The child who says nothing at all about himself, does not understand the situation; he does not see that the situation is a competitive one. He shows what he feels by what he says. The child who exclaims, "Sister, look, look at mine. How nice it is!" gives an unquestionable picture of a child awakening to a desire to excel. The child who says nothing probably feels nothing, unless, of course, he shows feeling by some other means.

(4) *Child's judgment.* In the child's judgment of his own work, we might expect to find another means of studying the competitive interest. *E* found it very hard, however, to understand these replies, for it seems probable that the assertive ego in the child is the strongest feeling he has and makes it difficult for him really to compare his work with that of another child. Each time after the children had built they were asked to stand for a moment at *E*'s side and to look thoughtfully at each other's work. Then they were asked, "Whose is prettier?"; or, "Whose is bigger?" The answers were by no means always logical, and seemed to be actuated by almost any motive except a decision according to worth. The answer of the 2-yr.-old child was interesting. In 58.3% of the cases he did not understand the question, and either did not answer at all or repeated the last word of *E*'s sentence, *e.g.*, *E*, "Which is prettier?" Child, "Prettier." *E*, "Who made the best building?" Child, "Building." Most of the remainder of the 2-yr.-olds answered, "Mine," without apparently thinking about the matter or weighing their answer. So far as *E* was able to analyze the situation, there was no correlation in this age-group between the child's building ability and his answer.

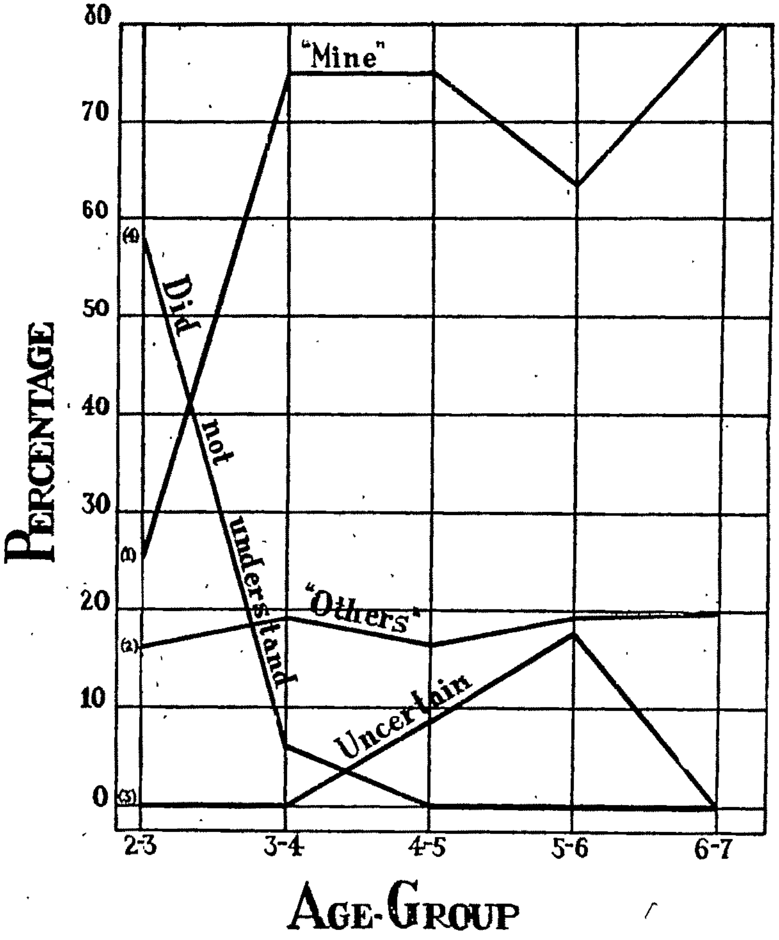

FIG. 3. DISTRIBUTION OF VARIOUS TYPES OF REMARKS

	Percentage Every Age-Group				
	2-3	3-4	4-5	5-6	6-7
"Mine"............	25.7	75	75	63.4	80
Did not understand	58.3	6.2	0	0	0
"Other's"..........	16	18.8	16.7	19	20
Uncertain.........	0	0	8.3	17.6	0

Likely the answer depends on his comprehension of the words used. If the child of this age understands the question, the answer, without hesitation, is "Mine." There was one exception to this generalization; this was the case of a boy who was not particularly interested or attentive, and who simply pointed to the other child's construction. In the next age-group (3-4 yr.) there were still some children (6.2%) who did not understand the question. Nearly all the children of this group answered, "Mine," without any hesitation. It may seem a contradiction that children who are at the most social age should prefer their own work without giving the work of the other child a thought. But a study of the situation reminds us that this social relationship has, as yet, nothing to do with the problem in hand. So he talks to the other child, looks at him, is thoroughly aware of him, yet if you ask him, "Whose building is prettier?," he says, "Mine."

There were a few isolated cases where the child replied by pointing to the other child. In two cases this occurred in children approaching 4 yr. of age (3·9 and 3·11, respectively); these were children who were building with a sister, and moreover, an older sister. In a third case the second child was older. In *no* case did a child consider the work of a younger child better than his own.

With the 4-5 yr.-old child the kind of answer remains much the same. At this age-level, 75% of the children consider their own work the better; 8.3% are uncertain, and 16.7% consider the other child's better. The percentage answering, "Mine," is higher in the next age group. Of the children between 5 and 6 yr. of age, 81% finally considered their own better, although of these 17.6% were uncertain just what to say. 19% of this age group considered the other child's prettier. The still older child (6-7 yr.) displayed less hesitation; 80% said, "Mine," without hesitation, and 20% indicated the other child's work.

Fig. 3 shows the distribution of the various types of remarks among the different age-groups.

It may be seen first, that the child of 2 yr. either does not understand the problem, or answers, "Mine." Secondly, that understanding of the building situation rapidly increases, and by the age of 4 yr. every child appears to know what is meant by 'prettier' or 'bigger.' Thirdly, that at no age-group in this experiment did the child seem to be greatly interested in the other child's building. We have already observed that he was much interested in the other child, but when it was a question of construction, his own work demanded his attention. Fourthly, that there is a rise in the percentage of children who consider their own work better.

These figures do not tell the whole tale, however, and we cannot leave the topic of the child's attitude without giving some attention to the differences contained within that answer, "Mine." The numbers do show a high percentage of children who answer this way. But we wish to call attention to the difference in the quality of this "Mine." Note the difference in the feeling one gets as to *thinking* on the part of one child and *no thinking* on the other:

Girl, C.K., age 2·6. Pulls many stones toward herself. Talks constantly but does not build. Looks around. Plays with stones. Talks to stones. Clean wash comes in. She remarks on the wash. Plays with stones. Finally builds a little.

Question is put, "Which is prettier?" Says without hesitating or thinking, "Mine."

C.K. gives no evidence of understanding the problem as a competitive one. She merely gives an answer. The child in the following protocol cannot build either, but shows both by her remarks and by her manner of answering that she would like to excel.

Girl, R., age 3·6. (with K., age 6·0.). Very diffident. Does not seem to know how to begin. *E* helps her to build as an encouragement. She does not yet build. *E* says, hoping to stimulate her, "I would like to see who can build better." She still does not build. *E* says, "R. and I will build." R. says, "I will make a train." Other child says, "I can also make a train." R. says sneeringly, "That isn't a train." *E* asks of both, "Whose is prettier?" R. points to construction which *E* built, and says, "Mine."

Still more indicative of the competitive impulse well developed is the following protocol:

Boy, E.A., age 4·6 (with H.G., age 3·11). *E*, "Would you like to build?" E.A., "Yes." Builds with speed and interest. With gusto says, "I am soon finished." Points to H.G., "He isn't finished yet." When both children have finished they stand by *E*. *E*, pointing to H.G.'s house, "Isn't that pretty?" E.A.,: "Yes, but once he hit me with a stone!" *E*: "Whose is prettier?" E.A., quickly, "Mine."

It seems from the foregoing examples that the answer "mine" does not always have the same qualitative value. Hence, the percentage of answers to that effect does not indicate the degree of competitive impulse. All that we can say definitely from the above graph is that verbal behavior is clearly correlated with the other indications of competition.

(*B*) *Relation to the other child.* We have thus far discussed the child's relation to the material; now we shall consider the responses to the other child.

(*1*) *Watches.* There were cases in which the child did not appear to observe the other child at all. This was true of the very young child who came to the table and was barely seated before he began to lay hands on the material. He appeared to notice nothing except that there were stones which he could manipulate. For example:

Girl, C.K., age 2·6. She is brought to the room where she finds table, chair and blocks. She seats herself at once in the first chair she comes to, the one nearest the door. Without waiting she begins at once to manipulate the stones. She touches them, shoves them about. Some stones fall; she picks them up. She talks constantly in an indistinct mumbling which cannot be understood. Jigs and jumps about with stones. She bumps up and down in her seat. Pulls some stones toward herself, shoves them away again. Pays no attention to the other child. *E* asks, "What are you making?" No answer. Goes on manipulating the stones. At close *E* asks, "Whose building is prettier?" Answer, "Prettier." *E*: "Who can build the best?" Answer, "Best."

It does not seem too much to say that this little girl was wholly concerned with herself and the blocks. This type of behavior was found in 54.5% of the children between the ages of 2 and 3 yr. This type of behavior, wherein the child is busy exclusively with the material as something with which to play, diminishes, as shown in Table V until the age of 4-5 yr. In 71.4% of the cases at this age-group, the child looks mostly at the other child. Then, as his ability and interest in the work increases, the child again appears to devote his attention more to the blocks than to the other child.

"Looking" can be further clarified. Sometimes the looking appeared to be merely gazing at the other child. At other times 'looking' was really copying, and resulted in definite imitation. The percentage of children who copied was at no time great, and was practically the same throughout the various age-groups. This was a surprising result, although it may be explained by the hypothesis that copying is more a question of type of child than of age.

(*2*) *Speaks to.* But the child does more than look at the other child; he speaks to him and about him. These remarks about the other child, as in the case of remarks about his own work, make a fertile field for research. There was practically no conversation of the parlor sort, no extraneous remarks. The child seemed to devote himself to the object in hand, whether it was mere manipulating, or whether it was building. One heard occasionally such phrases as, "I'm going home this week," or, "Last Sunday I saw a beautiful castle," but they were exceptional. The child confined himself to the situation. The fewest remarks were made by the youngest children. In fact children at the age of 2 yr. made no remarks at all about the companion-child. Table VI shows the percentage of children in the various age-groups making remarks. The things said were specific and connected with the work at hand. They were, as Table VI also shows, mostly of a negative nature.

TABLE V

PERCENTAGE OF CASES AT EVERY AGE-GROUP THAT WATCHES AND DOES NOT WATCH OTHER CHILD

	Age-Group				
	2-3	3-4	4-5	5-6	6-7
Watches	41.7	52.6	71.4	46.5	50.0
Does not watch	58.3	47.4	28.6	63.5	50.0

TABLE VI

PERCENTAGE OF CASES AT EVERY AGE-GROUP MAKING AND NOT MAKING REMARKS, TOGETHER WITH THE PERCENTAGE OF CASES MAKING POSITIVE AND NEGATIVE REMARKS

	Age-Group				
	2-3	3-4	4-5	5-6	6-7
No remarks	100	90.0	62.5	59.1	60.0
Remarks	0	10.0	37.5	40.9	40.0
Positive remarks	0	0	0	4.5	0
Negative remarks	0	10.0	31.3	9.1	20.0

The child's remarks were at times exceedingly curt. He showed, clearly, that he thought better of himself than of the other child, and did not hesitate to say so. Sometimes gestures accompanied these remarks, such as a derogatory sweep of the hand, or a supercilious expression. For example:

Girl, F., age 5·0, and boy, H., age 3·6. Both children have been building very industriously, but H. does not progress very well. He fumbles with the stones. F. says to him: "Na! Can't you even build?" He answers curtly, "Yes." She turns to *E*, throws back her head and laughs, "Ah! He says, 'Yes'," and laughs some more.

Consider also the tone of superiority of this case:

Boy, L., age 4·6, and girl, E.M., age 4·6. L. is slow in building; is apparently not accustomed to blocks. E.M. points to L. and says to *E*, "He there! He can't do anything." Then she points to her own and says, "Isn't that pretty?" Later L. says, "I can build a railroad!" Then E.M. replies in a supercilious manner, "He can't do a thing. *I* can build a railroad."

On a later occasion this same girl was building with another boy (H., age 3·11) who was more capable and built very well. In order to discover what her reaction would be, *E* asked her, pointing to H.'s construction, "Isn't that house pretty?" Whereupon she slowly answered, "Yes." Then suddenly, "*But one day he hit me with a stone!*"

(3) *Gestures.* Looks and words tell us what one child thinks of another. Still more, movements and gestures reveal competitive feelings. Gestures, displayed in the 'despotic' behavior of children, have been exhaustively studied by Charlotte Bühler, who classified those she observed in children, age 4–22 mo., when put in a social relationship with each other. The situation discussed in the present paper is to some extent similar. But this time the children do not have a toy between them, but are working with the same assortment of building stones. Bühler found *Wegnehmen* the earliest gesture of despotism. Although she differentiated sharply between despotism and rivalry, we too find that *Wegnehmen* is one of the first signs of the competitive impulse. Bühler also mentions "hitting," "pushing away," "the gestures of teasing," and the "laugh of triumph." She reports on the other hand that the inferior child permits himself to be given things, to be pleased with things. In the older child who is competing, I can also report all of the preceding gestures. But, in addition, I observed an unusual amount of physical movement, hiding of stones, stealing of stones, singing, and out-and-out fighting. For example:

> Girl, H., age 4·8; boy, K., age 4·10. *E* told them that she would like them to try again and see who could build the larger. K. grabbed a number of stones and hid them in his lap. H. objected, "You have taken some of my stones." No answer from K., he merely took another stone from H. Both children very eager.
>
> H., on finishing her building said, "Here's the bigger one." Her body was tense, jumped up and down, clapped hands, sang, "Mine is finished and it is bigger." K. not so tense, worked steadily, and said, "Mine will be still bigger." Took some of H.'s stones from her building. She said, "Don't you take any of mine away." Each considered his own building the larger. (As a matter of fact K.'s was a little larger, but not obviously so.)

This type of behavior is repeated again and again: a little girl leans over and takes a stone right out of another's building; a boy grabs all the stones he can get; another child stands up, apparently for a better physical position; one child snatches stones right out of another's hands; another child piles his stones in little groups and says they "belongs to me"; and so on for many similar cases.

There were also the gestures of intense attention. Sometimes a child would devote himself entirely to his construction. In his case, no matter how much his partner kept on prattling, he went on working. Needless to say, he considered his work better.

As Bühler has suggested, there are also gestures of competition that belong to the benign type. Sometimes when a child gave another a stone, it was not at all an act of disinterested generosity, but rather a display of friendly competition and superior skill. This type of giving, as the following example shows, must not be confused with mere social offering which appeared in the 2½-yr.-old.

> Girl, H., age 5·0; boy, E.K., age 5·9. *E* asked them no questions nor gave them commands.
>
> E.K. built industriously and with great interest. H. built, but not quickly. E.K. talked continuously; said to *E*, "that is a table," pointing to the table. H. paid very little attention to E.K., merely said "Hm-hm."
>
> E.K. stood back and looked at his building and said, with pride, "That's done." H. said, "I can't go any further, I need two more stones like those," pointing to a certain size of stones. E.K. went to his building, took out the stones H. needed, and said, handing them to her, "Here, H.; here, H." She took the stones and put them in her building. E.K. asked, "Do you need still more, H?" H. answered, "No."

When finished, both buildings looked almost exactly alike. *E* asked "Which is the prettier?" E.K. said quickly, "Mine is the prettier." H. was slower to respond, finally she pointed to E.K. (These children were very fond of one another.)

II. ANALYSIS OF THE FACTORS CAUSING VARIATIONS IN COMPETITION

It is the purpose in this section of the paper to analyze those factors which cause variations in the display of competition. Our study of the child's behavior in a competitive situation has shown that competition is not even found in all the children of any age-group. What then causes this variation? Is it a question of physical development? Then strong children would exhibit competition and the weaker ones not. Is it a question of temperament, of training, or of type—or all of these?

TABLE VII

PERCENTAGE OF CHILDREN EXHIBITING COMPETITION

Age-Group	Present	Absent	Doubtful
2–3	0.0	89.5	10.5
3–4	42.6	55.6	1.8
4–5	69.2	23.1	7.7
5–6	75.4	15.8	8.8
6–7	86.5	5.4	8.1

To understand the variations in competitive behavior one must look closely at the relation to the material and to the other child, and find the basic principle underlying these forms of behavior. It may be seen from Table VII that the degree of competition increases with increasing age, but that competition is not present in more than half the children until the child is between 4 and 5 yr of age. The growth of competition appears to conform to a regular developmental law which can be analyzed and understood.

Principle 1. The first hypothesis which we offer is that competition develops consistently with the *child's relation to the material.* The child's attitude towards the material is fundamentally altered when he has mastered it. He experiences no sense of competition toward material with which he is unfamiliar or which he has not yet mastered. There was a clear relation between the child's mastery of the material and the degree of competition displayed. At the same time there appeared that progression from functional behavior to social relationship and finally to an interest in the work itself which has been seen repeatedly in experiments at the Kinderübernahmsstelle.

(*a*) *The first stage.* Here no competition could be seen. This phase includes children from the age of 2 to 3 yr., showing at 3 yr. some progression toward the next phase. Children of this age-group made vague and undirected movements with the material. They were in the stage of manipulation or mere 'functioning.' Of these children 54.5% did not understand the question, did not grab stones, and did not make either positive or negative remarks about their own work. When they did understand, or appeared to understand, the question, in practically 100% of the cases the answer was, "Mine is prettier," without weighing or judging the matter. As the child of this age has no feeling for work, so likewise is the quality of work poor; and competition is lacking.

It is altogether probable that with other material the child would exhibit competition at an earlier age, but it would need to be material which he had already conquered. For with each new material, he must conquer the steps anew. Until he has conquered the material, he cannot be said to work with it; and until he works with the material, the desire to excel in the use of this material does not exist. But of course mastery of the material is not sufficient. He must grasp at the same time the idea of mastery and the idea of excelling. He must know what it means to do better than someone else.

We do not intend to convey the idea that pugnacity is absent in those cases in which the competitive impulse is not yet developed. In fact, in the very smallest children there was evidenced at times some indication of pugnacity. But we mean that the competitive impulse does not always exist contemporaneously with and as a concomitant of pugnacity.

(b) *The second stage.* The second phase of development in the competitive situation is one in which the child appears to be interested in discovering the other child. Until this time he has been busy discovering himself, developing his own powers through manipulating the material. As he completes this phase one of two things might happen. Either he might become especially interested in the blocks as building material, or he might become especially aware of the other child in the situation. What does happen is this: his powers are constantly developing in regard to the building, but his marked *interests* are in the social situation, in the other child. Once again, he appears to follow the already accepted path of psychological development: first, self-functioning, then the human contact. This does not mean at all that his skill in the use of the material has not increased, nor his understanding improved; and with both, naturally, some increase in the competitive impulse has appeared. All these go on at the same time. Yet we can say that the child from 3 to 4 yrs. of age is one whose interests lean very much to the social side of the problem.

An examination of the protocols and an assembling of the tables reveals a clear composite picture of the child of this age. It is presented here in Fig. 4.

During the second stage, the child's interest in the blocks is slight compared with his interest in the other child or in the social situation. This does not mean that he pays less attention to his building than the younger child does, but that he gives more attention to other people in the room and to the other child. In this stage he does not, for example, begin to grab stones. He simply uses what is at hand. The younger child does not grab, either; neither one sees the situation yet as a competitive one, and neither feels the need to take stones beyond those which lie close at hand. The younger child makes no remarks about the other child at all. But the child of about 3 yrs. of age begins to talk about the other child. The number of negative remarks increases at first, drops again when the child reaches 4 yr.,

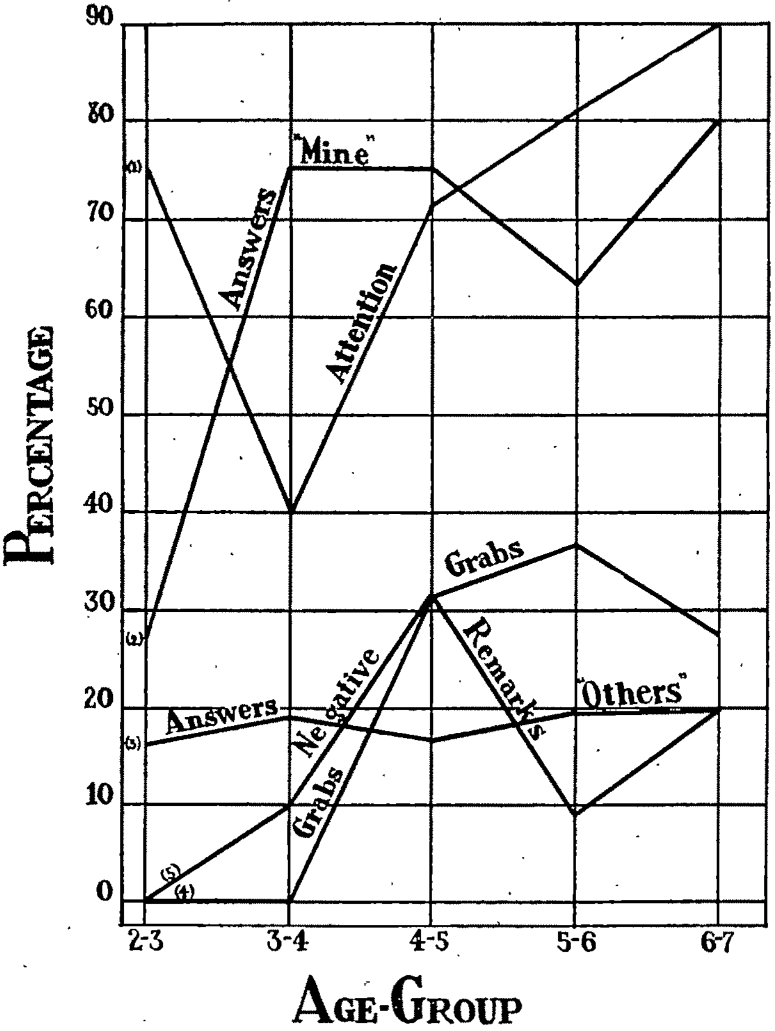

FIG. 4. COMPOSITE PICTURE OF CHILDREN'S RESPONSES

	Percent Every Age-Group				
	2-3	3-4	4-5	5-6	6-7
Attention...........	75	40	71.5	81	90
Answers "Mine".....	25.7	75	75	63.4	80
Answers "Other's"..	16	18.8	16.7	19	20
Grabbed............	0	0	30.7	36.4	27.3
Negative remarks...	0	10	31.3	9.1	20

and is not very great even when the child reaches 5 and has already become definitely aware of the problem. All of which seems to indicate a positive feeling toward the other child.

This is a child who has arrived at a point where he is more interested in the other child than in the material or in building. It may well be said that the other child is, after all, an integral part of the situation, and that a strong interest in him may be the first indication of the competitive aspect of the situation. Our answer is that the 3-yr-old's attitude may be best described as mere 'awareness' of the social contact.

(*c*) *The third stage.* In the third stage the child becomes conscious of the material as stuff for work; he understands the problem in hand, and arrives at a fulfillment of the competitive impulse. Now at this age, 4–6 yr., the child no longer sits and plays with the material, nor does he spend so much time in doing nothing. Instead, he attacks the material at once, as *building-stones*, and begins to work. From 52 to 90% of the children from 4 to 6 yr. of age attack the problem without hesitation. They display their interest in many ways. They begin to grab stones. The percentage of children who give attention rises sharply. The child makes negative, but no positive remarks about the other child, being more interested in what he makes than in the social relationship to the other child. The positive remarks about his own work rise rapidly, from 10% to 30%. He no longer answers "mine" to the question, "Which is prettier?", but seems instead to deliberate.

In other words this attentive, block-grabbing, negative-remark-making, but spontaneous child displays competition. These are the children who quarrel with each other over stones. They simply *have* to excel. These children also exhibit, at times, a patient and yet intense effort to improve their own building, sometimes trying over and over again to improve the construction. There were many instances of this type of effort, of which only a few can be presented.

> Boy, R.B., age 6·0. Very eager to build. Says, "I will build a house." Piles stones in a corner. Does not look at C. at all. Sighs, "Yes, what I am making is very pretty. I just need to work a little longer." Grabs many stones. Keeps on working after other child has finished. Says, "A church." Then, "Finished."

Good evidence of the child's interest in the building itself is the way the child in this phase handles the materials. He groups them, putting those of a like shape together, or matches them for size. The following protocol shows this quality of work quite clearly:

> Boy, F.J., age 6·0. Begins at once to work. Pushes some stones towards himself and groups them into a pattern. Seeks definite stones. Says, "That is a castle," then holds up a stone. "This one is larger." Holds up the stone and measures it against the other; "No, this one is larger!" Very attentive to his work. Asks *E* for certain stones that he lacks. Goes on building.

The child in this stage is master of the material and struggles to surpass his partner. He tries diligently to excel, and even when he does not succeed, he does not yield to defeat. Consider the case of this boy who was building to see whose would be bigger. When he finished, the other's work was so much bigger that he had to admit it, but could not forbear adding, "But I can build bigger, too!"

(*d*) *The fourth stage.* A still more developed phase in competition when building with blocks appears to exist. The somewhat older child recognizes even more clearly the meaning of the situation, and his desire to excel becomes tempered by his ability to judge his own work. I found at this stage a very interesting example of a girl who, immediately recognizing her inferiority in building, apparently lost interest, and would not build. But she understood the problem and the competitive situation. She showed it by rather outspoken remarks and by saying whatever good of herself she could find to say.

Principle 2. The second contention in the analysis of the factors causing variations in the display of competition is that competition seems to be a human inclination which is nurtured and directed by education. Hurlock has shown that children do better either after reproof or praise than after mere repetition; reproof and praise are, of course, implied in the educational method.[9] The results of the present experiments corroborate Hurlock. Instead of either praise or reproof, however, *E* injected the element of suggestion by presenting a task, an 'assignment' at the second attempt at building. The first time the two children built, they simply worked or played at random. After they had finished, they were asked whose was prettier, and then were permitted to build again. The mere asking of that question, "Whose is prettier?" is an incentive to a sensitive child. But to increase the effect, at the second building *E* said to the children, "Let us see who can build better," or "Would you like to see who can build better?" *E* saw a really appreciable increase in interest, in spontaneity, in energy, and, of course, in competition. We present in Figs. 5–6, the graphs of some of the essential criteria in establishing the presence of competition. In each case the curve representing the first building is to be compared with the curve representing the building after the assignment was given.

If the first contention, that the display of the competitive impulse is correlated positively with the command of the material, is true, then it follows that education in the use of certain material would increase the likelihood of the competitive impulse in the situation. With increased technical skill comes increased competition. There were a number of cases where each succeeding experiment showed such a marked increase in the competitive impulse that *E* permitted the children to try many times. Perhaps the reader can gather something of the tenseness, the speed, the competition in the air, the feeling that came so impressively to *E*. Note especially the kind of personal remarks made, the grabbing and hiding of stones, the increase of muscular tension, and the final out-and-out quarreling.

Girl, H., age, 4·8; boy, K., age, 4·10. No assignment given by *E*. H. began to build at once. Held up some big stones and said "I have two big stones." K. immediately said, "I have three."

H. talked while building. Said, "Here is my bed; a big bed." Handled stones carefully.

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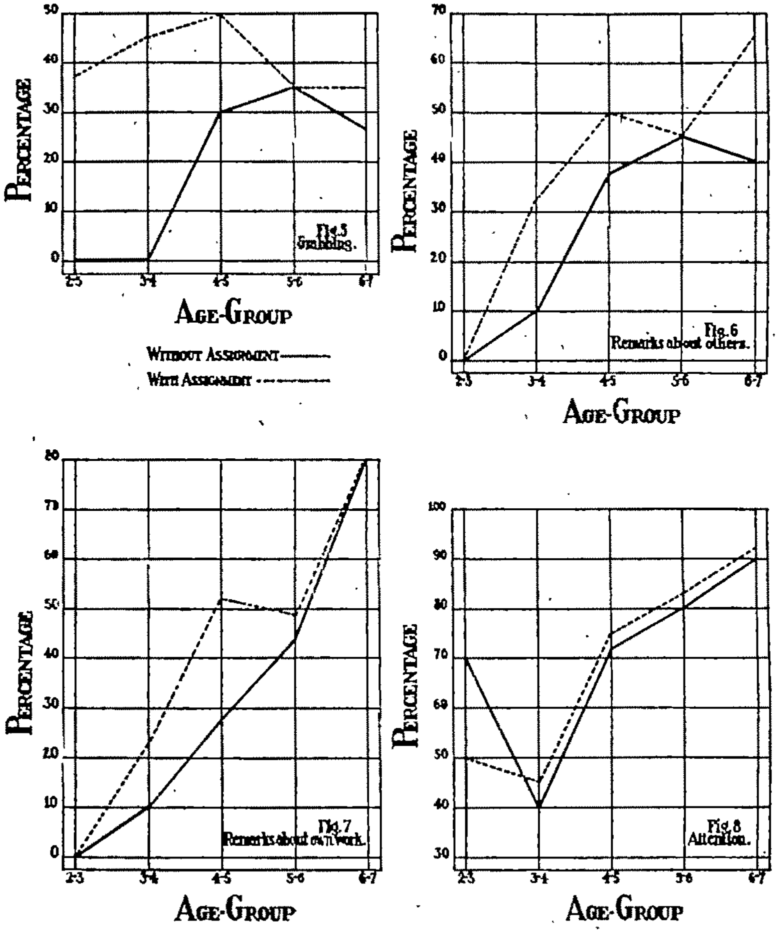
[9] Hurlock, *op. cit.*



FIGS. 5-8. SHOWING DISTRIBUTION OF THE VARIOUS CRITERIA OF COMPETITION AMONG THE DIFFERENT AGE-GROUPS

E said to K., "That's too high, it will fall." K. worked without talking. H., however, continued the conversation, "But mine isn't too high." "Later I'll make a bed." She seems to be building a fish-bath, for she said, "Here is the water and little fishes in it." To *E* she said, "Look here!"

E asked, "Are you through?" H. replied, "No, I'll be through in a minute." K. did not reply until *E* said to him, "And you?", when he simply said, "No."

When the building was finished, the children were asked which is prettier. Each considered his own the prettier.

Same children, same day.

E said to both, "I'd like to see who can build the bigger!" Both were eager to build. H. said to *E*, "Look at what I've built." K. watched H. K. grabbed a stone, and said "This belongs to me!" K. took a pretty stone, but gave another. H. said, "I'm not through yet." Sang, "Look here! look here! I'm building something fine!"

K. handled stones very carefully. Put stones in his lap when rebuilding or improving.

H. said to a stone, "You! You must stay there;" and to K., "Get away, you take too much." K., however, did not protest.

E asked "Which is bigger?" H. said quickly, "Mine is bigger;" and K. said "No, mine is bigger and wider." *E* said to H., "H., isn't K.'s bigger?" She smiled and said "Perhaps," and then changed her mind for she said, "Oh, no, mine is bigger." She pointed to a room in her construction, and said, "Here!" (K.'s construction was much larger.)

Same children, later in the same day.

E said, "I'd like to see once more who can build bigger." K. replied, "Wider;" and H., "Now I'll build wider, he will not." Both worked eagerly. K appeared to study the problem carefully. Arranged and rearranged the stones to make the building larger. Took many stones and put them in his lap. Looked up and smiled at *E*.

H. didn't talk much; once, however, when some stones fell, she said to the stones, "You! You!"

E asked K., "Are you through?" K. replied "Yes." (His stones were used up.) He took another stone from H.

E asked, "Which is bigger?" (K's building much larger.) H. said, "Mine is bigger." K. said, "No! Mine is!"

The experiment was repeated a fourth time.

H. took a stone from K., he fought with her and took some of her stones. They quarrelled. H. spoke to herself. Both built rapidly and with tense muscles. K.'s building was much larger, yet each child considered his own the bigger.

The way *E* puts the question is also of some importance. Probably the degree of the competitive impulse can be intensified by the behavior of the educator or person who has to deal with the child. Because no data were to be taken on this point, *E* made a decided effort not to inject her personality into the giving of the assignment. She always presented the questions in a cool and unimpassioned manner. But there were a few occasions when, through interest in the process, there slipped into those impartial questions a heartiness or an eagerness to which the child immediately responded. Sometimes two children who had been really apathetic during the first building showed a much more competitive attitude if *E* said with a rising inflection and interested smile, "Wouldn't you like to try again?" This was not a part of the planned experiment, but was too evident and interesting to be allowed to pass unmentioned.

It might seem that if one could know the degree of the child's mastery of the material and the degree of his understanding of the problem, and also the training he had had in the specific building situation, one could predict the amount and kind of competitive impulse he would display. But here enters the last and most subtle of the factors affecting the display of competition. The competitive impulse is not only a variable related to chronological and physiological factors, and to education in the matter in hand, but is also quite dependent on individual differences, the causes of which constitute the crux of the situation.

Here are two girls who have been put into the same situation. They are the same age; they have reached the same psychological development; they both understand the problem and build about equally well. Yet one child exhibits a marked competitive impulse, and the other very little. They have been given an assignment to see who could build larger. They are not building *together*; each is building with another child. The factors are as nearly constant as can be obtained; same time of day, same month. Both families are in poor financial condition, but both children seem well and normal.

Girl, B.W., age 4·6. *E* says, "Let's see who can build bigger." B.W. looks around the room. Builds, but with great uncertainty. Sighs as she builds. Continues in this way. When asked, "Whose is larger?" she is undecided.

Compare this with the tenseness and evident eagerness to excel shown by H., just reported. B.W. is rather interested, but passive and unemotional in her response. She fulfills none of the criteria of the competition profile. H. is interested instantly and shows a clearly marked competitive impulse. She is eager, talks positively about herself, grabs stones, makes characteristic movements such as talking to the stones, makes negative remarks about the other child. She considers her own building bigger, even though that of the competing child is twice as large.

This variation in the competitive impulse can be seen at a much earlier age. Both of the children in the next comparision are 3½ yr. old, and neither one has reached a fully developed competitive stage, but clearly one tends very much more in that direction than the other.

Boy, H., age 3·6. *E* says, "I would like to see who can build better. Can you build a train?" H., "Oh! Yes!" Sings, "Train! Train! Train!" Makes a noise like steam. Stands up to build. Movements become sharper, more eager. Says, "I am building a locomotive." Fumbles somewhat with the stones, but goes on building. Other child observing this fumbling says, "Can't you even build?" H., very curtly, "Yes." Stops building. Comes to side of *E*. Stands by her side. *E* asks, "What is that?" Answer, "A train." Adds another stick then says, "So! now I have a window; I must build a little more." Suddenly to *E*, "Isn't mine pretty?" Sings: "A train, an express train!" *E*: "Aren't you finished yet?" Answer: "No, I will stand and build." *E* asks both children, "Which is prettier?" Other child answers, "Mine." H. says quickly, "No, mine."

In the construction of this 3½-yr.-old boy there was something to be seen, but the result was not in proportion to the certainty and assurance of this little fellow that he was making "an express train."

Girl, R., age 3·6. *E* says, "Who can build the nicest train?" R. looks about. Seems somewhat uncertain. Glances at B. (her partner) to see what he is making. Goes on quietly trying to build but not with great success. Other child invites her to build on the floor. She goes on building on the table. When finished, both children are asked to stand by *E* and to look carefully at each other's constructions. Question is put to R. first, "Which is prettier?" She smiles shyly at *E* and points to her own.

There is a decided difference in tone between the attitudes of these two children. H. is perhaps not much more advanced than R. in constructive ability, but his whole attitude is very unlike hers. He is determined and aggressive. He builds and rebuilds. He looks at no one, gives his construction a name and builds straight toward an end. He makes positive remarks about his own work. The girl, R., is uncertain; she glances at the other child to watch and copy. She tries nothing new, such as the floor as a place whereon to build, even though it is suggested to her. The boy is experiencing the first phases of the competitive feeling. He wants to make his work as fine as he can. The girl is not quite oriented in the idea of excelling. The cases illustrating this point are numerous. Here is a girl of 6·2 yr., who is capable and interested and yet exhibits no competitive signs at all, and another girl of 6·9 yr. who exhibits competition to a most marked degree, etc.

Bühler includes a discussion of types of children in her study on "Despotism and Rivalry." Even in the case of babies she differentiates between the behavior of those she terms the despots and of those who are passive and receptive. She mentions a third type who bend everything before them through their activity. A child of this type overcomes children both older and younger through the very intensity of his far-reaching activity.

It is somewhat difficult in so complex a situation to say exactly what causes these variations in temperament. It seems, to *E* at least, that one must at the outset realize that the conditions prevailing at the time play a tremendous rôle in what may appear as 'type' conduct. Such a case is C., who apparently had no interest in building, and certainly evidenced no competitive spirit. This child's apathy is certainly better understood if the fact be reported that 10 min. later she came and threw herself into *E*'s arms sobbing out her lonesomeness. Her apathy was not apathy. It was grief. The disinterested child may not be disinterested; perhaps he is hungry or tired, or misses his mother, or is new to the Institution. Maybe he wants to go out in the garden. There are a thousand possibilities.

Yet despite all that, after observing daily for months the behavior of these children as they build together (and the frequency of the observation lessens the importance of chance factors) the observer can report a difference in the individual temperaments of the children. Some children have a strong desire to excel, a desire that manifests itself by stealing, hiding, making nasty remarks, getting "hot and bothered" when competing. Other children have a desire to excel but manifest it differently. These children simply give their attention to the blocks. They build and rebuild. They make some remarks, but neither so many nor so disagreeable as the first group. They take many stones, but they do not rush so fast in their grabbing, and they often ask before they take. A third type is not competitive at all. Sometimes we can offer no explanation except in terms of the child's general disposition.

These types may be the result of inborn differences, or they may be the result of social conditioning. The differences to be observed in children of 2-yr. of age (or, of course, older) are, in our opinion, the results of both. One has only to watch babies in their cribs, and certainly children at play, to recognize variation in temperament. But with some degree of certainty we can state that the tendency to compete can be developed in a child; that it did grow in some children as we observed them perform their building a repeated number of times.

SUMMARY

By competition we mean that effort manifested by a child when he is influenced by the 'desire to excel.'

The criteria for the presence of the competitive impulse are:

(A) An understanding of the problem, determined by (1) the way the material was attacked; (2) the use made of the material; and (3) the answer given to the *E*'s question.

(B) The interest shown in the problem, judged by:

  (1) Attention during building, which in turn was measured by (a) lessened disturbance by outside influence; and (b) consistency in building.

(2) Physical signs of interest, *e.g.* (a) tensity of muscles; (b) increased speed; and (c) unusual or characteristic gestures.

(3) Remarks showing interest. (a) Positive or negative remarks about other child; and (b) positive or negative remarks about own work.

(4) Technique in building. (a) Building and rebuilding; (b) grouping of stones; (c) evident desire to go on constructing even after other child is finished.

(C) A competitive attitude toward the other child, viz. (1) watching or copying; (2) grabbing or stealing stones from other child; (3) offer of help or of stones; (4) disagreeable or critical remarks; (5) gestures of quarreling.

The results of the study reveal:

(A) Competition in building with blocks is not found in children of all ages, nor in all children of any one age.

(B) There appear to be 4 fundamental factors affecting the display of competition in building with blocks: (1) the degree of the child's understanding of the idea of excelling; (2) the degree of the child's ability to dominate the material; (3) the educational factors in the situation; and (4) the individual temperament as a competitive factor.

(C) The growth of the competitive impulse in the use of building stones seems to follow a well defined and orderly course, as follows: (1) no competition; the child discovers himself in relation to the material and is interested in functioning with that material (age-group 2-3 yr.); (2) some competition; a little better understanding of the idea of excelling; child "discovers" the other child, and although his constructive ability increases he is more interested in the social relationship than in the competitive situation (age-group 3-4 yr.); (3) competition; the child discovers the work and manifests his desire to excel (age-group 4-6 yr.); and (4) competition: with increased critical judgment (age-group 6-7 yr.).

(D) In the use of building stones no competition was seen at all in children of two years of age. At least 90% of 6-yr.-olds have it well developed. It can be said to make its appearance usually at about the age of 4 yr.

THE RELATION OF MAZE ADAPTABILITY, MAZE LEARNING, AND GENERAL INTELLIGENCE

By Richard S. Schultz, University of Pennsylvania

The general purpose of this investigation was to study a number of factors related to maze learning.[1] In particular, it was concerned with two problems: (1) the relation of general intelligence to ability in solving a maze problem and in maze learning; and (2) the relation of the ability to *solve* a maze problem to the ability to *learn* a maze problem.

This investigation differs from previous studies in its emphasis on the relation of adaptive capacity to maze learning. It attempts to obtain a measure of motor adaptive capacity as well as of motor learning in the same task.

Following earlier studies, a general intelligence test is used as one expression of adaptive capacity. But, in addition, another expression of adaptability is obtained, in a series of five maze problems, through the use of the Miles Duplicate Maze.

HISTORICAL REVIEW

In two independent studies, the coefficients of correlation between maze learning ability and general intelligence range from −0.30 to −0.45.[2] Another study has approached the problem of general adaptive capacity in maze learning by a comparative analysis of the performance of children and adults. In this investigation the conclusion is reached that extreme degrees of intelligence may be handicaps in maze learning.[3]

A study of the relation between intelligence and the ability to solve maze problems is reported by Cox.[4] The Miles Duplicate Maze is presented in a series of five problems of varying difficulty. This situation is not one essentially involving learning; but rather an attempt to measure the intelligence factors

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[1] From the Psychological Laboratory of the University of Pennsylvania. The writer wishes to acknowledge his indebtedness to Professor Edwin B. Twitmyer, Dr. Morris S. Viteles, and Professor Samuel W. Fernberger, and to Dean Charles R. Turner, of the School of Dentistry.

[2] W. S. Hunter, Correlation studies with the maze in rats and humans, *Comp. Psychol. Monog.*, 1, 1922 (no. 1), 37-56; C. J. Warden, Relative economy of various modes of attack in the mastery of a stylus maze, *J. Exper. Psychol.*, 7, 1924, 270-272.

[3] M. C. Gould and F. A. C. Perrin, A comparison of factors involved in the maze learning of human adults and children, *J. Exper. Psychol.*, 1, 1916, 122-154.

[4] C. M. Cox, The intelligence factor in the solution of space problems with the two-story maze, this Journal, 40, 1928, 542-561.

involved in the solution of a relatively new maze problem. The correlations obtained in this earlier study between performance time and the Thorndike College Entrance Test are given in Table I in the report of the present investigation. A qualitative analysis has also been made of intelligence factors involved in these maze problems. Cox suggests that the Miles Duplicate Maze may serve as a valuable performance test of intelligence, if properly standardized.

A number of investigations on the maze indicate the presence of such factors as distractability, temperament, general emotional tone, fatigue, methods of attack, interpretation of problem, which complicate the learning process.[5] Boring, in a study of maze learning with dementia precox subjects, found it advisable to use a paper maze as a preliminary device in order to create a proper attitude and to familiarize the subject with the general nature of the task.[6] Warden, in work with the white rat on the problem box,[7] compared a preliminary adjustment period of 7 initial exploratory trials or standard feeding periods for one group with 7 regular trials of a control group in learning. The results indicate that a period of random activity at the beginning in learning serves to eliminate emotional disturbance, hastens formation of habit pattern, and reduces the total learning time 60%. Methods of extraneous control in learning acts of motor skill are reviewed by Carr.[8] The paper is especially concerned with an evaluation of various forms of 'tuition' (guidance, directions, information, attitude, etc.) and incentive as they are related to the learning process in the maze situation. Perrin also considers various techniques and methods of presentation in learning the stylus maze.[9]

METHOD

Apparatus. The apparatus, the Miles Two-Story Duplicate Maze, consists of two identical maze patterns, one mounted directly above the other on a wooden box with two open sides. A black cloth fastened to the top platform falls over the open side parallel with *S*'s body, and covers the arm and hand on the bottom maze. The opposite open side gives *E* a full view of the bottom maze.

A small key-piece, an exact replica of the center of the maze, is attached to the top platform and serves as an index to the position of the bottom maze. The lower maze is mounted on a large rotating disk and can be turned to any desired position in reference to the stationary upper maze.

The task involves the movement of a stylus through a visible top pattern, while at the same time another stylus (with a rubber disk to eliminate contact with the maze) is moved through the screened bottom pattern. The path traced through the lower maze is recorded on a sheet of paper placed under the pattern.[10]

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[5] Gould and Perrin, *op. cit.*, 122-154; also Perrin, An experimental and introspective study of the human learning process in the maze, *Psychol. Monog.*, 16, 1914 (no. 70), 36 ff.; Hunter, *op. cit.*, 50-52.

[6] E. G. Boring, Learning in dementia precox, *Psychol. Monog.*, 15, 1913 (no. 63), 51-55.

[7] C. J. Warden, The value of the preliminary period of feeding in the problem box, *J. Comp. Psychol.*, 5, 1925, 365-372.

[8] Harvey Carr, Teaching and learning, *Ped. Sem.*, 37, 1930, 189-218.

[9] Perrin, *op. cit.*, 36 ff.

[10] The top maze was mounted on glass, thus eliminating a scratched surface which would obviously become a disturbing variable. The large disk for the bottom maze had to be set with extra screws around the edge and a special fastener in order to make it more rigid and secure. Four extra studs were also attached to the disk in order to hold the bottom maze more rigidly.

The arrangement of the two mazes and the use of two hands in operation has the advantage of presenting a problem involving the visual, tactile and kinaesthetic senses. This has been classed by Miles as a motor task with maximum indirect visual guidance.[11] The use of cues from one hand to the other, in addition to visual, tactile, and kinaesthetic senses, possibly make the guidance element more complex.[12]

Procedure. The maze task is divided into two parts. The first part is referred to as an 'adaptability' task. The *Ss* are required to trace through 5 successive positions of the maze as quickly as possible. These problems of varying difficulty are obtained by rotating the bottom maze to the following angles in a counter-clockwise direction—0°, 180°, 90°, 180° reverse, and 90° reverse. The last two problems are mirror reversals of the 180° and 90° positions. In presenting the 5 positions of the maze, an attempt is made to test the various abilities and traits that may be evoked in adaptation to the Miles Duplicate Maze situation. In general, the instructions and procedure for the adaptability task follow that of the previous investigators.[13]

After the adaptability task, there is an approximate 5-min. rest interval. In the second part of this experiment one of the above problems (180° reverse position) is given as a 'learning' task. Standard instructions and procedure for the learning task were developed by the present investigator.

The test score in 'adaptability' is based on the time taken, in one presentation of each problem, to trace from the center through to the last exit of the maze. An adaptability problem not solved in 900 sec. was recorded as a failure. In 'learning' the number of trials and the time per trial were recorded. The learning task is finished when two successive trials are made without any retraces and entrances into blind pathways. A detailed record was kept on a standard mimeographed form of the quantitative and qualitative aspects of the performance.

General intelligence. The Psychological Examination (for High School Graduates and College Freshmen), American Council on Education, was used. It was administered, before any maze tests were given, as a group examination.

Subjects. The Main Group of *Ss* for this investigation includes 89 men, students in the Dental School, University of Pennsylvania. All were in the second year class. They had also completed 2 years of a college pre-dental course. A number of them are college graduates and several have pursued graduate and specialized courses. They averaged about 22½ years old and ranged from 20 to 28 years in age.

A second group of *Ss*, 11 in number and hereafter referred to as the Experimental Group, was used in one part of this experiment. This group has a median age of 20 with a range of 17 to 27 years. It consists of 9 men in the undergraduate schools, an instructor, and a college graduate.

RESULTS

(*1*) *Maze adaptability, general intelligence, and chronological age.* An examination of Table I, for the Main Group, indicates that the two separate total maze adaptability time-scores and the gross time-scores for each problem correlate insignificantly with intelligence test scores.[13]

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[11] W. R. Miles, The two-story duplicate maze, *J. Exper. Psychol.*, 10, 1927, 365-377.

[12] C. M. Cox, *op. cit.*, this JOURNAL, 543-545; Comparative behavior in solving a series of maze problems of varying difficulty, *J. Exper. Psychol.*, 11, 1928 203-206; Miles, *op. cit.*, 371-374.

TABLE I

Correlation of the Intelligence Test with the Time-Scores in the Various Maze Problems, and Comparison of the Results for the Main Group of *S*s with Cox's Results

	Problem				
	0°	180°	90°	180°R	90°R
Main Group	−.18±.07	−.18±.08	−.04±.08	−.19±.07	+.34±.07
Cox's Results	−.26±.06	−.59±.05	−.40±.07	−.11±.09	−.09±.09

	Total Score		
	Gross	Weighted	Av.
Main Group	+.04±.09	+.03±.09	
Cox's Results			−.39±.09

TABLE II

Comparison of Total Gross Time-Score in the First Four Problems with the Gross Time-Score in the 90° Reverse Problem for Groups Classified in Thirds Based on Intelligence Test Score

(70 *S*s, the odd *S* was grouped in the middle third)

	First four problems			90° R problem		
	Upper	Middle	Lower	Upper	Middle	Lower
Average	726.5	1077.9	949.0	410.8	364.6	242.4
Median	585.0	1047.0	882.0	333.0	401.0	197.0
75% score	426.0	579.0	466.0	185.0	191.0	121.0
25% score	1056.0	1344.0	1206.0	608.0	504.0	322.0
Quartile deviation	315.0	382.5	370.0	211.5	156.5	101.5
Range	342–1290	311–2200	242–2670	97–862	64–806	75–796

The correlation of intelligence test score with performance time on the 90° reverse problem deserves closer examination. Table II shows that the upper third of intelligence has spent less average time than the middle and lower thirds on the first four adaptability problems. But in the last problem (90° reverse position) in which only

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[13] A total adaptability score was obtained in two ways. (1) The time scores for each individual on the 5 problems were simply added together. (2) Another total score was obtained by finding the ratio of the S.D. for each problem to the S.D. of the 0° problem. The raw score of each problem was then multiplied by its respective ratio. All the problems are, therefore, given a relatively equal value in the total score. These weighted scores added together yield the total weighted score. The validity of this method was ascertained by correlating the total weighted scores with their respective gross scores for the 55 *S*s successful in all adaptability problems. This gave a correlation of +0.94, P.E. 0.01. The total scores are referred to respectively as the gross score and the weighted score.

70 *Ss* were successful, the average time score is higher for the upper third of intelligence and lower for the middle and lower thirds. The more time used in tracing the maze in the four positions preceding the 90° reverse problem may possibly lead to greater familiarity with the maze pattern in general; and consequently influences the performance time on the last adaptability problem.[14]

Intelligence test scores may be compared in another way with maze adaptability performance. The number of failures in each maze adaptability problem is tabulated, in Table III, for the intelligence test quartiles. One fact stands out clearly in this comparison. The highest quartile of intelligence has decidedly less failures in all maze adaptability problems.

TABLE III

NUMBER OF FAILURES IN MAZE ADAPTABILITY COMPARED WITH QUARTILES OF INTELLIGENCE TEST

(89 *Ss*, the odd *S* was grouped in the Third Quartile)

	Quartile Rank in Intelligence Test				
Problem	Lowest	Second	Third	Highest	Total
0°	0	0	0	0	0
180°	2	2	1	0	5
90°	7	8	10	1	26
180° R	1	2	1	0	4
90° R	6	5	6	2	19
Total	16	17	18	3	54

The results on the relation of general intelligence to maze adaptability do not, in general, agree with Cox's.[15] The following facts may be of significance in this connection. The Main Group of *Ss* was selected as to intelligence.[16] In maze adaptability, this group is superior in performance to 2 other groups (see Table IV).[17]

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[14] Perrin, *op. cit.*, 36 ff. After learning a maze pattern, a change in the position of the maze or a placement of the stylus in various parts of the maze gave little difficulty. The *Ss* were able to adjust to the changes readily, showed signs of familiarity with the paths and the general pattern, and the speed of tracing through the pattern closely approximated, in a short time, the speed for the originally learned maze position.

[15] Cox, *op. cit.*, this JOURNAL, 542-546; also *op. cit.*, *J. Exper. Psychol.*, 206-208.

[16] L. L. Thurstone and T. G. Thurstone, The 1929 psychological examination, *Educ. Record*, 11, 1930, 107-108. When the general intelligence test scores are compared with the larger mixed group of 34,507 cases from 131 colleges, cited in the above reference, the average and median scores of our *Ss* rank above the 75th percentile.

[17] Influence of training appears most important as a possible explanation for any significance that may be attached to the above facts. The *Ss* in the Main Group have had special training, to a very great extent, in tasks involving a variety of motor skills. They have also been trained to explore and to make

TABLE IV

Significant Measures Based on Successful Cases: Comparison of the Results of the Main Group, the Experimental Group, and Cox's Group

	Main Group				
Problem	No.	Av.	Median	S.D.	Range
0°	89	67.0	55.8	41.0	20–217
180°	84	217.1	150.0	180.6	33–876
90°	63	420.7	336.0	264.4	56–895
180° R	85	166.4	120.0	136.8	36–698
90° R	70	341.2	300.0	212.4	64–862
Gross total score	55	1054.5	1100.0	416.7	317–1987

	Experimental Group				Cox's Group*	
Problem	No.	Av.	Median	Range	Av.	S.D.
0°	11.	51.1	54.0	21–105	96	92
180°	10	337.2	274.0	95–584	361	311
90°	7	596.0	545.0	290–871	463	257
180° R	10	284.2	179.0	42–829	217	179
90° R	7	559.1	561.0	149–850	432	298
Gross total score	6	1720.3	1377.0	1202–2227	—	—

*Consists of 67 college students, 39 men and 28 women.

There is, as is shown in Table V, a low correlation between chronological age and maze adaptability. It reveals a tendency for a lower (better) time score to be associated with a higher chronological age, and vice versa. A consideration of Table I combined with the results in Table V suggests that the degree of success in maze adaptability is favored by a low chronological age and that intelligence is negligible.

A further analysis of the relation of intelligence and chronological age to maze adaptability is found in the comparison of successful and

TABLE V

Correlation of Chronological Age with Total Maze Adaptability Score for Cases Successful in All Five Problems

	r	P.E.$_r$
Total gross time score..............	−.25	.08
Total weighted time score..........	−.25	.09

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delicately controlled movements in unseen or limited visual fields with the use of a mirror for work in teeth cavities. However, the available data do not warrant an assumption that any specific transfer or even transfer of general attitudes has directly influenced the results of this group. Correlations of the total weighted adaptability time-score with 8 criteria of technical abiilty in Dental courses range from −0.25, P.E. 0.09 to −0.37, P.E. 0.08.

failure groups (failure in one or more problems). Success as contrasted with failure in maze adaptability appears to be favored by a low chronological age and a high intelligence test score (see Table VI).

TABLE VI

COMPARISON OF THE INTELLIGENCE TEST SCORE AND CHRONOLOGICAL AGE OF THE 55 *Ss* WHO SUCCEEDED IN THE ADAPTABILITY PROBLEMS WITH THE 34 *Ss* WHO FAILED

	General intelligence (in points scored)		Chronological age (in months)	
	Failed	Succeeded	Failed	Succeeded
Average	181.8	193.3	277.7	269.4
Median	182.9	193.3	271.3	267.3
S.D.	44.8	57.2	19.0	17.7
Range	75–268	57–299	244–338	239–317
D/σ diff.		+1.06		+2.05

(2) *Maze learning, maze adaptability, general intelligence, and chronological age.* The data that follow present an analysis of the relation between the ability to *solve* a maze problem and the ability to *learn* a maze problem. In the literature, there appears no report of such a comparison of maze learning and maze adaptability with the Miles Two-Story Duplicate Maze or with any similar apparatus.

The 180° reverse position was selected as a learning task following preliminary experimentation with the other four positions of the maze. The problem ranks second in difficulty based on average time score (Table IV). It correlates highest with all of the adaptability problems, with the exception of the 90° reverse position, as shown in Table VII.[18]

TABLE VII

INTERCORRELATION OF TIME-SCORES ON THE FIVE PROBLEMS FOR THE MAIN GROUP

Problem	0°	180°	90°	180° R
180°	+.31 ±.07			
90°	+.18 ±.08	+.22 ±.08		
180° R	+.45 ±.06	+.43 ±.06	+.34 ±.08	
90° R	+.10 ±.08	−.02 ±.08	+.31 ±.08	+.19 ±.08

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[18] The general trend and magnitude of these intercorrelations agree very closely with previous findings by Cox, *op. cit.*, this JOURNAL, 545-547. W. T. Heron (Ability *vs.* chance in learning the stylus maze, *Comp. Psychol. Monog.*, 2, 1924 (no. 8), 14-17) reports a similar magnitude of intercorrelations for time scores in five slightly different maze problems that were completely learned. C. L. Hull (*Aptitude Testing*, 1928, 215) says, "While experimental results have failed to show any convincing evidence for distinct levels of abilities, it has revealed what may turn out to be a significant tendency or law, this is that highly complex intellectual activities correlate highly with each other, the less complex correlate to an intermediate degree, and the relatively simple motor activities correlate with each other only slightly."

The low correlations in Table VIII indicate a positive relationship between performance in maze adaptability and maze learning. Group comparisons appear more significant.

TABLE VIII

CORRELATION OF TOTAL TIME-SCORE IN ADAPTABILITY WITH TOTAL TIME-SCORE IN LEARNING

(37 selected *Ss*)

	r	P.E.$_r$
Total gross time-score............	+.11 (+.24)	.11
Total weighted time-score.........	+.19 (+.25)	.11

The total weighted adaptability scores for the select successful group[19] are arranged in rank order and then divided into approximately equal thirds. Comparisons of the maze-learning data are then made possible by distributing the learning scores under these three maze-adaptability sub-groups.[20]

TABLE IX

FIRST PERFECT TRIAL: COMPARISON OF TIME AND NUMBER OF TRIAL FOR THE SELECT SUCCESSFUL GROUP, DIVIDED INTO THIRDS ON THE BASIS OF TOTAL WEIGHTED ADAPTABILITY SCORE

(37 *Ss*, odd *S* grouped in middle third)

	Time			Trial		
	Upper	Middle	Lower	Upper	Middle	Lower
Average	41.8	47.9 (49)	59.9 (58)	11.5	11.5 (10)	16.5 (15)
Median	37.0	47.0 (38)	34.0	6.0	7.0	14.0
75% score	15.0	21.0	24.0	2.0	5.0	4.0
25% score	51.0	49.0	67.0	17.0	11.0	23.0
Quartile deviation	18.0	14.0	21.5	7.5	3.0	9.5
Range	14–100	9–149	18–162	1–34	1–26 (24)	1–42

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[19] This group of 37 cases has been especially isolated because all the *Ss* were successful in both adaptability (all 5 problems) and learning. Whatever relations are revealed between adaptability and learning should appear more clearly with this group, as the conditions of relative and complete achievement were uniform for it.

[20] In examining the various data on learning, the nature of the distribution of the scores and the effect of removing 2 extreme cases may be considered. The total learning times scores are massed toward the lower end of the distribution. Two extreme cases occur beyond the upper range of 3 S.D. These 2 cases fall in the middle and lower sub-groups based on total weighted adaptability score. When these 2 scores are eliminated the differentiation among the three sub-groups in Tables IX, X, XII, and XIII, becomes less distinct. But the indicated general trend in the various comparisons persists. The manner in which the coefficients of correlation are affected by this change is shown in Tables VIII and XIV. Figures in parenthesis indicate the effect of removing the 2 extreme cases.

The various factors in the learning data, in Table IX and X, are consistently differentiated by the above classification. The largest differences appear between the upper and lower sub-groups.

TABLE X

COMPARISON OF TOTAL TIME, TOTAL NUMBER OF TRIALS, AND AVERAGE TIME PER TRIAL FOR THE SELECT SUCCESSFUL GROUP DIVIDED INTO THIRDS ON THE BASIS OF TOTAL WEIGHTED ADAPTABILITY SCORE

(37 selected *S*s, the odd *S* was grouped in the middle third)

	Total time			Total trials			Av. time per trial		
	Upper	Middle	Lower	Upper	Middle	Lower	Upper	Middle	Lower
Average	831.6	1266.7 (970)	1816.6 (1479)	19.2	23.4	32.5 (26)	50.2	66.9 (62)	79.8 (81)
Median	733.0	881.0 (822)	1425.0	18.0	12.0	24.0	49.0	64.0 (56)	61.0
75% score	623.0	492.0	722.0	11.0	7.0	8.0	31.0	47.0	40.0
25% score	1138.0	1181.0	1875.0	28.0	25.0	43.0	57.0	70.0	95.0
Quartile deviation	257.5	344.5	576.5	8.5	9.0	17.5	13.0	11.5	27.5
Range	251–1698	251–4831 (2481)	370–5526 (3208)	3–35	2–82	4–73 (60)	31–84	16–131 (119)	39–180

In Table XI all of the cases successful in learning are divided into two groups based on performance in adaptability. The group with failures, in one or more adaptability problems, consumes not only longer time, necessarily, in adaptability, but also in learning. This agrees with Table XV, which shows that the failure group in learning was the poorer group in adaptability. As in the preceding data, both Tables XI and XV reveal a positive relationship between performance in maze adaptability and in maze learning.

TABLE XI

COMPARISON OF TIME-SCORES IN LEARNING AND ADAPTABILITY

	Adaptability time		Learning time	
	Success in both	Failure in adaptability	Success in both	Failure in adaptability
	(37 *S*s)	(16 *S*s)	(37 *S*s)	(16 *S*s)
Average	1060.8	2099.1	1301.5	1457.8
Median	1125.0	2182.0	969.2	1168.0
75% score	725.0	1637.0	575.0	763.0
25% score	1300.0	2281.0	1400.0	1931.0
Quartile deviation	287.5	322.0	412.5	583.0
Range	372–1987	1425–2803	237–5526	365–2895

The errors should give information,[21] not revealed in trial and time scores, concerning the relation between maze adaptability and maze learning. In Table XII, the lower third when compared with the upper and middle thirds makes more errors in learning and eliminates errors more slowly as shown by the percentage of cases that complete learning by the 35th trial.[22]

TABLE XII

COMPARISON OF THE SELECT SUCCESSFUL GROUP, DIVIDED INTO THIRDS ON THE BASIS OF TOTAL WEIGHTED ADAPTABILITY SCORE

(37 *S*s, odd *S* was grouped in middle group)

	Thirds		
	Upper	Middle	Lower
Total no. errors	635	676 (571)	978 (639)
% successful by the 35th trial	100	80 (83.3)	58.3 (63.6)

A more direct presentation of the number of errors eliminated and the rate of error elimination is revealed in Table XIII which shows (1) the total number of errors for every 5 trials, and (2) the ratio, in percentage, of the total number of errors in the first 5 trials to the total number of errors in every succeeding group of 5 trials.

TABLE XIII

TOTAL NUMBER OF ERRORS FOR EVERY 5 TRIALS AND RATIO IN PERCENTAGE OF ERRORS IN FIRST 5 TO ERRORS IN SUCCEEDING GROUPS OF 5 TRIALS, FOR THE SELECT SUCCESSFUL GROUP DIVIDED INTO THIRDS ON THE BASIS OF TOTAL WEIGHTED ADAPTABILITY SCORE

(37 *S*s, the odd *S* was grouped in the middle third)

	Upper third		Middle third		Lower third		Entire group	
Trials	Total errors	% ratio	Total errors	% ratio	Total errors	% ratio	Total errors	% ratio
1-5	201	100.0	244 (224)	100.0	238 (170)	100.0	683	100.0
6-10	141	70.2	122 (110)	50.0	185 (135)	77.7	448	65.4
11-15	106	52.8	87 (68)	35.7	152 (92)	63.8	345	50.4
16-20	98	48.8	78 (63)	32.0	125 (64)	52.5	301	44.0
21-25	63	31.4	56 (40)	23.0	117 (71)	49.1	236	34.5
26-30	22	11.0	51 (37)	20.9	87 (59)	36.5	160	23.4
31-35	4	2.0	38 (29)	15.6	74 (48)	31.1	116	16.9

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[21] The study of errors has been confined to entrance into blind pathways with the exception of one retrace error. This is an elbow turn leading from 6a true path before entering exit path 6. A diagram of this maze pattern is presented by Miles, *op. cit.*, 371.

[22] Only 35 trials of the learning curve have been included, the range of trials is 2-82. The number of trials, however, are representative of the complete learning curve, since 78% of the group (29 out of 37 *S*s) learned before or at the 35th trial.

The error curve for all sub-groups shows a sudden drop in the first 10 trials. This agrees with the fact that 34% of all the perfect tracings were made in the first 10 trials. The various comparisons indicate that the lower third, slowest in adaptability, makes most errors in learning; has most errors to eliminate; and eliminates errors at a slower rate. The middle third is superior to the upper third up to the 25th trial. At this point the error curve drops suddenly for the upper third.[23]

The coefficients of correlation in Table XIV are low and statistically unreliable. They signify only a possible trend of agreement between lower total learning time (better performance), higher intelligence test score, and higher chronological age.[24] A similar tendency appeared with the data on maze adaptability. But intelligence appears slightly more significant in maze learning than in maze adaptability.

TABLE XIV

Correlation of Intelligence Test Score and Chronological Age with Total Learning Time-Score

(37 selected *Ss*)

	r	P.E.$_r$
Intelligence test score	−.25 (−.35)	.10
Chronological	−.18 (−.05)	.11

In Table XV, the successful and failure groups in learning are presented for comparison.[25] The results in this table indicate that the failure group in learning spends more time in adaptability; the learning period is decidedly longer in terms of trial and total time; intelligence test scores are lower; and age does not appear significant.

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[23] The upper third actually completes learning at the 33rd trial; errors at the 35th trial should be regarded as zero.

[24] E. L. Thorndike (*Adult Learning*, 1928, 106, 155) says, "The general tendency from all our experiments is for an inferiority of about 15% as a result of 20 years from 22 on. Learning representing an approximation of sheer modifiability unaided by past learning shows considerably more inferiority than this. Actual learning of such things as adults commonly have to learn shows considerably less." Experience, interest, caution, care, and organization are important factors possibly favoring older individuals. R. W. Husband (Certain age effects on maze performance, *Ped. Sem.*, 37, 1930, 325-328) found that *Ss* 30 years of age were somewhat inferior in maze learning to those around 20, especially in a comparison of time scores. Gould and Perrin (*op. cit.*, 122-154) discovered that adults whose average chronological age was 21 years when compared with children whose average age was 11 yr. 6 mo., in maze learning made better records in trials, time, errors, and distance traversed. B. H. Candee (The relationship of intelligence and age to efficiency in maze learning, cited in review of literature by S. Y. Liu, The relation of age to learning ability of the white rat, *J. Comp. Psychol.*, 8, 1928, 75) says, "The high school students made better records than did the children from the grades, while the college students were slightly superior to the high school students."

[25] The number of cases varied in parts of the experiment. In learning, 24 *Ss* were used in preliminary experimentation, and consequently are not included in these results.

TABLE XV

Comparison of the Results of the Groups That Succeeded and Failed in Learning the Maze

	Succeeded (53 *Ss*)			Failed (12 *Ss*)		
	Av.	Median	Range	Av.	Median	Range
Learning time in sec.	1365.0	1042.0	237–5526	2281.1	2123.0	1098–4077
No. trials	26.4	22.6	2–82	56.0	60.0	4–100*
Adaptability time in sec.	1377.7	1300.0	372–2803	1702.9	1696.0	424–3466
Intelligence test score	193.8	191.7	73–299	170.3	161.0	75–247
Chronological age in mos.	270.3	268.1	239–317	268.9	268.0	251–299

*The required minimum for all *Ss* was 30 trials; 2 *Ss* made 4 failure trials (900 sec.) in succession.

(3) *Case study of failures in learning.* This section includes a brief description and analysis of the cases that failed to achieve the learning criterion within a total test period ranging from 2–3 hr. The value of this presentation is manifold. It will present in relief the general nature of the difficulties in this motor problem, the variety of methods of procedure, and the characteristics of the unsuccessful learner. It will also serve to reëmphasize the fact that this motor problem is not a simple trial and error task; but that it evokes a complex response which is indicative of numerous characteristics, traits, and abilities of the individual.

In reading the descriptive and observational data, it is well to recall the quantitative differentiations already made between this group and the successful group in age, intelligence, adaptability, and learning (cf. Table XV).

The 12 *Ss* who failed may be divided into two groups. One, consisting of 8 *Ss*, particularly manifests temperamental, general behavior, and personality characteristics that may account more or less for their failure in learning. A brief summary for each individual in this group follows.

S 1 appeared very tense during performance. There were frequent complaints, "don't see why it won't work," "don't see how one can concentrate," "just sheer luck if I get out of here, I'm using no plan." He seems to lack persistence and planfulness. He was very disappointed in not being able to complete learning satisfactorily.

S 2 criticized and commented on his performance throughout the experiment, "this is hard," "should have watched myself," "upper maze confuses me trying to keep my mind on lower." He knew that slight entrances were being made into vertical blinds toward body—"don't want to increase time," "disgusted with myself for making foolish errors." He claimed he is too sure of himself.

S 3 appeared very tense and hyperactive. He is easily excited; is the only child in the family. He states that he has a verbal memory defect especially in foreign languages, and difficulty of concentration. He states that he likes to talk about himself freely, but that he is shy and can't make friends easily. This and other 'factors' have been treated by a psychiatrist. During the performance he used profanity in the most abusive manner, appeared very resistive, at times almost to the point of violence; although he wanted to continue the test and give his best performance.

S 4 appeared very distractable, "can't keep my mind on this thing (maze)." He has a lethargic appearance, easily discouraged, wanted to stop work on the maze several times, complained of fatigue in hand frequently.

S 5 stated that he gets nervous in examinations. He appeared disturbed throughout the experiment, and frequently displayed restlessness by shifting feet, shifting body, twisting and turning head. His hands are very large, heavy, clammy, and have a slight tremor. He complained of task, "awful complicated," and had difficulty in coördinating movements at the beginning of the maze pattern.

S 6 stated that he is restless all the time, very fidgety, "can't sit still or read even a good book for any length of time." When an error was made, especially during learning, he would give a general bodily reaction and would ask to be returned to the beginning of the maze.

S 7 is ambidextrous. He appeared very self-conscious and lacks confidence, "I must be a pretty bad specimen." He seemed to lack persistence and ability to initiate, "if I didn't get out of the other one (90° pattern) how am I going to get out of this one (90° reverse pattern)?" "I wonder whether I'm doing what's permissible, orienting myself." (Looked away from top of maze, trial and error on lower maze). "Damned thing doesn't work," (but did not make the next move which would have been correct); seems to have difficulty in attention—"Gee, my mind seems to be off the damned thing." After the experiment he gave several instances of distractability and day dreaming in his everyday activities.

S 8 is also ambidextrous. The natural inclination was toward lefthandedness, and the right hand has been trained. Frequently he must make a conscious decision as to which hand to use in a particular task. He has a special aversion toward changing a daily habit, "develops nausea and gets nervous." Throughout the experiment he appeared nervous, would frequently tense the body, nod the head abruptly, and display facial tics and grimaces.

The remaining 4 *S*s do not give such an overt display of behavior. Two of them made very low intelligence test scores (75 and 96, that is, in the lowest 10% of the Main Group). These data are supported by observation on performance in the maze, which indicated a difficulty in comprehension of task, and a marked trial and error procedure. Both were diligent plodders. One moved very rapidly and the other very slowly.

The other 2 *S*s found difficulty in adopting a method of procedure. One case may be described as failure due to pronounced pressure at the beginning of the maze, very rapid movement, poor coördination, and poor motor control. The other case although displaying a better performance failed to adopt a uniform method of tracing through the maze.[26] This *S* seemed to have difficulty in utilizing side cues. The performance varied between following the inner and outer sides of the paths. Culs-de-sac were eliminated early in the performance.

SUMMARY

(1) This investigation shows that the ability to solve a maze problem is consistently related to the ability to learn a maze problem.

(2) General intelligence, as measured by a group test, appears more significantly related to the ability to learn a maze problem than to the ability to solve a maze problem (adaptability).

(3) Chronological age is more consistently related to performance in maze adaptability than general intelligence. Success in maze adaptability as contrasted with failure appears to be favored by a low chronological age and a high intelligence test score. But the relative degree of success is influenced by a higher chronological age, and general intelligence is negligible.

(4) General intelligence, performance in maze adaptability, method of procedure, temperament and other personality traits appear as significant factors in the case study of failures in learning.

(5) The results of this study, on the 5 maze adaptability problems, stand in opposition to the earlier results obtained by Cox: (a) significant measures are lower (smaller time-scores); and (b) correlations with general intelligence are negligible, in contrast to relatively higher correlations. However, intercorrelations (among the 5 problems) in the present investigation agree very closely with those obtained by Cox.

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[26] Success in learning seems to be dependent upon the formation of a plan for tracing through the maze or an expressed recognition of the nature of the maze pattern. Although no information was revealed as to a particular tracing, there appeared a marked uniformity of response. One method of tracing, which appeared easiest, was adopted by 68% of the *Ss*. J. A. Gengerelli (Principle of maxima and minima in animal learning, *J. Comp. Psychol.*, 11, 1930, 227) says, "The essence of the experimental findings is essentially this: an animal, faced with a situation susceptible of an indefinite number of solutions, tends to elect that one which involves the least distance."

BEHAVIORISM, PSYCHOANALYSIS, AND THE PSYCHOLOGY OF P-FUNCTION

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Subjectivism, regarded as a theory which conceives an inner mental substance and an outer physical substance as partners in a metaphysical dualism, has in recent years fallen into all but universal disrepute; but the consequent readjustment of psychological theory and practice has not yet been completely effected. In the welter of discussion provoked by this circumstance, psychoanalysis and behaviorism emerge, not necessarily as antithetical, but as extreme positions to the right and left. It now begins to appear that a central position, which should avoid the older metaphysical view and also escape the partialities of the newer extremes, would afford the psychology of our times a very great advantage. In his psychology of the P-functions Bentley has attempted such a central position. Let us examine this position and compare it right and left with behaviorism and psychoanalysis.

The functional position outlined in *Psychologies of 1930* found a partial expression as far back as 1924 in *The Field of Psychology*. But this treatise, classic though it is in certain methodological features, falters between structuralism, the best elements of which it sought to conserve, and a consistent functionalism. "The Composisition of Experience" (Part I of the *Field*) analyzes 'experience' into the usual 'elements,' sensation, sensimages, and affective qualities; while "The Organization of Experience" attempts, with doubtful success, to regard these constituent 'elements' as they stand integrated. It is, therefore, not until we come to "The Psychosomatic Functions" that we reach the expected functional treatment. Perceiving, remembering and imagining, acting and feeling, understanding and thinking, are there described as modes-in-which-the-organism-acts, operates or performs, each mode differing from the others in certain typical aspects which may be observed and described. "Socialization" and "Development of the Psychological Organism" deal with the progressive development of these functions, which are conditioned at any moment of their history by stock, growth, physico-chemical

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exchange, association with other organisms, human events, and social traditions.

This treatment of the facts of psychology obviously makes the mind (as experientially defined) a co-partner with the physical structures in the *psychosome*; and it is the functional unity of this psychosome, or mind-body, which lies behind the psychosomatic functions. Here is more than a remnant of structuralism. More than a remnant also of the various functionalisms, cognitive and biological, which have been filling our psychological shelves since Stout and Angell wrote in the '90s. It is to the elimination of these substantively different partners that the author of the *Field* addresses himself in *The Psychologies of 1930*. Here the key-note is *function*. It is function of a psychological sort (P-function, in abbreviation) which supplies all the fundamental material for psychological studies of the living organism. No mental category is employed for supplying a mental substrate or 'structure' for the P-functions. There is no longer mind-body or psychosome. There is no longer substantive coöperation. The only 'structures' to be found in the organism are found by the anatomist and the morphologist. These furnish the common ground for all functions, albeit they are to be utterly stript of every materialistic or other philosophical connotation which would tend to prejudge or to limit their functional competency.

This departure from the *Field* involves, as a corollary, a revised view of the natural relations of psychological to biological studies. The new view sets these studies in a functional coördination. The biological functions (B-functions) are of the physiological sort as regards the internal economy of the organism and of the adaptive sort as regards the active relation of the organism to its environment. The concept of 'the environment' is handed over to the biologist and quite removed, as a basal term, from the field of the psychologist.

Now it should be profitable to compare for likenesses as well as for dissimilarities this revised view of psychology—which makes function basal, distinguishes psychology from the biological sciences, rejects the 'mental' both as content and as cause, and nevertheless preserves all of the usual problems and procedures of psychology—with that form of behaviorism which has proposed to limit itself to physical, chemical, and biological terms and concepts. We take, for the purposes of our comparison, the writings of Watson.

Both Watson and Bentley have agreed that the 'traditional' views of the task of the psychologist are unsatisfactory. For Watson, this task is the description and explanation of behavior. With the completion of the behaviorist's program (which cannot, to be sure, be realized until his methods are improved) no really essential problem of introspective psychology will remain unsolved.[1] On the other hand, to impose upon psychology the task of transliterating behavior into terms of consciousness is, in any event, to proceed by analogy. It would mean that, after the behavior has been described, the conscious content of the animal or human subject would have to be *constructed*. Obviously, this involves an inference, and since an experimental science has nothing to do with inferences, the analogical procedure is absurd if psychology is to be a science.

In the second place, Watson and Bentley are more nearly agreed than might at first sight appear as to what constitutes the subject-matter of psychology. We have seen that that subject-matter consists for Bentley in certain distinctive functions of the total organism. The same is, in theory, true of Watson, who objects strenuously to the appellation, "muscle physiologist,"[2] and declares that "the behaviorist is interested in the way the whole body works," in "total activities," not in "muscle twitches."[3] In this connection, the outline of his *Psychology from the Standpoint of a Behaviorist* is illuminating. After the opening chapters, which deal with orientation and methods, the author describes, in the three following chapters, the mechanisms of behavior. If we are to credit his insistence that his program is different from the physiologist's, all of these chapters must be regarded as strictly preliminary. The rest of the book is devoted to a consideration of the functions of the 'whole organism.' Successive chapters treat of hereditary and acquired "modes of response," of the "organism at work" (turning from the acquisition of these modes to their employment by the adult individual), and of "the personality and its disturbance" (considering "how well or how poorly" the individual functions "in his present environment").[4]

Here we find a difference which is fundamental to an understanding of the sense in which either writer may legitimately be referred to as a functionalist. Watson means by 'function' either a mode of operation or a kind of achievement. Thus, when he speaks of function as a way in which the whole organism works, he is using the term in the former sense; but when he speaks of function as a product of this way-of-working, as an adjustment or adaptation, he is using it in the second sense. For Watson, the adaptive signification is primary; but precisely the opposite is true for Bentley, whose P-function always denotes a mode of performance.

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[1] J. B. Watson, Psychology as the behaviorist views it, *Psychol. Rev.*, 20, 1913, 158-177.

[2] Watson, *Behaviorism* (rev. ed.), 1930, 15.

[3] *Ibid.*, 48. Cf. his *Psychology from the Standpoint of a Behaviorist*, 1919, 40-41 (hereafter referred to as *Standpoint*).

[4] *Standpoint*, 392.

Once more, a fundamental difference arises over the criterion of objectivity. Watson defines objectivity epistemologically, *i.e.* on the basis of the traditional dualism of subject and object, of mind and body. By definition, therefore, all things 'mental' are subjective, while that is objective which is 'outside' of the observer in such a way as to be perceptible only to a bystander or onlooker. Thus, at one stroke, psychology is limited to those problems which may be envisaged from the bystander's system of reference. Bentley, on the other hand, defines objectivity in terms of method. It follows that there is for him no point in categorically denying the possibility of solving any problem that may suggest itself, because the solution will, in any particular instance, depend upon the ingenuity of the investigator, who must discover a type of procedure that will enable him to check his results experimentally at each step. A fact is objective, not with reference to a spatial analogy between an inner and an outer, but rather when manipulation of conditions reveals that (to use a figure) the universe or 'nature,' instead of the experimenter, has spoken thus-and-so. In actual practice, science knows no other form of objectivity than the methodological, a feature that enables the psychologist to test any theoretical issue that may present itself, even the behaviorist's tacit assumption that scientific laws can be formulated concerning only those phenomena which can be observed from the bystander's system of reference. When this deliverance is put to experience in the form of a question, we need go no farther than, say, the third law of color-mixture to ascertain that nature's deliverance does not coincide with the behaviorist's.

Another moment enters into the appropriation of the bystander's system of reference. In his early article on "Psychology as the behaviorist views it," Watson's point of departure is the fact that human psychology has not made good its claim as a *natural* science. This failure, we are told, is due to a mistaken conception of its subject-matter. It follows that the remedy must come through revising this mistaken conception; and behaviorism prescribes that the traditional subject-matter of psychology shall be conceived in precisely the same way as the sciences of nature (physics, chemistry, and the rest) conceive theirs. "This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier from psychology which exists between it and the other sciences. The findings of psychology . . . lend themselves to explanation in physico-chemical terms."[5]

Two questions naturally arise in this connection. First, if it is futile to construe behavior in terms of consciousness, as most psychologists would now agree, why is it any less futile to construe it in terms of electrical and chemical processes?[6] Obviously, the procedure is based on analogy in one case no less than in the other. The fact is, Watson evidently believes that the final test of scientific respectability, the "true explanation," of any biological or psychological phenomenon requires that its explanation be "couched in physico-chemical terms."[7] This brings us to a second question. Does the category of one science provide a more "true," or more nearly "true," explanation than that of another? Perhaps, if we care to give a metaphysical turn to the term 'explanation,' but if we remain on descriptive grounds, an affirmative answer is hardly plausible for the simple reason that the question itself is virtually meaningless. The purpose of the sciences is to describe, each from its own point of view, a common world. Scientific procedure is thus essentially selective, and the very excuse for the existence of more than one science lies in the fact that nature's several dimensions burst the mold of any single descriptive category. This being the case, it is nonsense to discuss degrees of trueness.

But apparently Watson is not to be deterred by the warning that questions of truth and reality should be left to metaphysics. A less militant protagonist of the bystander's system of reference would doubtless rest content with the simple declaration that the point of view is adopted and adhered to because it is believed to be the most fruitful approach to the study of psychological subject-matter. But Watson dutifully takes a critic to task for saying that the behaviorist merely ignores, but does not deny, the existence of what traditional psychology called 'mental states.' This is, he reproves, "a serious misunderstanding of the behavioristic position."[8] Passages like this plainly indicate that the behaviorist intends to cast his lot professionally with the materialistic metaphysicians. Whether this type of

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[5] *Op. cit.*, footnote 1.

[6] Cf. *Behaviorism* (rev. ed.), 210.

[7] *Ibid.*

[8] Watson, Is thinking merely the action of language mechanisms? *Brit. J. Psychol.*, 11, 1920, 87-104.

Weltanschauung is defensible does not in the least concern us here; but this dabbling in metaphysics sets Watson in sharp contrast with Bentley, who draws a clear line of demarcation between scientific and speculative thought, and carefully consigns the problem of the ultimate nature of truth and reality to the latter.

In *Psychologies of 1925*, Bentley urged that, in preparing to treat scientifically a body of facts, the nature of the facts themselves should receive first consideration. Instead of being thrust into a preconceived pigeon-hole, these facts, if they are to be understood, must be described "in such subdivisions or aspects" as their own nature suggests. It is for this reason that Bentley refrains from analyzing the P-functions into 'simples' of any kind; for, if psychologists are, after all, dealing with modes of operation, it is their primary task "to define and to depict" these functional modes as such.[9] Just as one may acquire considerable information about the products of a printing press, their value, etc., and still be crassly ignorant of the way in which the press works, so we may acquire considerable information about the accomplishment of the organism, the worth of this accomplishment from the standpoint of environmental adjustment, and the like, and still remain comparatively ignorant of the going-on-to-accomplishment.

While, as has been suggested, behavior for Watson may, in a certain broad sense, be equated with the way the whole organism works, behavior, in his hands, nevertheless acquires meanings distinctly foreign to Bentley's P-function. As a preliminary step toward pinning down what is signified by the behaviorist's 'behavior,' it is necessary to distinguish at the outset between the behaviorist's faith and the behaviorist's practice. McDougall characterizes Watson as a *strict* behaviorist who "believes that two very simple conceptions suffice for the explanation of every instance of behavior, namely, the reflex and the conditioned reflex.[10] The characterization is apt so far as it goes; but this article of faith is no more important than the belief that the "true explanation" of every instance of behavior must be couched in physico-chemical terms.

Watson bases much of his actual procedure upon units of description which are 'simples' selected from some ontogenetic or phylogenetic source. "In order to understand man you have to understand the life history of his activities."[11] Tracing the "activity stream" back to its ontogenetic source in the human infant, Watson ends his quest for a 'simple' by adopting the more or less discrete reflex movement as ultimate. Accordingly, the congenital reflexes become the elements out of which all future behavior is compounded; behavior, in this sense, being nothing more nor less than such a compound. An interpretation of a world-wide economic situation and the Babinski reflex have fundamentally the same dimension; their only difference is a matter of complexity. The program of psychology differs from that of physiology only in the type of phenomenon reduced to this lowest common denominator. That program consists in noting movements and in analyzing them into their simple elements.

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[9] M. Bentley, *Psychologies of 1930*, 1930, 95-114.

[10] Wm. McDougall, *Psychologies of 1925*, 1926, 273-291.

[11] Watson, *Psychologies of 1925*, 1-35.

In so far as Watson is faithful to this principle of reduction, it is true that the discrete reflex serves as his basic category. But Watson the psychologist is not satisfied to limit himself to a program so obviously analogous to physiology, and to the extent that he introduces non-physiological features, of course, he abandons the discrete reflex movement as a category. Thus stimulus and response are both originally defined in physiological terms.[12] But, as he goes on, stimuli are introduced which only by the widest stretch of the imagination are describable in electrical, chemical, thermal and mechanical terms; he frankly expresses his fear that, after all, the reflex is an "over simplication."[13] He even concedes that, in its present state, physiology "cannot help us much" in a psychological description of behavior.[14]

Obviously, a new and more distinctively psychological category has been introduced, namely, environmental adjustment. That an 'adjustment' and a 'reflex' cite dimensions of experience that cannot easily be resolved into one another, without speaking loosely, will, I think, be disputed by few. Certainly no physiologist could be expected to be enthusiastic over the proposal to state the difference in my reactions to two situations as due to my witnessing a hunter in the one case "shooting squirrels" and in the other case "shooting rabbits."[15] But it is no less evident that Watson slips carelessly from one category to the other. "In this volume," he remarks in *Psychology from the Standpoint of a Behaviorist*, "the terms adjustment, response, and reaction are used almost interchangeably."[16]

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[12] Vide *Standpoint*, 10-11.

[13] The fear is, however, suppressed in a parenthesis: *Behaviorism* (rev. ed.), 1930, 206-207. Cf. *Behaviorism*, 1924, 157.

[14] *Behaviorism* (rev. ed.), 210.

[15] *Standpoint*, 5.

[16] *Ibid.*, 12, footnote.

It would, of course, be a mistake to believe that the inconsistency has developed recently.[17] Watson is correct in holding that his has never been a muscle-twitch psychology; even though his contention is at most only a half-truth. In the work just mentioned, he indicates that "what we seek to have psychology busy herself with is just this matter of *environmental adjustment.*" The difference between psychology and physiology, in fact, lies precisely at this point; for "nowhere in physiology do we get the organism, as it were, put together again and tested in relation to its environment as a whole."[18] In short, the functional view of the Chicago School seems to have been embraced from the beginning: "In the main," he announces in 1913, "my desire . . . is to gain an accurate knowledge of adjustments and the stimuli calling them forth."[19]

This quotation brings us to the last important difference between Watson and Bentley. We may say that Bentley entertains the critical, Watson the Baconian, conception of science. Bentley takes seriously in his psychology the distinction between knowledge, appreciation, and use, contending that the psychologist must, if he is to *understand* his subject matter, adopt the knowing attitude.[20] Watson, on the other hand, states as the twofold aim of psychology: (1) the prediction of human activity with reasonable certainty; and (2) the formulation of laws and principles whereby man's actions can be controlled by organized society.[21] Although passages may easily be selected which fit awkwardly enough into such a view, the behaviorist's goal, as thus articulated, is inextricably linked with the discovery, by the manipulation of stimuli, of responses which are "in line with progress," "good," "desirable," and "appropriate."[22]

Such being his aim, Watson brings upon himself the necessity of treating his subject-matter causally. The control of human response depends upon a knowledge of the various types of stimuli which affect man and "of the places where such stimuli must be applied in order to produce appropriate action."[23] "Any object in the general environment"[24] may be a stimulus and its function as stimulus does not depend, theoretically, upon its relation to the organism; for causation is characteristically efficient, transitive, unilinear.[25] The stimulus incites, and then the organism moves. "The human is so built (and so are all other animals) that it *must move* when these stimuli *assail* it from within or without."[26]

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[17] Nevertheless, it does appear to be true that the psychological phase of the behaviorist's program comes into greater prominence in the revised edition of *Behaviorism* where Watsonian dogmatism is somewhat less rabid.

[18] *Standpoint*, 8 f., 20.

[19] *Vide* footnote 1.

[20] Bentley, *The Field of Psychology*, 1924, 13-16.

[21] *Standpoint*, 1-2.

[22] *Behaviorism* (rev. ed.), 20-47.

[23] *Standpoint*, 48.

[24] *Behaviorism*, 6.

We have here the causal schema of early English associationism minus a benevolent mind which manufactures ideas out of the sense material thrust upon it. As to the source of the conception, it should be noticed that, while Watson makes occasional reference to mechanics,[27] this science knows nothing of such a naïve view of causality. Unilinear causation, as a force which "produces," "calls out," "rouses," "excites," would work havoc with any sort of mathematical formula, because all content would be compressed into one side of the equation. Every cause is instead regarded as, in some sense, an effect; every force a function of relation which may be stated in terms of *differences* of velocity or work, and so on. The real source of the conception appears to be common sense, which conceives the relation of one thing to another after the analogy of certain functions of the experiencing individual. The vicious circle traversed by Watson accordingly seems to be somewhat as follows. Given a body of functions exhibiting a certain distinctive character known as agency, an anthropomorphic view of causality is derived therefrom, which, when it is projected in turn into the environment and used as an explanatory principle, obliterates the distinctive character of the virgin subject-matter. At this juncture, Bentley's motive for the substitution, for psychological purposes of description, of context for environment—whether 'inner' or 'outer'—becomes apparent. He does not believe that the P-functions, like the behaviorist's stimuli, happen non-contextually as by divine fiat. Least of all does he believe that, if and when regarded causally, the organism is a closed system. But having no oracular utterance to deliver on the subject of causality, and believing that it is not incumbent upon the sciences to provide a metaphysical *explanation* of their subject-matter, he rests content with the more modest goal of *description.*

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[25] According to the scholastics, who derived their conception from the Platonic-Aristotelian *αἰτία*, a cause was an original substance, while an effect was either an activity of this causative thing or another substance which came to be in consequence of such activity. According to the rationalists of the seventeenth century, cause and effect are motions, motion tending to be regarded in turn as substantial. While it is not here intended to attribute philosophical sophistication to Watson, it is interesting to note that he adheres mainly to the scholastic notion of cause, *i.e.* any object (in the sense of naïve epistemological realism) in the environment; and to the rationalistic conception of effect, *i.e.* some sort of motion that is reducible to a physico-chemical substance. Cf. in this connection, W. Windelband, *Geschichte der Philosophie*, 1892, 324-326.

[26] *Behaviorism*, 197 (italics mine).

[27] *Standpoint*, 392-394.

Where behaviorism inclines toward the right, psychoanalysis inclines toward the left of the psychology of P-function. Elsewhere the author has subjected psychoanalysis to more extended analysis,[28] pointing out that the practical motive, which is historically responsible for Freud's speculative approach to the meaning of the neurotic symptom, accounts primarily for the introduction of features which mar psychoanalysis as a psychological system. While the analysis was mainly critical, the constructive suggestion was offered that, since psychoanalysis deals with ways in which symptoms evolve in the life-history of the patient, these phenomena are primarily functions and functional products. Unnecessary difficulties might therefore be avoided by the elimination of the substantive and causal features. If these functions were grounded in the total organism instead of a doubtful mental substance, and if description supplanted causal explanation, psychoanalysis would swing decidedly in the direction of the psychology of P-function.

While Watson, as we have seen, proceeds in close analogy with physiology, Freud ignores physiology as irrelevant to his purpose. But like behaviorism, psychoanalysis is interested in practical control; and so the treatment here, as there, necessitates causal explanation, but in terms of mental substance instead of physical substance.

Psychoanalysis arose as "a method of treating nervous patients medically."[29] The peculiarity of several neuroses is that no corresponding organic lesions have as yet been laid bare, the correction of which will bring about recovery. "There are either no discoverable corresponding changes of the anatomical organ of the soul," Freud declares, "or else the changes are of such a nature as to yield no enlightment."[30] On account of this sterility of the physiological approach, the symptom is diagnosed and treated psychologically. "We shall entirely ignore the fact that the psychic apparatus with which we are here dealing is also familiar to us as an anatomical specimen. . . . We shall remain on psychological ground."[31] On this basis, a therapeutic program has been inaugurated, the clinical success of which confirms the contention that scientific laws can be formulated successfully from what may be called the experiencer's as opposed to the bystander's standpoint.

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[28] The categories of substance, cause and function in Freud's psychology, *Psychol. Rev.*, 39, 1932 (forthcoming).

[29] S. Freud, *A General Introduction to Psychoanalysis* (G. S. Hall, tr.), 1920, 1.

[30] *Ibid.*, 6.

[31] Freud, *The Interpretation of Dreams* (A. A. Brill, tr.), 1913, 424-425.

While in this respect psychoanalysis resembles the psychology of P-function, a difference between the two appears in the causal etiology of the former. In so far as Freud's genetic account of the symptom is descriptive, it differs in no essential detail from the genetic account of function given in *The Field of Psychology*.[32] But Freud, as has been suggested, is not content to stop with anything short of causal explanation; and his handling of the concept of causality is little, if any, more satisfactory than Watson's. Vitalism and mechanism are left side by side without any creditable attempt to remove the discrepancy between them and this despite the question as to whether the psychologist need involve himself in philosophical discussions of this nature. In fact, the negative importance of causal explanation for etiological purposes as illustrated by this inconsistency on the part of Freud appears to cast grave suspicion upon the line of argument designed to show that the scientist must provide an explanation of the subject-matter he is to describe.

The nearest we can come to a unit of explanation for Freud is unconscious idea or force. In rebelling against traditional psychology, Freud seemingly makes the common mistake of failing to replace the foundation of the view against which he launches his attack. Traditional psychology had held that psychological activity and consciousness are coextensive; that psychological subject-matter is homogeneous in kind; and that mind differs in kind from body. Being convinced, by his study of neurotic symptoms in their genetic setting, of the rôle of psychological activities which have no experiential coloring, Freud virtually equates psychological activity and the "unconscious" ("consciousness" being a sort of "sense organ" only); but the rest of the foundation is hardly shaken, except that his terminology threatens, at times, the homogeneity of his subject-matter.

As regards method, psychoanalysis is decidedly inferior to the psychology of the P-functions and to behaviorism, both of which, as we have seen, insist on high standards of experimental control. Psychoanalysis proceeds by clinical divination, and explains in terms of an assumed mental substance. The practical utility of this method when used as a therapeutic expedient may readily be conceded; but until it is brought to a more empirical level psychoanalysis can scarcely—as its defender Flügel concedes[33]—claim recognition as a science.

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[32] *Field*, 491-509.

By way of summary, we may say that the psychology of the P-functions has developed a thoroughgoing functional point of view, cutting free both from the structural type of analysis and from those compromising kinds of functionalism which have been drawn either from a teleological biology of adaptive ends or from the logic of cognition. This psychology of the P-functions differs, on the one side, from behaviorism in its system of reference and in its fundamental treatment of perceiving, remembering, acting, understanding, thinking, and the like, and, on the other side, from psychoanalysis in its expurgation of mental substances and causes. If differs from both in avoiding problems of ontology as well as in aiming at description for scientific purposes instead of at prediction and control for practical ends.

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[33] *Psychologies of 1930*, 374-394.

INTENSITY AND NUMBER OF CONES IN FOVEAL STIMULATION

By C. H. GRAHAM and N. GOLDMAN, Temple University

For a long time it has been known that when a retinal image is of large area the intensity of illumination required to produce threshold stimulation is not so great as when the image is small. The brightness required varies in an inverse manner with the area—the larger the area, the lower the intensity; and the smaller the area, the higher the intensity. Despite the knowledge of this general relationship and the popularity of the problem, few hypotheses have been put forward as promising a valid explanation until the recent work of Adrian and Matthews.[1]

Much of the early work was deficient in many details. The most notable of the defects was the failure to provide a large enough range of areas of the stimulus-objects used. Because of this fact, any of the laws having to do with the reciprocal relation between intensity and area are applicable over a rather limited range of retinal surface. The work of Piper provides us with an excellent example of this shortcoming.[2] Piper's law—to the effect that the product of the square root of the area and intensity is a constant—was derived from experiments on the periphery in which the areas of the stimulus objects covered the limited range of 1–100 sq. cm. The law of Abney and Watson[3] (*Intensity* × *Diameter* = *Constant*) was obtained with a range which was even smaller than that of Piper. Piéron, as the result of a series of studies, has come to the conclusion that no general equation can satisfy the relation over a large range of areas.[4]

Reeves has performed one of the few experiments which may be considered adequate.[5] His range was large, the areas of the stimulus-surfaces varying from 0.75–14,400 sq. mm. One of the authors of this paper, as a preliminary to more extended investigation, attempted to give a quantitative formulation to Reeves' results. He found that the equation $AI^{1.3} = Constant$ holds over the middle range

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[1] E. D. Adrian and R. Matthews, The action of light on the eye, *J. Physiol.*, 63, 1927, 378 ff.; 64, 1927, 279 ff.; 65, 1928, 273 ff.

[2] H. Piper, Über die Abhängigkeit des Reizwertes leuchtender Objekte von ihren Flächen bezw. Winkelgrösse, *Zsch. f. Psychol.*, 32, 1903, 98.

[3] W. deW. Abney and W. Watson, The threshold of vision for different colored lights, *Philos. Trans.*, 216, 1916, 91.

[4] H. Piéron, La sommation superficielle des impressions lumineuses au niveau de la fovea, *C. r. Soc. Biol.*, 101, 1929, 1104.

[5] P. Reeves, The effect of size of stimulus and exposure-time on retinal threshold, *Astrophys. J.*, 47, 1918, 141.

of areas, but does not describe the relationship with the small and large stimulus surfaces.[6]

The researches of Adrian and Matthews provide the first step toward a satisfactory analysis of the problem. Such an analysis will be of immediate importance for our understanding of both photochemical and neural activity during light stimulation, and of general importance in so far as it contributes to our knowledge concerning the nature of synaptic transmission.

In an experiment involving the measurement of action currents from the optic nerve of the eel, Adrian and Matthews found that when the area of the stimulus-object was changed without varying the illumination there was a reduction in the latent period of the discharge, a quicker rise to the maximum and an increase in frequency which was not nearly proportional to the area stimulated. "It is obvious that these facts do not agree with the simple conception of the retina as a mosaic of sensitive points each connected with its own nerve fibre and each acting independently of the rest. The shortening of the latent period when the size of the retinal image is increased can only be explained on the assumption of some kind of interaction between the different stimulated points."[7]

Later experimentation has demonstrated that the interaction takes place in the nervous interconnections of the synaptic layers of the retina. This conclusion is a result of analyses made of rhythmic discharges found in the optic nerve of the eel when the entire retina is stimulated for a long time. Under such conditions of illumination the action current discharge loses its irregular character and consists of a series of regular waves at frequencies ranging from 5–15 a sec. This process is not due to rhythmical stimulation of the retina for when strychnine is applied to preparations which do not normally show the intermittent discharge, the synchronous activity appears at once.[8] If the process were photochemical we should suppose that strychnine would have no effect. Again, if four points on the retina are illuminated simultaneously, the reaction-time of the discharge is shorter than when any one of the points is stimulated alone. Strychnine causes a tremendous increase in the range over which this interaction occurs.

It seems safe to assume that the interaction process is a summation effect which takes place in the synaptic layers of the retina. Experiments of the type previously described leave no doubt as to the fact that the parallel effects of increase in area and increase in intensity are due to variations in the nervous summation process demonstrated.

PROBLEM

The purpose of this paper is to make an analysis of the relationship existing between the threshold intensity of illumination and the size of area stimulated for the fovea of the human eye. Since determinations made in terms of area mean little (because of the manner of distribution of foveal elements), the investigation is extended for the purpose of making a correlation between *number of cones* available for stimulation and *intensity* of illumination. It is hoped that such a procedure will give a more accurate picture of the fundamental phenomena investigated than has been the case when *area* has been the variable. In order to make the analysis as complete as possible, the areas used have been chosen to cover a range varying from one which forms a retinal image covering as few as 12 to 16 cones to one which forms an image involving nearly the whole extent of the fovea.

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[6] C. H. Graham, The relation between area and intensity of visual thresholds, *Amer. J. Psychol.*, 42, 1930, 420. An error was made in this report which should be corrected. The retinal areas as given by the graph are all four logarithmic units too small. Every point on the curve should be raised that amount if the areas are to be read in sq. mm. This error does not, however, affect the validity of the equation. The only term changed by the transposition is the constant.

[7] Adrian and Matthews, *op. cit.*, *J. Physiol.*, 63, 1927, 378.

[8] *Idem.*, *J. Physiol.*, 65, 1928, 273.

METHOD

Apparatus. Our apparatus provides facilities for experimental precautions which have been overlooked by previous investigators of the problem, none of whom thought it necessary to provide a uniform starting point for the dark-adaptation process. The *S*s were allowed to stay in the dark for varying intervals up to the time when the threshold was determined. In no case was there a previous light-adaptation period.

Accurate and uniform results are obtained only when there is a uniform starting point and a uniform determination point. The uniform starting point can be accepted as the time when the eye is completely light-adapted. The determination point is taken as the time when the fovea is completely dark-adapted. The apparatus of the present experiment provides us with means of controlling this phase of the work.

Observations were made in a dark room. Light-adaptation was ensured by having *S*s look from a definite distance at a light-adapting surface for a uniform period of time. The dark-adaptation process was initiated by switching off the surface illumination. After a definite period of dark-adaptation *S* moved his head, placed his eyes at the observation openings, and was ready for a measurement. The threshold-determining objects were white circles of varying area, a different area for each determination. *S* was guided in his fixation by two small spots of light (luminous paint) which were at a horizontal distance of 25 cm. from the threshold object, the latter being in a position halfway between the spots. The areas of the circles were such that the range of visual angles subtended varied from 2.4′ to 3°.

The apparatus was, except for the stimulus-surfaces and the orientation spots, exactly the same as that used by Graham in an investigation of foveal binocular summation.[9] It consists of three principal parts: (1) A light-adapting screen having a brightness of 115 millilamberts. This is placed in a large, wall-board screen. In the screen are the observation openings which are fitted with artificial pupils of 4 mm. diam. (2) A threshold-determining object—a circle of white light—the area of which can be varied by means of cardboard diaphragms of different areas. These diaphragms are slipped into a groove in front of the milk-glass plate through which the light from the threshold-determining lamp passes. In the groove, behind the diaphragm, but in front of the milk-glass plate, is a holder into which neutral tint filters (Wratten) of varying densities may be slipped. These are used for gross adjustments of intensity. The threshold disk is at a distance of 75 cm. from S's eyes. (3) A long, blackened box containing a 25-watt concentrated-filament bulb. (This lamp is behind the stimulus-object which is in the front part of the box.) Variations in the intensity of illumination of the white stimulus-circles are made by moving the lamp away from or toward the front of the box. Intensities are determined from a measured reading on the basis of the inverse square law.

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[9] C. H. Graham, An investigation of binocular summation: I. The fovea, *J. Gen. Psychol.*, 3, 1930, 494 ff.

Some slight modifications were made in the apparatus of the previous experiment. A buzzer was provided as a signalling device for S's reaction to the threshold stimulus. When the S saw the white circle he pressed the key controlling the buzzer, and at the sound E switched off the threshold light. Artificial pupils of 4 mm. diam. were placed in the observation openings. A slide which could be adjusted by S (at the command of E) was used to cut off vision through either of the pupils. In practice E gave the order as to which of the pupils was to be covered.

Procedure. The S was seated in a chair (which was placed in a fixed position), with his eyes on a level with the artificial pupils and at a distance of about 35 cm. from the light-adapting surface which was slightly above them. Two minutes were allowed for light-adaptation. (At this intensity —115 millilamberts—light-adaptation is complete in this time.) During this interval E made the necessary adjustments of the apparatus, *i.e.* the slide determining the observing eye was set, the neutral tint filter was placed in its holder, and the diaphragm was slipped into the groove. At the end of the light-adapting period the lights were turned off and the dark-adaptation process was initiated. A period of 3 min. was allowed for dark-adaptation (enough for the fovea). A warning signal was given 10 sec. before the end of this period, and the determination of the threshold was made at the end of the third minute.

A movement of about 15 cm. was enough to bring S's head to the chin-rest and artificial pupils. He had been instructed to fixate the center of the area between the two small luminous paint spots and to press on the buzzer key when he saw a white *circle.* The criterion of form was used in order to meet the requirements of foveal perception.

When the determination was to be made E gave the signal, switched on the 25-watt light and moved the carriage toward the S at a rate of about 10 cm. per sec. When the buzzer sounded the carriage was stopped, the stimulus light was switched off, the distance on the scale was read and the carriage moved back. The distance on the meter stick was recorded.

After the determination had been made, the S moved the slide so as to allow observation with the other eye, and within 30 sec. another determination was made. With the completion of the threshold measurements for the two eyes the experimenter switched on the light-adapting lights and the same procedure was repeated with a diaphragm of different area. All timing operations were made by means of a stop-watch. Four determinations were made with every S for each area of diaphragm—two for the left eye and two for the right.

The preliminary work occupied more time than was required by the experiment reported here. Results were obtained under several types of stimulating conditions. Among these were some which involved threshold determinations for red light (Wratten filter No. 70) with and without artificial pupils, and some in which the stimulus-patches of white light were observed without artificial pupils. In these latter series a greater number of areas were used in the middle ranges than is the case in the main experiment, but the trend of the results was found to be so regular that only 12 disks of different area were used in the final measurements. In addition to these variations in procedure, we tested for the effects of differing periods of light- and dark-adaptation, and for the influences dependent on the spacing of the luminous paint spots. Our experience in the early experiments has led us to believe that the procedure finally adopted is one which approaches the optimum.

Results

Area and intensity. Measurements were made with 5 *Ss.* The averaged data are presented in Table I and in Fig. 1. The relation expressed by these two methods is so similar to the relation given by every *S* individually that we are content to reproduce only the group averages. The table and graph give an expression of the way in which the threshold intensity decreases as the area of the retinal image increases.[10] With the artificial pupils in use the pupillary area was 12.57 sq. mm. for all *Ss.* Retinal areas were calculated on the assumption that a visual angle of 1′ subtends a retinal distance of 0.005 mm.

A survey of Table I and of Fig. 1 makes it clear that as the stimulus-area increases in size the threshold intensity decreases.

TABLE I

Relation Between Area and Intensity of White Light Thresholds

Stimulus area	Diameter (cm.)	Visual angle	Retinal area (sq. mm.)	Intensity (photons)
1	0.054	2.52′	0.0001131	1.96
2	0.07	3.24′	0.0001985	1.23
3	0.1	4.56′	0.0004085	1.19
4	0.125	5.76′	0.0006396	0.339
5	0.14	6.36′	0.0008050	0.239
6	0.162	7.20′	0.001087	0.194
7	0.2	9.12′	0.001630	0.104
8	0.5	22.68′	0.01031	0.056
9	1.0	48.08′	0.04126	0.012
10	2.0	91.56′	0.16503	0.0093
11	3.0	138.00′	0.37133	0.0064
12	3.8	172.80′	0.59529	0.0071

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[10] The intensities are expressed in terms of photons, a physiological unit of intensity proposed by Troland which makes allowance for variations in pupil size (cf. L. T. Troland, The measurement of visual stimulation intensities, *J. Exper. Psychol.*, 2, 1917, 1-24). Hecht has modified Troland's definition and regards the photon as the retinal illumination produced when the eye looks at a brightness of 1 millilambert through a pupil of 1 sq. mm. (cf. S. Hecht, The relation between visual acuity and illumination, *J. Gen. Physiol.*, 11, 1928, 261).

The relationship, however, is not regular. Between areas 3 and 4 a distinct break is to be noted in the intensity values. This means, of course, that the small areas (1, 2, 3) require an increase in intensity which is out of proportion to the decrease in area. Considerations concerning the possible significance of this break in the curve will be taken up later. A similar break in the intensity values occurs between areas 8 and 9. The results as obtained in the preliminary experiments without artificial pupils (those in which a greater number of intermediate determinations were made) seem to verify this observation. An interpretation of this discontinuity will be made later also.

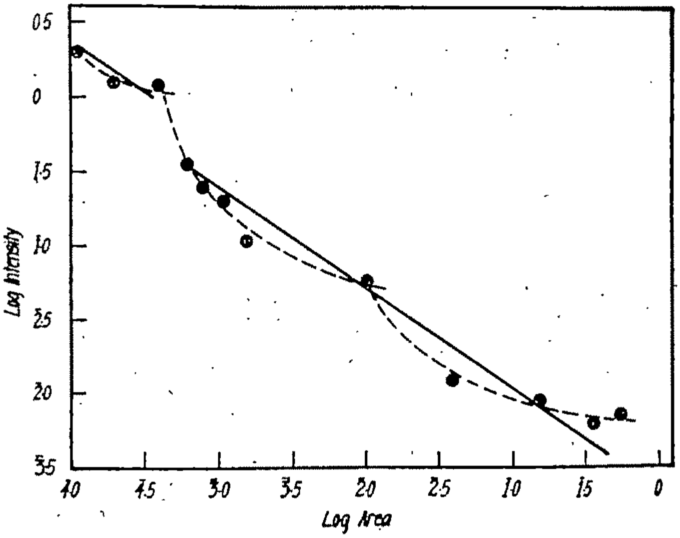

FIG. 1. RELATION BETWEEN THE SIZE OF THE STIMULUS-AREA AND THE INTENSITY THRESHOLD

The two solid straight lines drawn through the points of the curve indicate the fidelity with which the equation obtained with Reeves' data applies to the present measures. These lines are drawn so that any point satisfies the equation $AI^{1.3} = K$, (the value of K differing in each line) or approximately $A^{0.75} I = k$. (It makes little difference whether we consider area or intensity as the independent variable.) We may note that this equation provides only the crudest of empirical descriptions. The broken line through the data is a more accurate index of the true relationship.

The results as presented make it clear that an increase in the number of retinal elements available for stimulation has as its correlate a decrease in the amount of light energy per cone necessary for stimulation. The reciprocal relation is not a regular one. There are two discontinuities in the curve relating the two variables. These will be clearly distinguishable in the curves to be presented in a later section.

In considering the discontinuity in the curve which is found after area 3 the writers find it necessary to raise a point which centers around the following fact. When we work with stimulus-objects of small area it is certain that the size of the retinal areas stimulated are not those which geometric optics would indicate. This state of affairs is due to conditions of scatter and other optical defects. It might be argued that the break in the curve following area 3 is due to such factors. Such a hypothesis may be tested by plotting the product of area and intensity against the diameter. If the break still occurs, we must assume that scattered light cannot account for our results. The reason for such a graph is clear. If the light is scattered the stimulated area is larger than that dictated by geometrical optics, but the intensity per unit area is smaller. The product of area and intensity when plotted in the previously described manner should show no discontinuity if this source of error is a factor. When the results of the present experiment are treated in this manner it is found that the break still occurs after area 3.

We may mention that with red light (without artificial pupils) the equation *Intensity* × *Diameter* = *C* seems to hold over a wide range of the curve. The three smallest areas tested in the final series with white light were not, however, used in these early observations. The constant varies in value from 0.00019–0.00028. The preliminary experiments with white light (smallest areas eliminated, no artificial pupils) show that the same relation holds. In this case the constant varies between 0.00013 and 0.0002. Intensities are in terms of millilamberts. The diameters are the retinal diameters. Little significance is attached to these expressions because of the fact that it seems impossible to derive any meaning from them.

Intensity and number of cones. In order to arrive at a better understanding of our results we have extended our analysis to take into consideration the number of cones available for stimulation in each of the retinal areas tested. A formulation involving *area* and *intensity* means little, because of the fact that the cones are not distributed in such a manner that we can consider the number as directly proportional to the area. The population is less dense along the superior-inferior axis than along the temporal-nasal axis. Further than this, there is a definite decline in the number of cones per unit area as the distance from the center of the eye increases.

Determinations of the number of cones available for stimulation in each of the retinal areas tested were made on the basis of the observations of Rochon-Duvigneaud.[11] Photomicrographs of sections through the fovea comprise an important part of the work of this investigator. All of the photomicrographs are of vertical sections showing the different layers of the retina and give excellent lengthwise views of the cones. Since the magnification is given, we were able, by careful measurement, to determine the *linear* number of cones contained in given *distances* from the center of the fovea. The measurements were carefully checked with those of Rochon-Duvigneaud with respect to width and height of cones. Because of the fact that the sections were cut only along the superior-inferior axis, we could not determine the average distribution along the four axes,

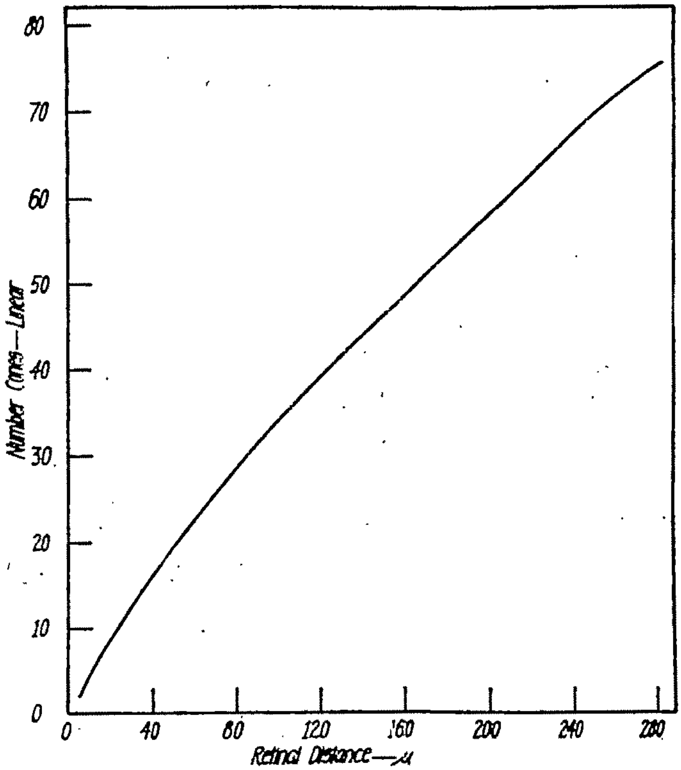

FIG. 2. RELATION BETWEEN THE NUMBER OF CONES AND THE DISTANCE FROM THE CENTER OF THE RETINA

and so we present the data as the average distribution of cones for the superior and inferior axes only.

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[11] Rochon-Duvigneaud, Recherches sur la fovea de la rétine humaine, *Arch. d'anat. microscop.*, 9, 1907, 315.

Fig. 2 gives a graphical presentation of the way in which the number of cones (linear) varies with the distance from the center of the retina. This graph was drawn on a large scale, and when the number of cones for a given radius of retinal area was determined we read off the number of elements corresponding to the retinal distance of the radius. This reading was checked by means of linear interpolations of the tabulated material. Results obtained by this method are given in Table II.

TABLE II

INTENSITY AND NUMBER OF CONES ALONG RADIUS OF RETINAL IMAGE

Stimulus area	No. Cones	Intensity (photons)
1	2	1.96
2	3	1.23
3	4	1.19
4	5	0.339
5	6	0.239
6	7	0.194
7	9	0.104
8	20	0.056
9	47	0.012
10	64	0.0093
11	85*	0.0064
12	96*	0.0071

*These two values were determined by extrapolation of the curve constructed from the data of Rochon-Duvigneaud.

Here are presented the number of cones lying along the radius of each of the retinal images given by the different sizes of diaphragm together with the corresponding threshold intensities. Fig. 3 gives a graphical picture of the relationship. The logarithmic graph is presented for the reason that the range of intensities encountered warrants such treatment. While it is true that the number of cones is not so great as to necessitate similar manipulation, it is certain that the data appear in a more balanced manner if the procedure is adopted in the treatment of the cones also.

In order to arrive at an approximation to the number of elements in the different retinal areas, the number along the radius was squared and multiplied by a constant which careful estimation determined as 4. This expression is equal to the square of the number lying along the diameter. The number as calculated in this manner takes care of the fact that the number lying on the superior-inferior axis (the basis of our calculations) is smaller than the number lying on the temporal-nasal axis.

The relationship existing between the available number of cones and the threshold intensities is given in Fig. 4 which is a graph of *log I* as a function of *log Number Cones* (total). Figs. 3 and 4 show that the discontinuities found in Table I and Fig. 1 still exist in the present treatment.

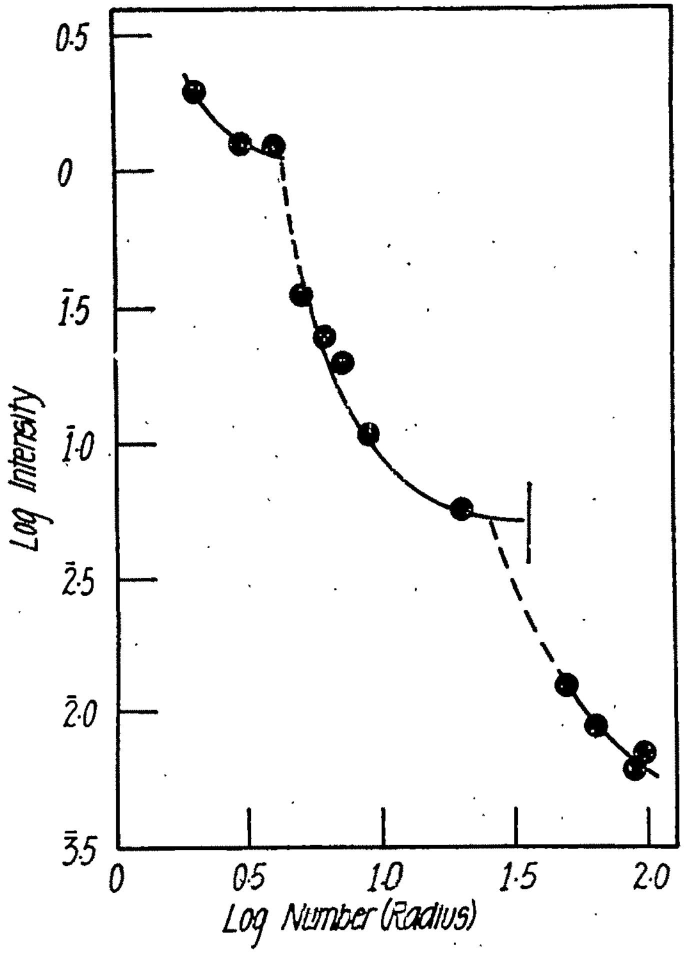

FIG. 3. RELATION BETWEEN THE INTENSITY THRESHOLD AND THE NUMBER OF CONES LYING ALONG THE RADIUS OF THE RETINAL IMAGE

DISCUSSION

In discussing the results of this experiment we have consistently stressed the fact that the relationship between number of cones and intensity is a discontinuous one. The first break in the curve occurs when the retinal area stimulated increases from approximately 0.00041 sq. mm. to 0.00064 sq. mm. (64 to 100 cones). At this point there is found a precipitous decrease in the intensity necessary for stimulation.

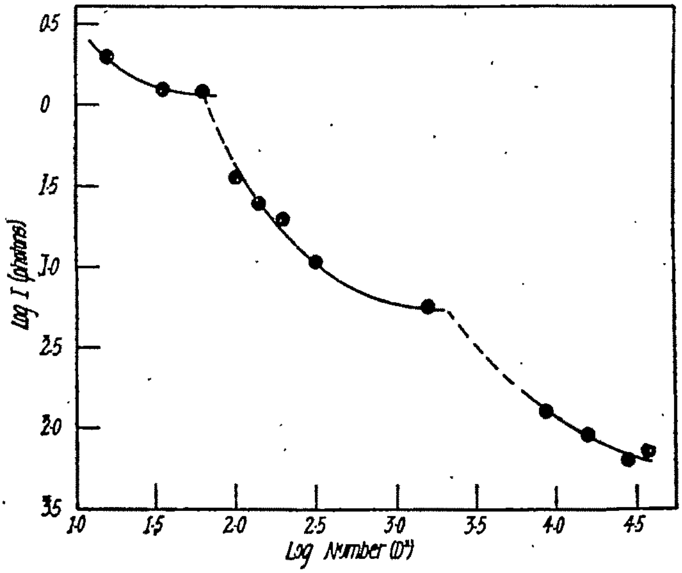

FIG. 4. RELATION BETWEEN THE INTENSITY THRESHOLD AND THE TOTAL NUMBER OF CONES AVAILABLE

The second abrupt change occurs when the number of cones increases from 1600 to 8836 (areas 0.01 sq. mm. to 0.041 sq. mm.). (This break was verified in the earlier series under conditions where a greater number of intermediate areas were presented to the subject.) We are led to believe that this latter change of slope is due to the presence of functioning rods. Such an interpretation is made plausible because of the fact that the diameter of the rod-free area has been determined as about 200μ (Rochon-Duvigneaud). The vertical line in Fig. 3 designates the approximate position on the curve where the rod-free area ends. It is to be noted that the position of this line and the break in the curve coincide very well. Because of the agreement we find it probable that the second discontinuity can be attributed to the presence of rods.

It is impossible to attribute the abrupt change after the third of the smallest areas to a similar anatomical feature of the fovea. The photomicrographs show that there is no topographical correlate for the sudden change in slope. We are left, then, with the conclusions that the discontinuity must have its basis in some functional process.

The data show that with very small retinal areas (making available for stimulation only a small number of cones) a disproportionately high light intensity is required to produce a threshold effect. When the number of end-organs reaches a certain minimum (approximately 50–100) there is a decidedly lower light requirement. The only interpretation which can be made of such a disproportionate decrease in the light intensity is that there is coming into play a type of physiological process which requires a certain number of end-organs before its threshold of activity is reached. When the number of cones is below this threshold number, each element requires a high intensity for stimulation.

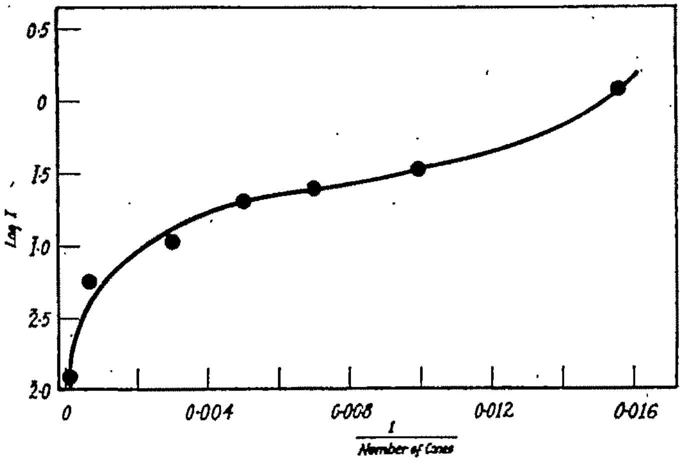

FIG. 5. RELATION BETWEEN LOG I AND THE RECIPROCAL OF THE NUMBER OF CONES AVAILABLE

(Because of the range of stimulus-areas used and the difficulty of representing all of them in one graph, the data from areas 1, 2, 10, 11, and 12 were not included in constructing this curve.)

The form of the curve after the first break is approximately what one would expect from the work of Adrian and Matthews. As the area increases the threshold intensity of light decreases in a regular, hyperbolic manner until (if we judge from the form of the curve) an increase of area ceases to affect the intensity. On the other hand, the behavior of the small areas is best described by an independent curve.

Since the relationship of area and intensity over a large range is not a simple one we must conclude that the total process initiating the nerve impulse is different with very small areas from what it is with larger areas. There can be little doubt that the sudden change in intensity as the number of cones increases from about 50 to 100 is due to the development of the summation process which takes place in the synaptic layers of the retina. The mechanism of the process is obscure. The results of this experiment point to the fact that with small numbers of end-organs there is little or no summation. As the number available for stimulation increases from approximately 50 to 100 the threshold for interaction (or at least for a more efficient interaction process) is reached.[12]

We are led to believe that our results cannot be interpreted on the basis of an alternative possibility, *i.e.* Hecht's concept of a statistical distribution of thresholds as affording an explanation for the area-intensity relation.[13] It would follow from the application of this idea that a plot of *log I* against the *reciprocal of Number of Cones Available* would give an approximately straight line with positive slope. Such a state of affairs follows from the assumption that the number of elements brought into play in a given region is approximately proportional to the logarithm of the intensity, and from the idea that it is the number activated which determines the threshold. Fig. 5, which shows the data of this experiment treated in this manner, demonstrates considerable deviation from this expectation. Although the test cannot be considered critical, the deviation from the expected relation leads us to examine our results in terms of interpretations dealing with synaptic reactions similar to those demonstrated by Granit and Harper[14] who have shown that reflex activities having summation and occlusion aspects play definite rôles in vision. Statistical distribution of thresholds is not excluded as a contributing factor, but it is difficult to see how it alone can explain the results.

SUMMARY

A critical summary of some of the early work on the problem of the relation of area and intensity of visual thresholds has been presented. It is shown that in most instances the experimental work has been inadequate. There follows a brief description of the recent work of Adrian and Matthews on the measurement of action currents from the optic nerve of the eel, together with a discussion of the theoretical implications of the investigation.

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[12] It is necessary to point out that the possibility of a shift in fixation in the case of the small areas cannot account for the results. Such shifts are known to occur in dark-adaptation. However, such a shift would be in the direction of a retinal area of higher sensitivity and the threshold would be lower. Further, we believe that the presence of the luminous orientation spots would eliminate all tendencies in this direction.

[13] S. Hecht, *op. cit.*, 278.

[14] R. Granit and P. Harper, Comparative studies on the peripheral and central retina: II. Synaptic reactions in the eye, *Amer. J. Physiol.*, 95, 1930, 211-228.

By means of a method which has been described we attempted to determine the relationship existing between the intensity of the threshold stimulus and the area of the retina (predominantly the fovea) stimulated. The results as obtained make it clear that no simple relation exists between the two variables.

Nor is there a simple relation existing between *intensity* and *number of cones* for the areas tested. With small areas a disproportionately high light intensity is necessary for stimulation. When the area is such that it comprises 50 to 100 cones there is a sudden decrease in the intensity required. This change of relation is interpreted as signifying the arousal of an interaction process which requires a minimum number of end-organs (or nerve fibres) before it reaches its maximum efficiency.

Another discontinuity is found in the curve relating *number of cones* and *intensity* when the number increases from 1600 to 8836. We are led to believe that this change of relation is brought about by the presence of functioning rods. Determinations concerned with the limits of the rod-free area strengthen this supposition. It is found that the number of cones within the rod-free area and the number in the vicinity of this break are approximately the same.

AFFECTIVE CONTRAST BETWEEN MODALITIES

By Albert J. Harris, Purdue University

In a previous paper by the writer,[1] evidence was presented which showed that affective contrast between experiences of the same modality is of the nature of mass-contrast, adequately described by the law of affective equilibrium. This law is stated as follows: "The affective value of the experiential correlate of a stimulus varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group."[2] From that evidence no prediction could be made whether affective contrast could occur between experiences of different modalities, or what the nature of such contrast, if found, might be. The experiments reported below were performed in an attempt to answer these questions.[3]

Experiment I

The apparatus consisted of (1) 90 white cards, 4 in. square, on which were mounted 1-in. squares of 90 different Milton Bradley colored papers; (2) 23 1-oz. bottles containing 5 cc. of 23 odorous liquids;[4] (3) the presentation-box—a large box with metal top and front, containing a glass turntable. Stimulus-bottles placed by *E* on the turntable, at the open back of the box, could be brought quickly around under a hole, in the top front, at which the *O* sniffed. Color cards

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*Accepted for publication March 17, 1931.

[1] A. J. Harris, A study of affective contrast, this Journal, 41, 1929, 617-626.

[2] J. G. Beebe-Center, The law of affective equilibrium, *ibid.*, 41, 1929, 64.

[3] A more complete account is given in the writer's dissertation, *Studies of Affective Contrast*, on file in the Harvard University Library. The writer wishes to acknowledge his indebtedness to Dr. J. G. Beebe-Center, for suggestion of the problem and invaluable help throughout; and to Dr. E. G. Boring and Dr. H. H. Remmers for valuable criticism, especially on the statistical aspects of the work.

[4] The odorous liquids in approximate order of pleasantness, were:

Oil of jasmine
" " Ceylon cinnamon
" " sweet orange
" " cloves
" " carnation
" " bergamot
" " cananga
" " juniper berries
" " anise
" " nutmeg
" " fennel
Oil of thyme
" " petitgrain
" " patchouli
" " rosemary
" " rose geranium
" " cumin
Denatured alcohol
Cod liver oil
Butyl nitrate
Tincture of asafoetida
Valeric acid
Carbon disulphide

The essential oils were obtained from Fritsche Bros., N.Y.C.

were exposed through an aperture in a gray cardbord screen, attached to the top of the box. The *Os* were Messrs. W. D. Turner (*Tu*), K. W. Oberlin (*Ob*), H. L. Kim (*Ki*), and Miss D. Selling (*Se*). All were experienced in making affective judgments.

In a preliminary series of judgments, the 9 most P (pleasant) and 9 most U (unpleasant) odors were selected for each *O*, giving individual P odor sets and U odor sets. On the basis of previous results, *E* selected for each *O* a set of 20 colors about evenly distributed over the scale from very P to very U, for that *O*. The experiment proper involved the presentation of the 20 colors alternating with 9 odors in one continuous series. A haphazard order of presentation was arranged in which each color followed each of the 9 odors once. The experiment was divided into two parts, in one of which P odors were used, and in the other, U odors. For *Tu* and *Se* the P set was used in Part I and the U set in Part II; for *Ob* and *Ki* the U set was given first. Each part was completed in 3 hours, spaced 1 week apart, with no extra interval between parts. Each hour was divided into 3 series of 20 colors and 20 odors each. The colors and odors were presented alternately, with an odor always first, and with an interval of approximately 15 sec. between each 2 stimuli. There was a rest of 3 min. between series.

The instructions were as follows:

> Odors are to be presented to you one at a time. You are to take two sniffs of each, and report whether the odor is very pleasant, pleasant, indifferent, unpleasant, or very unpleasant. After each odor a color will be presented. You are to judge the color in the same way as the odors. Try to judge each stimulus independently, *avoiding all memory or association effects*.

Protocols made at the end of each hour were recorded verbatim by *E*. In calculating results, the judgments were given numerical values as follows: very P, +2; P, +1; indifferent, 0; U, −1; and very U, −2.

Contrast might appear in this experiment in either of two ways. In the first place, each color might be more P after the more U odors of a given set than after the more P odors of the same set; this would be an individual contrast effect. In the second place, there might be a mass contrast effect, with the colors as a group, judged to be more P when intermingled with the U odors than when intermingled with the P odors.

Individual effect of the odors. As each color followed each of the 9 odors of a given set once, comparison of the average affective value of the 20 colors following one odor, with the average value of the same colors following another odor of the same set, will indicate any difference in effect of the two odors. Table I shows the values obtained when this is done. It is noticeable that the figures on the *right* side of each horizontal line of averages are, if anything, slightly higher than the averages on the left side of the same line in any case. If there were individual contrast, the averages on the *left*, following the relatively U odors, should be considerably higher. Furthermore, when the total averages are considered, the uniformity of the 9 columns is striking. The average for the colors following the least P odors and that for the colors following the most P odors are equal. There is no indication that the individual odors of a given set had any differential effect upon the pleasantness of the colors. We may conclude, then, that there is no evidence of individual contrast in this experiment.

TABLE I

AVERAGE AFFECTIVE VALUES OF THE COLORS FOLLOWING THE INDIVIDUAL ODORS

	Odor	(Least P)		Rank Order of Odors						(Most P)
O	Set	1	2	3	4	5	6	7	8	9
Tu	P	1.30	.85	1.55	.95	1.45	1.45	1.30	1.10	1.40
	U	.60	.45	.55	.50	.85	.70	.55	1.00	.80
Ob	P	.55	.30	.48	.48	.30	.30	.40	.35	.55
	U	.65	.25	.40	.55	.55	.65	.55	.43	.43
Ki	P	.80	.85	.85	1.10	.80	.65	.70	.60	.75
	U	.60	.60	.25	.35	.40	.40	.75	.45	.45
Se	P	.00	.30	.15	.20	.25	.10	.10	.15	.20
	U	.05	.20	.15	.00	−.05	.15	.00	.15	.00
Av.		.57	.48	.55	.52	.57	.55	.54	.53	.57

Mass effect of the odors. In Table II the total average affective value of the colors is shown for the two conditions, presented with P odors and with U odors. For 3 of the *O*s, the average is higher with P odors than with U odors. This type of effect—the opposite of contrast—we may call an assimilation effect. For the fourth *O*, there is a slight shift in the direction of contrast.

The question immediately arises whether these shifts are actually the result of the change in odors, or show merely chance variation. Two statistical methods are available for testing the probable significance of the shifts.

In one of these methods, which we may call Method X, the average affective value of the colors in each part of the experiment is considered as the mean of 9 values, which are the average affective values of the 9 successive presentations of the 20 colors. The standard error of each mean can then be found, and the standard error of the difference between the two means.[5]

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[5] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 128-33.

In the second method, Method Y, the shift is considered to be the difference between the number of colors that on the average became more P with a change in odors, and the number of colors that on the average became more U. The number of colors that on the average became more P with a change in odors, the number of those whose affective value did not change, and the number of those that on the average became more U, are given in columns 5, 6 and 7 of Table II. It is obvious that the greater the preponderance of changes in one direction over changes in the other is, the more significant it is. The relative significance of different degrees of preponderance can be stated statistically. The standard deviation of the proportion for a given category may be obtained by use of the formula:[6] $\sigma_{\text{prop}} = [(\text{p.q.})/\text{N}]^{\frac{1}{2}}$.

When the standard deviations for the two categories (colors that became more P and colors that became less P) have been obtained, the standard error of the difference between the two categories (preponderance in one direction) may be found in the usual way.

In general, the difference between two measures may be considered maximally significant if it is 3 or more times as large as its standard error.[7] The results obtained by use of the two methods are shown in columns 4 and 8 of Table II. It is apparent that by both methods the shifts for *Tu* and *Ki* are statistically significant, and that by both methods the shifts for *Se* and *Ob* are of very doubtful significance. It will, of course, be noted that the standard errors are based on what would ordinarily be considered too small a number of cases.

TABLE II

SHIFTS IN THE AFFECTIVE VALUE OF THE COLORS WITH A CHANGE IN ODOR SETS: EXPERIMENT I

O	1 Color av. after P odors	2 Color av. after U odors	3 Shift in color av.	4 Method X $D/\sigma_{\text{diff.}}$	5 No. colors higher after P odors	6 No. colors with no change	7 No. colors higher after U odors	8 Method Y $D/\sigma_{\text{diff.}}$
Tu	1.25	.67	.58 a*	5.58	17	1	2	7.21
Ob	.40	.49	.09 c*	1.32	7	2	11	1.30
Ki	.79	.47	.32 a	3.45	15	3	2	5.04
Se	.17	.08	.09 a	2.78	9	5	6	.99

* 'a' indicates assimilative shift; 'c' indicates contrast shift.

That is an unavoidable result of the experimental procedure, and must be considered as a limitation upon the conclusions to be drawn.

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[6] T. L. Kelley, *Statistical Method*, 1923, 86-90.

[7] Garrett, *ibid*, 134.

The protocols of the *Os* agree with the statistical conclusions. *Tu* reported, after an hour with the P odors, that a state of great exhilaration had been built up, and that, although he tried to avoid calling the colors more pleasant because of this general state, he did not know if he had succeeded. *Ki* reported, after an hour with U odors, that it was harder than usual to get pleasure from the colors. Other similar reports were made by these two *Os*. *Ob and Se* both reported that the odors had no effect upon their judgments of the colors.

Discussion. This experiment shows definitely that affective contrast is not the usual result of inter-modal affective stimulation. Of the 4 *Os*, only one gave any sign of contrast, and then the amount was so small as to be statistically doubtful. The experiment also shows that for some *Os* an assimilative effect may occur; colors will be judged to be more P when intermingled with P odors, and less P when intermingled with U odors. The influence in these cases seems to be the result of a general mood created by the odors.

The idea that moods may influence affective judgments is not new. The effect of a headache or toothache upon one's aesthetic appreciation is a commonplace of everyday experience. Experimental evidence on the question, however, is slight. Recently, P. T. Young has reported a case in which, during an experiment on the constancy of affective judgments, a marked drop in pleasantness occurred on a day when the *O* was in an unusually unpleasant frame of mind.[8] It is probable that strong moods, caused by factors not controlled by the experimental situation, would result in assimilative effects similar to those found in this experiment. The reports of the *Os*, however, definitely give the odors as the cause of the general states they describe, and the moods reported were always in accord with the type of odors presented.

The fact that no clear evidence of intermodal affective contrast was found in this experiment did not mean that such an effect would never occur. Special circumstances might be necessary which were not provided in this experiment. Accordingly, further experimentation was planned.

EXPERIMENT II

The apparatus was the same as in the preceding experiment. The *Os* were Messrs. Oberlin (*Ob*) and Kim (*Ki*) from the preceding experiment, and 6 new *Os*, Miss J. Steinberg (*St*) and Messrs. W. A. Hunt (*Hu*), M. H. Lewis (*Le*), J. D. Frank (*Fr*), E. J. Ludvigh (*Lu*) and M. Sherif (*Sh*), all research students in the Harvard laboratory. For *Ki* and *Ob* the same colors and odors were used as in Experiment I. For the other *Os*, individual P and U odor sets were selected as in Experiment I, after one hour of preliminary judgments on odors alone. A set of 20 colors, used for all of these *Os*, was selected by *E*. The order of presentation of colors and odors was the same as in Experiment I.

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[8] P. T. Young, Studies in affective psychology, this JOURNAL, 42, 1930, 26.

The experiment had 3 parts. For *St*, *Hu*, *Le*, *Fr*, and *Lu*, the colors were presented intermingled with the U odors in the first part, then with the P odors, and finally with the U odors again. For *Sh*, *Ki*, and *Ob*, the P odors were given first, then the U odors, and finally the P odors. Each part consisted of 3 experimental hours, as before. The hours were spaced 1 week apart, with no extra interval between parts. The instructions were essentially the same as in Experiment I.

Individual effect of the odors. The average affective value of the 20 colors following each odor of a given set was calculated as in the preceding experiment. As before, the averages for all *Os* were practically the same, following the least P odors of a set, as they were following the most P odors of the same set. No traces of individual effect of the odors were found.

TABLE III

SHIFTS IN THE AFFECTIVE VALUE OF THE COLORS WITH A CHANGE IN ODOR SETS

EXPERIMENT II

O	Change of odors	1 Color av. after P odors	2 Color av. after U odors	3 Shift in color av.	4 Method X $D/\sigma_{diff.}$	5 No. colors higher after P odors	6 No. colors with no change	7 No. colors higher after U odors	8 Method Y $D/\sigma_{diff.}$
St	U-P	.56	.44	.12 a	3.58	8	8	4	1.42
	P-U	.56	.59	.03 a	1.14	4	11	5	.36
Hu	U-P	.41	.32	.09 a	.95	9	4	7	.65
	P-U	.41	.49	.98 c*	.75	6	5	9	.99
Le	U-P	.55	.47	.08 a	1.14	12	3	5	2.39
	P-U	.55	.67	.12 c	2.30	6	4	10	1.32
Fr	U-P	.25	.17	.08 a	1.31	11	4	5	2.04
	P-U	.25	.22	.03 a	.55	9	2	9	0.0
Lu	U-P	1.18	.53	.65 a	13.68	18	2	0	13.44
	P-U	1.18	.25	.93 a	16.52	20	0	0	—
Sh	P-U	.54	.86	.32 c	3.09	2	2	16	6.31
	U-P	.56	.86	.30 c	3.23	2	8	10	2.98
Ki	P-U	.92	.85	.07 a	.82	9	5	6	.99
	U-P	.87	.85	.92 a	.22	8	2	10	.64
Ob	P-U	.06	.57	.51 c	6.88	1	1	18	10.64
	U-P	.34	.57	.23 c	2.95	4	4	12	2.84
						129	65	126	

* 'a' indicates assimilative shift; 'c' indicates contrast shift.

Mass effect of the odors. The main differences between this experiment and the preceding one were the greater number of *Os* and the addition of a third part. It was hoped that this would give a better opportunity for individual differences to appear, if that were the solution of the problem, and also that the more extended experiment might offer a condition, necessary for the occurrence of contrast, that was not present in the first experiment.

The results for the 8 *Os* are summarized in Table III. The total color averages for each part are shown in columns 1 and 2. Column 3 shows the amount and direction of the average shift between each two parts. The directions in which the individual colors shifted are shown in columns 5, 6, and 7. The results obtained by applying Methods X and Y are shown in columns 4 and 8.

It will be noted that for *St*, *Hu*, *Le*, *Fr* and *Ki*, not one shift is indicated to be significant by both methods. The first shift for *St*, a small shift in the direction of assimilation, significant by Method X but not by Method Y, is statistically the most significant. However, it is no larger in amount than the second shift for *Le*, which is of doubtful significance by both methods. For *St*, *Hu*, *Le*, and *Fr*, the two shifts are in opposing directions, one in the direction of contrast and one in the direction of assimilation. For *Ki*, both shifts were in the direction of assimilation, but were too small to be considered significant. When the total number of colors becoming more P after P odors is compared with the total number of colors becoming more P with U odors, it is evident that there has been no tendency in either direction. Tossing of coins could hardly give a more nearly even division than 126 and 129.

Since 5 of the 8 *Os* show no significant shifts in either direction, and the number of colors that shifted in an assimilative direction is practically equal to the number that shifted in a contrast direction, we may conclude at once that alternate presentation of affective stimuli is not, in itself, sufficient to produce either contrast effects or assimilation effects.

The results from the other 3 *Os*, however, show definite shifts. The figures for *Lu* show two very large shifts in the direction of assimilation; the colors became more P when mingled with the P odors, and less P when presented with the U odors again. Both shifts are highly significant by the two statistical methods. For *Sh* and *Ob* both shifts were in the direction of contrast. In each case the two shifts were fairly large, the first shift being slightly larger for both *Os*. The first shifts for both are significant by both methods. The second shift for *Sh* is significant by Method X, while by Method Y the probability that it is significant is 99.9 in 100. The probability that the second shift for *Ob* is significant is 99.85 in 100 by Method X, 99.76 by Method Y.[9] It seems evident that for these two *O*s we have evidence of definite intermodal affective contrast.

Discussion. In an attempt to discover factors that might have been responsible for the different kinds of results obtained, each *O* was requested to answer a set of questions at the conclusion of the experiment. The questions were as follows:

(1) Did the odors and colors seem to come in one continuous series, in pairs, or as two separate series?

(2) Did you judge the colors and odors on the same scale?

(3) Did you notice any influence of the colors on the odors or of the odors on the colors?

(4) Did you notice if moods of pleasantness or unpleasantness were ever set up by the odors?

(5) Did the pleasantness or unpleasantness of the odors ever last through the colors?

(6) Did you ever make any judgments that were based on memory?

(7) What did you think was the purpose of the experiment?

An examination of the answers to these questions showed no one point with respect to which the two *O*s whose results showed contrast shifts differed from the rest of the *O*s; indeed, they did not agree very well with each other. Analysis of their individual answers, however, seemed to show what had been happening.

Ob had been one of the *O*s in Experiment I, and in that experiment had not given evidence of significant contrast. During that experiment he reported that the odors and colors were entirely separate, "as if there were two experiments at the same time." After the present experiment he reported that there had been a fluctuation between two series and one continuous series. This seems to indicate a gradual coalescence of the two kinds of experiences into a subjectively unitary group, thus producing the condition necessary, according the law of affective equilibrium, for the occurrence of a mass contrast effect. Such subjective grouping between experiences of different modalities, would seem to require long familiarity with the two kinds of stimuli presented in an objectively unitary group. *Sh*, the other *O*, whose results showed shifts in a contrast direction, did not have the background of familiarity with the odors and colors together that helped to produce subjective grouping in the case of *Ob*, and his answers showed that he experienced the odors and colors as forming two separate series. Shortly before starting to observe in this experiment, however, he had read Beebe-Center's paper on affective equilibrium, and had expected some such effect to occur. He was the only *O* who had definitely expected one kind of result while making observations. It is probable that this expectancy was the main factor responsible for the observed contrast effects in his case.

Thus, of the two cases of contrast, one seems to be an instance of intermodal mass contrast of the type described by the law of affective equilibrium, dependent upon the grouping together of the two kinds of experiences. In the other case, the observed contrast may have been an artifact produced by expectancy.

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[9] Garrett, *ibid.*, 134.

The answers from *Lu*, the *O* whose results showed marked assimilation, were similar in many respects to the reports of *Tu* in Experiment I. Both reported that moods created by the odors influenced their judgments of the colors. Both also reported two kinds of attitudes. One attitude could be characterized as rather passive and uncritical, with spontaneous judgments; this they correlated with a strong tendency to assimilate. The other attitude was more active, involving more critical judgments; with this, they reported, the tendency to assimilate was weaker, if not entirely lacking. As to the 5 *Os* whose results showed no significant shifts, one of them, *Ki*, had taken part in Experiment I and then had shown significant assimilation. His answers at the close of the present experiment showed that the effect of the U odor set had decreased, in his case, to the point where no definite moods were set up; with the disappearance of the moods, the tendency to assimilate also disappeared. Of the 4 other *Os*, *St* and *Fr* reported no moods and no grouping between the odors and colors. *Le* and *Hu* both reported the presence of moods created by the odors, and also reported definite attempts to prevent these moods from having any effect upon their judgments. *Le* reported no grouping, while *Hu* reported possible grouping between the odors and colors. It is worthy of note here that *Sh*, whose results showed contrast and who reported contrast, also reported that it took effort to avoid judging the colors according to moods induced by the odors. The reports of *Le*, *Hu*, and *Sh* seem to show further instances of the 'critical' attitude reported as occasional by *Tu* and *Lu*.

It would seem, from these results that there are two conditions necessary for the appearance of valid assimilation effects in the results. One is the existence of a fairly strong mood of pleasantness or unpleasantness, created by the odors. The other is the presence of a certain type of attitude which may roughly be described as 'uncritical.'

There is a strong resemblance between the 'critical' and 'uncritical' attitudes noticed in this experiment and the 'objective' and 'physiological' ('intrasubjective') attitudes described by Bullough.[10] But while Bullough has considered these attitudes to be characteristic of different types of subjects, the present writer is inclined to consider them as temporary attitudes which can both be adopted by an *O* at different times in the same experiment.

It is possible that different results would be obtained, if different modalities than those used in the present experiments were employed. It would be interesting, as a check upon the conclusions drawn from the evidence of the present experiments, to see if the tendency to assimilate would become less, if stimuli less extremely P or U than odors are used, and whether the relative frequency of cases of contrast would increase in proportion to the amount of similarity between the modalities. An experiment designed to promote subjective grouping between two modalities should demonstrate whether such grouping is necessary for the occurrence of intermodal contrast. A modification of the present technique, requiring the *O*s to make paired comparisons between stimuli of the two modalities, as well as absolute judgments of the individual stimuli, should meet this requirement.

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[10] E. Bullough, Recent work in experimental aesthetics, *Brit. J. Psychol.*, 12, 1921, 86.

Nothing is known about the relation of affective contrast to somewhat similar phenomena in other fields of judgment. Hollingworth's experiments, which led to his formulation of the concept of the 'central tendency of judgment,'[11] and more recent experiments by Truman and Wever[12] and Wever and Zener[13] have shown the dependence of absolute judgments upon the total experienced situation in other fields. It is possible that a generalization of Beebe-Center's law of affective equilibrium will apply to the method of absolute judgment in any field where such judgments can be made.

SUMMARY

In the two experiments which have been described, an attempt was made to see what changes might be produced in judgments of the affective value of stimuli of one modality, when they were presented intermingled with affective stimuli of another modality. The results may be summarized as follows:

(1) Affective contrast does not usually result when odors and colors are presented alternately in a continuous series. In such a situation, there is no single type of influence by the odors upon the judgments of the affective value of the colors that will appear in every case. In slightly more than half of the cases observed, there was no significant change in the affective value of the colors when P odors were substituted for U odors, or *vice versa*.

(2) Affective contrast, while not the usual result of intermodal affective stimulation, may sometimes occur in such a situation when special conditions are present. It has not been definitely established what these special conditions may be; there is some evidence, however, that subjective grouping by the *O* of the two kinds of experiences into a unitary group is one, and perhaps the main, factor.

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[11] H. L. Hollingworth, The inaccuracy of movement, *Arch. Psychol.*, 2, 1909, No. 13, 21-39; The central tendency of judgment, *J. Philos.*, 7, 1910, 461-469.

[12] S. R. Truman and E. G. Wever, The judgment of pitch as a function of the series, *Univ. Calif. Publ. Psychol.*, 3, 1928, 215-233.

[13] E. G. Wever and K. E. Zener, The method of absolute judgment, *Psychol. Rev.*, 35, 1928, 474-477.

(3) When a definitely P or U mood is induced by strongly affective stimuli of one modality, judgments of the affective value of stimuli of another modality, during the same period of time, tend to be influenced in the direction of the prevailing mood. The name *affective assimilation* has been used for such an effect. If the *O* reports himself as adopting a rather critical attitude in judging the stimuli, this tendency will not be apparent in his actual judgments; if he reports himself as adopting a rather uncritical attitude, the tendency will be quite noticeable in the results.

EMOTIONAL EFFECTS PRODUCED BY THE INJECTION OF ADRENALIN

By Hadley Cantril and William A. Hunt, Harvard University

In his paper on the emotional action of adrenalin, Marañon[1] stated that the injection of adrenalin produced in his *S*s only a "cold emotion," a feeling "as if afraid," "as if about to cry." True emotions, however, were aroused in a very few cases, both spontaneously and by suggestions; but he attributed these to the emotional predisposition of some *S*s. He therefore concluded that the mere injection of adrenalin, with its consequent stimulation of the sympathetic nervous system, was not sufficient to cause a genuine feeling of emotion. It seemed to the present authors that Marañon did not give sufficient emphasis to those *S*s who reported genuine emotions, and that he was not wholly successful in explaining them by predisposition. Cannon[2] and Wheeler[3] accept his results. Woodworth[4] follows Cannon. Bard[5] even goes to the extreme of not mentioning the cases involving genuine emotion. Landis[6] is more fair in his treatment, and mentions the need for further work on this point. Consequently, the present writers determined to test the emotional results of adrenalin on a group of normal observers, checking carefully any emotions that might appear.[7] It seemed desirable to approach the matter in a purely psychological way, rather than to use the combined clinical, medical, and psychological approach of Marañon.

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[1] G. Marañon, Contribution à l'étude de l'action émotive de l'adrenaline, *Rev. fr. d'endocrinol.*, 2, 1924, 301.

[2] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, 1929, 356 f.

[3] E. B. Newman, F. T. Perkins, and R. H. Wheeler, Cannon's theory of emotion, *Psychol. Rev.*, 37, 1930, 305-326.

[4] R. S. Woodworth, *Psychology*, revised ed., 1929, 308.

[5] P. Bard, The neuro-humoral basis of emotional reactions, *Foundations of Experimental Psychology*, 1929, 480 f.

[6] C. Landis, The expressions of emotion, *ibid.*, 511.

[7] The authors were in no way motivated by a desire to prove or disprove the James-Lange theory of the emotions. Their interest lay wholly in the general implications of the work, and not in its application to any specific theory. Indeed, in view of James' exceedingly ambiguous article in 1894 (cf. The physical basis of emotion, *Psychol. Rev.*, 1, 1894, 516), the final formulation of his theory seems so vague and so open to personal interpretation that it appears almost useless for anything but the purpose of controversy.

There was one point in particular on which the work might shed light. This is the supposed relation of pleasantness and unpleasantness to the antagonistic action of the cranio-sacral and sympathetic divisions of the autonomic nervous system. F. H. Allport[8] has based his theory of emotion upon this hypothetical relation. He states: "The cranio-sacral division of the autonomic, supplemented under certain conditions by the cerebrospinal system, innervates those responses whose return afferent impulses are associated with the conscious quality of pleasantness. The sympathetic division produces visceral responses which are represented in consciousness as unpleasantness."[9] In a recent popular presentation, F. H. Lund even goes so far as to call the sympathetic system the "tension nerves" and the cranio-sacral the "pleasure nerves."[10] As Hulsey Cason points out, this relationship still remains to be shown in spite of its widespread acceptance.[11] The feeling states produced by sympathetic innervation through the injection of adrenalin might have an important bearing on the question.

TECHNIQUE

The general technique involved the intramuscular injection of 1.5 c.c. of adrenalin chloride. The standard Parke-Davis undiluted solution (No. 88) was used. Previous experimentation had shown that a dose of this size would produce a very definite reaction and still be harmless to the patient. As the interest was primarily in the psychological reaction, the only objective check used was the pulse rate. This was taken at frequent intervals. Previous work had shown this to be reliable, as the rate always suffers a slight drop at the onset of the syndrome. A secondary check is the noticeable increase in the strength of the pulse.

The *Os* used were Messrs. E. Land (*L*) and R. Proskauer (*P*), Harvard undergraduates; A. Wheelwright (*W*), graduate student in the Department of Physics; C. BoDine (*B*), Simeon Cantril (*SC*), and J. Goodman (*G*), medical students; S. Rosenzweig (*R*) of the Psychological Clinic; J. Logan (*JL*), a law student, and Mrs. E. Hunt (*EH*). In the first two-thirds of the experiment, the authors also acted as *Os* and their results are included under *C* (Cantril) and *H* (Hunt).

The experiment itself fell into three sections. In the first section, *W*, *L*, *P*, *B*, *G*, *C*, and *H* were all given a single injection of adrenalin. *O* was first asked if he were emotionally predisposed in any way at the time. (If so, he was dismissed until a later appointment). He was then required to rest for 5 min. After this his pulse was taken and the adrenalin injected. The *O* was then seated at a table with pencil and paper and asked to write his introspections. His pulse was taken every few minutes. In two cases, two *Os* were seated in the same room, separated only by a screen.

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[8] F. H. Allport, *Social Psychology*, 1924, 89 f.

[9] *Ibid.*, 90.

[10] F. H. Lund, *Emotions of Men*, 1930, 115.

[11] H. Cason, Pleasant and unpleasant feelings, *Psychol. Rev.*, 37, 1930, 230.

In the second section, *C*, *H*, *L*, and new *Os*, *JL* and *EH*, were each given two injections. One of these injections, unknown to the *O*, consisted merely of a normal salt solution which afforded a check on the validity of the syndrome. The general procedure was the same as above.

In the final section, the experiment was repeated under more rigid conditions to check the previous data. The experimenters themselves did not observe. *JL*, *EH*, *L*, and two new *Os*, *SC* and *R*, were used. Each was given two injections of adrenalin. At the beginning of each sitting the *Os* were asked the three following questions:

> Do you at present feel any disposition toward any particular emotional state?
> Has anything (either physically or mentally) within the last few days or quite recently happened to disturb your normal state?
> Can you recall any dreams that you have had in the past few nights?

If the answers to the first two questions were negative, and the third failed to reveal any disturbing dream content, the experiment proceeded. Otherwise, the *O* was dismissed until a later date. After a 5-min. period of rest, *O's* pulse was taken and the adrenalin injected. His pulse was then taken again and he was handed the following instructions:

> You have been given an injection of adrenalin. You are now asked to report orally to the experimenter and *as fully as possible* any physical or mental symptoms which develop. Do not omit anything, no matter how irrelevant it may seem to you. Try not to become impatient and try to keep your mind free from any material not called up by the actual situation.

The instructions were then removed and *O* was seated in an easy chair before a large grey screen. (In one case, that of *R*, the screen was unavoidably absent.) The *E* sat at a table behind the screen ready to take down the observations. No one else was in the room. *O's* pulse was taken every 5 min. and also immediately upon the report of the symptoms of the syndrome.

RESULTS

Physical symptoms. The physical symptoms which followed the injection of adrenalin were similar in the three sections of the experiment. The physical syndrome always consisted of a muscular tremor localized particularly in the legs and hands, a tremor in the voice, increased respiration, immediate slight decrease and subsequent rise in the pulse rate, and an increase in the strength of the heart beat. Several *Os* felt a tenseness in the viscera and others reported a tenseness localized in the chest.

Subsequent to the injection of the salt solution in the second part of the experiment, no *Os* reported any noticeable physical effects.

The following are a few typical introspections concerning the physical effects of the injection of adrenalin.

"General muscular tremor and lack of faith in my muscular control. Breathing is more conscious and seems even more necessary than usual." (*C*)

"Definitely conscious of strong heart beat. Feel a tremor in all my muscles, particularly hands and legs. Tremor in voice." (*B*)

"Sudden release of muscular control. A tense feeling in chest and breath feels short. Heart pounding regularly and monotonously—like 'Bolero' (of Ravel)." (*JL*)

It should be mentioned that all the *O*s experienced a prolonged muscular tremor in the arms and legs. This effect lasted in some cases as long as two hours and is worthy of further physiological treatment in connection with the specific action of adrenalin.

Subjective aspects. The subjective aspects will be divided into three parts, corresponding to the three sections of the procedure. (1) In the first part *P*, *W*, and *G* reported only the physical concomitants of the syndrome. The following introspections illustrate this type of report.

"Trembling, particularly in hands. Voice shaky. Legs trembling. No psychic effects." (*W*)

"Trembling definitely in hands and arms. Resulting in difficulty in writing. Tremble in voice. Feel no more pleasant than before and unpleasant only as much as trembling gives me a feeling of unusualness. No emotions at all. Calm and collected. Seem to have all the externals of excitement but no feeling of excitement." (*G*)

B, *C*, and *H* reported a 'cold' emotion.

"Feel as though I had had a big scare, not fright but like a reaction after tremendous fright. Don't feel unpleasant. If there is any affective state it is somewhat pleasant. Certain bodily restlessness but no worries, cares, or mental anxieties." (*B*)

"Feel as I do either before an event in which I am to take an active part—such as a public appearance or athletic competition or before some great joy which I am anticipating. Likewise, it is much the same as the feeling I have after being scared for an instant by a danger which passes almost as soon as it appears. General restlessness and lack of faith in my muscular response—and yet I want to do something to relieve this uneasiness. . . . . An undirected uneasiness without cause and purpose and hence I am annoyed. No positive pleasant or unpleasant emotions but the desire to have one of some kind to relieve my restlessness." (*C*)

"Bodily feeling of extreme nervousness. The bodily feeling almost swamps the psychic state, but the nervousness is wholly a matter of physical symptoms. I feel nervous, but I am not nervous. The reason seems lacking. I lack an object and the intellectual state is not fulfilled. To sum it up, I feel vaguely nervous, excited, but any definite emotion is lacking, and the excitement is a detached, bodily affair." (*H*)

L seemed to get something resembling a genuine emotion.

"Excitement (have felt like this in large class when I think I have a good idea, when raising my hand). No elation, but a background of elation. Feel light, as I used to in a burst of creative activity (writing). A pleasant state, possibly by association with such activity. Not unfamiliar. Used to attempt to bring on such states deliberately in adolescence. . . . . During times of tremendous intellectual experiences in the past, times when my vision seemed peculiarly clear and far reaching, I have experienced similar reactions, but not even then so strongly. A tremendous illusion of power, vitality." (*L*)

(2) In the second section of the experiment no *O*s reported any subjective symptoms subsequent to the injection of the salt solution.

Following the injection of adrenalin, *JL* noted marked physical changes but no psychic accompaniment.

"Muscles relaxed—sensation of no control. Throbbing of pulse through chest and back of head. Physical sensation as violent as any emotional reaction but no emotions present. Rather a distinct lack of them." (*JL*)

A 'cold' emotion was reported by *L* and *C*.

"Whole reaction seems localized in chest. Feel like breathing deeply to shake it off. Heavier and more and more like a feeling of great apprehension, except entirely localized. More accurately, chest feels as it does during great apprehension, but this feeling so localized that I can introspect detachedly as if I were considering a slightly injured finger. This reaction was dominated by a lack of confidence, a desire to withdraw, in short all the elements of fear except mental pain." (*L*)

"Feeling which is neither pleasant nor unpleasant. General excitement and I should like an object for it. Some annoyance at being unable to find relief from the excitement by some action." (*C*)

In the introspections of *H* and *EH* there is a genuine emotion noticed.

"The sound of my own voice trembling acts as context for fear. Feel extremely worried. Not exactly anxiety but a foreboding which is unpleasant. Whenever, without my mind on the symptoms, I do something like putting my hand to my face, the trembling of the hand gives me—on its perception—an extreme anxiety—not fear—but foreboding. No definite object but nevertheless an emotion." (*H*)

"I seem oppressed with a vague fear of something—feeling much the same as when I've lain awake all night, frightened that Bill might die. In spite of knowing the cause of his illness the fear was not specific and neither is this. I am oppressed with a nameless fear. I want to get away from it just as the night of Bill's illness I wanted to relax and sleep and tried to rationalize my fears. But that was impossible then and it is impossible now. It is decidedly unpleasant." (*EH*)

(3) In the third part the mere physical symptoms of the syndrome were reported once by *EH* and *SC* and twice by *R*.

Reports of 'cold' emotion were given once by *SC*, *L*, and *EH*, and twice by *JL*.

"A feeling in my abdomen as if I were going to get into an argument. Feeling as when somebody tells me they don't believe in vaccination." (*SC*)

"Felt a bit pleasant a moment ago. This succeeded by a disturbed feeling now, neither pleasant nor unpleasant. Decided predisposition to an emotion. Only a constant conscious attempt to keep calm prevented an emotion. It was an effort to keep calm." (*L*)

"Tenseness. The feeling as though something were going to happen that I expected to be disagreeable. An overwhelming desire to have something happen through which I might relieve the suspense." (*EH*)

"Feeling of something exciting going to happen. A desire for action. Throughout reaction there is a definite feeling of unrest which cannot be described as pleasant although there is nothing clearly unpleasant—might be summed up as uneasiness. I feel that during the whole reaction I should be readily subject to any kind of emotional suggestion." (*JL*)

"I feel very carefree and pleasant. Pleasant state of being continues although heart beat and trembling increased. Now a feeling of uneasiness and indifference. Feel as if I could either laugh or cry. As you do when drinks are being mixed and you are afraid you won't get one but hope you will. I want to do something but don't know what it is." (*JL*)

L reported a genuine and sudden emotion.

"Extreme fear was present, but no content for it at the time. Probably an unconscious reason but at the time nothing but fear. The strongest reaction yet. I found myself shaking, chest trembling, building up rapidly in intensity, whereupon I abruptly recognized that I was intensely afraid." (*L*)

It will be seen that while the majority of reports show no emotion, or at best a 'cold' one, there are cases of genuine emotions aroused which seem difficult to explain away as due to predisposition. The important thing to be noticed is that the bodily symptoms may occasionally satisfy some of the *O*s, while at other times they demand an intellectual content or reason before they will report an emotion. Some of the *O*s never appear able to experience an emotion without this content. In those *O*s not experiencing an emotion, there frequently appears a need for catharsis, a need to throw off the tension by completing the reaction. It is a desire to get the matter 'off their chests' by having an emotion and releasing the tension aroused. It is also apparent that there are no strict affective correlates of the adrenalin syndrome. The *O* may find it pleasant, unpleasant, or affectively indifferent. In one case we even have an exceedingly pleasant state resembling elation.

These results are summed up in the following Table I.

TABLE I

SUMMARY OF THE RESULTS OF THE EXPERIMENT

Classification of Reports	No. Reports
Total injections	22
Physical symptoms	22
Pleasant emotions	1
Unpleasant emotions	3
'Cold' emotions	10
No emotions	8
Unpleasant affect	3
Pleasant affect	3
Indifferent	5
No mention of affect	11
Need for catharsis	6

CONCLUSIONS

On the basis of the above results we may conclude that in general the injection of adrenalin is not sufficient to produce an emotion. A 'cold' emotion may be aroused, but the lack of any object or reason for the emotion usually deprives it of its genuineness. In a few cases, however, the injection of adrenalin with its subsequent bodily symptoms is adequate to produce a genuine emotion; the mere bodily concomitants are sufficient for an emotional state without the presence of any object or reason.[12]

The experimental findings of the present investigation agree in general with those of Marañon but the present authors differ from him in their interpretation of the genuine emotions aroused in their experiment.

While the results of the present experiment may not justify the formulation of a general theory of emotions, the writers wish to venture a few theoretical implications. It seems to them that an analysis of the conscious emotional state would reveal its dependence upon the following possible factors.

(*1*) *A primary autonomic reaction* centered in the autonomic nervous system and involving changes in the vasomotor system, the respiratory system, the viscera, and certain peripheral changes such as sweating and muscular tremor.

(*2*) *Secondary behavioral reaction patterns*, both instinctive and learned, dependent upon the cerebro-spinal nervous system. This includes such reactions as flight, attack, facial expression, and vocalization.

(*3*) *An intellectual content.* By this we mean the presence in consciousness of some object or situation to which the emotion is directed and around which the emotion is intellectually organized. This involves the active participation of the higher logical processes, probably centered in the cerebral cortex.

The *usual* emotional state seems to involve a combination of (*1*) and (*3*). However, as the only criterion we have of a conscious emotional state is the *O*'s acceptance of it as such, the writers believe, on the basis of their results, that (*1*) is the only *sine qua non* for an emotion, for it may in some cases suffice itself to produce the emotional state. Nevertheless, such cases are rare. The status of (*2*) is at present unsettled, but this factor is probably of secondary importance.

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[12] These emotions resemble that described by Carl Sandburg as follows:

"We are afraid. What are we afraid of?
We are afraid of what we are afraid of.
We are afraid of this, that, these, those, them."

The tendency of Marañon and others to explain the genuine emotions arising from the adrenalin syndrome by referring to predisposition seems unjustified to the present authors. True, in some cases, in addition to the report of the emotion, the *O* reported an associative content, viz. some reference to previous experiences or memories. But like associative content was also present in the report of 'cold' emotions, so that the difference cannot be explained on this basis. It seems to the authors that the differences of report are due to some critical attitude assumed by the *O*. In other words the situation must contain certain logical relationships in order that most *O*s experience a genuine emotion. In some cases, however, such a relationship is unnecessary. On the physiological level this may be due to differences in the strength of cerebral inhibition. These differences may be present not only between *O*s but within the same *O* at different times.

No doubt the physiological reaction involved in an emotion includes the basis for an affective state, but the report of pleasantness and unpleasantness seems to depend upon a special affective judgment which is not necessarily included in the report of the simple emotional consciousness.

The lack of any constant affective correlates of the adrenalin syndrome indicates that the theories which base unpleasantness upon the activation of the sympathetic division of the autonomic nervous system are palpably unfounded.

Finally, the need for cartharsis reported by some *O*s would seem to show that the emotional reaction contains within itself a definite drive to completion through expression. This resembles the tendency to closure exhibited by *Gestalten*.

LOCALIZATION OF BRIGHT AND DULL PRESSURE*

By WILLIAM A. HUNT, Harvard University

In some unpublished work mentioned by Hoisington,[1] Horiguchi reports the localization of the pressures established by Nafe in his study of the affective processes.[2] Bright pressure is localized in the general region of the shoulders and neck, and dull pressure in the abdominal area. In two previous investigations of bright and dull pressure, the present author found his *Os* substantiating this localization.[3] Its manifest importance made him desire a more careful investigation under conditions as well controlled as possible. As the localizations reported by his *Os* had referred to general bodily areas and had varied with different *Os*, he decided to concentrate on what seemed to be the most important aspect of the matter, namely, that bright pressure was always localized *above* dull pressure.

In order to minimize the influence of any bias on the part of the *Os*, it seemed best to use the method of correlation. Thus the *Os* were first to report on the localization of the bright or dull pressures aroused by a series of stimuli which could then be assigned values signifying the bodily locus of the pressures involved. At a later sitting affective values were to be ascertained for the same series of stimuli. Correlation between the two series would then reveal whether or not pleasant stimuli arouse pressures in a bodily locus in constant relation to that of the pressures aroused by unpleasant stimuli. The correlation of bright and dull pressure with pleasantness and unpleasantness, which is assumed by this procedure, is justified by the author's previous work.[4]

At the same time it seemed promising to investigate the presence of any other internal bodily pressures. For this purpose a series

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[1] L. B. Hoisington, Pleasantness and unpleasantness as modes of bodily experience, *Wittenberg Symposium on the Feelings and Emotions*, 1928, 237-240.

[2] J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544.

[3] W. A. Hunt, The relation of bright and dull pressure to affectivity, this JOURNAL, 43, 1931, 87-92; The pressure correlate of emotion, *ibid.*, 43, 1931, 600-605.

[4] *Opp. citt.*

was inserted in which the *O*s were merely asked to localize any bodily pressures present, no mention being made of either bright pressure or dull pressure.[5]

PROCEDURE

Twenty colored papers of varied hue and brightness were used as stimuli. They were exposed through an aperture, 5 in. square, in a large screen, 6 ft. high by 4 ft. wide, with a 2-ft. wing on each side. The screen was painted a cream gray. Its size enabled *E* to sit at a table behind it, unseen by *O*. The large gray field aided *O* in concentration by shutting out distracting stimuli. *O* sat in a big easy chair, which permitted extreme relaxation and removed many of the confusing bodily tensions accompanying normal posture. His eyes were about 4 ft. away from, and on a level with, the aperture in the screen. The position of both chair and screen was kept constant. Lighting was artificial and constant.

In order to prevent the bright and dull pressure judgments from influencing the judgments of other internal bodily pressures, the latter judgments were taken first. The *O* was told to sit in a comfortable, relaxed position, and was then given the following instruction:

> I shall present various colored stimuli. You are to attend to the general experience involved in observing them, and to notice whether or not this experience includes any internal pressures. If such internal pressures are present, please localize them on the chart and report the number to the experimenter.

The instructions were then removed and *O* was given a chart consisting of an outline of the human torso. A line running down through the middle of the body was divided into equal sections by the numbers 1 to 8. 1 was at the top in the region of the upper neck and head, and 8 at the bottom in the region of the lower abdomen. The series of colors was then presented three times, in constant order, with about a 10 sec. interval between successive colors. There was a brief rest at the end of each series. The localizations were recorded and *O* was dismissed for two weeks.

After this time had elapsed, he was called in again. This time the instructions were:

> I shall present various colored stimuli, and you are to judge whether they are pleasant, unpleasant, or indifferent.

The same colors and procedure were used, and the affective judgments were recorded.

Two weeks later at a final session, *O* was given these instructions:

> I shall present various colored stimuli. You are to attend to the general experience involved in observing them, and to notice whether or not this experience includes any bright or dull pressures. If such bright or dull pressures are present, please localize them on the chart and report the number to the experimenter.
>
> 'Bright pressure' resembles tactual bright pressure, or a touch quality lying between bright pressure and tickle. It is a diffuse bodily feeling with expansive qualities. In addition to its decided brightness, it may be described as buoyant, lively, elusive, and welling. It is like expansiveness of the body.
>
> 'Dull pressure' has been described as dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended and has qualities of constriction. It is dull, inert, heavy, sinking, leaden, condensed, and like bodily contraction.

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[5] The writer wishes to express his indebtedness to Dr. J. G. Beebe-Center of the Harvard Psychological Laboratory for his constant assistance and encouragement.

Thus the experiment consisted of three series dispersed over a period of six weeks. A series of localizations of internal pressures was followed by a series of affective judgments, and this by a series in which *bright* and *dull* pressures were localized. Seven *O*s were used. Four of these, Mr. H. Cantril (*C*), Mr. E. Land (*L*), Mrs. E. Hunt (*H*) and Mrs. H. Taylor (*T*), had previously observed in the writer's experiments on bright and dull pressures. The remaining three, Mr. J. Volkmann (*V*), Dr. O. Trimble (*Tr*), and Mr. H. Odbert (*Od*), were new to the problem. Originally, one other naïve *O* had been included, but it was necessary to drop him due to his failure to keep the experimental appointments.

The treatment of results was the same for both pressure series. The localizations had been reported through numbers corresponding to different sections of the torso as marked on the chart. These numerical values were then averaged for each stimulus, and the result was taken to indicate the average locus of the pressures aroused by that stimulus. Thus the values for each series fell into a distribution representing the dispersion of these average localizations over the torso, from top to bottom. Where no localization was reported, the judgment was dropped out and not allowed to affect the average for that stimulus. In the few cases where no localizations were reported for any of the three presentations of a stimulus in a series, these stimuli were merely discarded from the distribution, and in computing correlations the corresponding number of the other series was discarded also.

The affective judgments were also assigned numerical symbols: a value of +1 was given a judgment of pleasant, 0 to one of indifferent, and −1 to one of unpleasant, and the sum of these values for each color was considered to represent the degree to which that color elicited pleasantness rather than unpleasantness. The series was then arranged in a distribution with the positive, pleasant values at the top. Correlation of this series with each of the pressure series would then give a coefficient representing the degree to which pleasantness was accompanied by pressures *above* those accompanying unpleasantness.

RESULTS

In the first series, that concerning *any* internal bodily pressures, all the *O*s reported localized pressures except *Od*. He gave occasional judgments but claimed they were extremely difficult to make and hence were exceedingly doubtful. The 4 experienced *O*s reported that the pressures observed were not bright and dull pressure as such, but were ordinary localizable pressure sensations in the body cavity. Correlation of this series with the affective series gave the following results:

	Observer					
	H	*C*	*T*	*L*	*Tr*	*V*
r	.90	.91	.75	.89	.85	.66
P.E.	.04	.03	.07	.03	.04	.08

In the bright and dull pressure series, all the *Os* except *Od* once more reported localization of the pressures. The correlations with the affective series were:

	Observer					
	H	*C*	*T*	*L*	*Tr*	*V*
r	.93	.94	.70	.97	.85	.83
P.E.	.03	.02	.08	.01	.04	.05

It will be noted that there is a slight tendency, of doubtful statistical significance, for these values of *r* to be higher than those of the first group.

Correlation of the two pressure series with each other yielded the following coefficients:

	Observer					
	H	*C*	*T*	*L*	*Tr*	*V*
r	.90	.91	.93	.87	.83	.73
P.E.	.04	.03	.02	.04	.05	.07

Od was given another trial in order to see if further practice would overcome his difficulty. It did, but, because of the doubtful influence of training, his results are not considered with the main group. In the second trial his judgments came with greater ease and gave *r* of 0.86, 0.89, and 0.93, which are in keeping with the other results.

These results concern only the relative localization of the pressures accompanying hedonic tone. The data, however, also yield information of the absolute localization of these pressures. Table I gives the frequency distribution of the localizations according to *O*, series, place of localization, and the supposed affective value of the stimulus. It should be remembered, however, that in an endeavor to rule out any bias, the *Os* were asked in the pressure series only to report the number corresponding to the bodily locus. No mention was made of the affective

TABLE I

Frequency Distribution of Pressure Localizations

Capital letter indicates *O*. P = pleasant stimuli. U = unpleasant stimuli.
a = simple pressure localizations. b = bright and dull pressure localizations.

Number of Cases

Bodily Locus		*H*				*C*				*I*				*L*				*Tr*				*V*			
		P		U		P		U		P		U		P		U		P		U		P		U	
		a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b
Neck	1									2	3	1						1	4						
	2										4		3					5	4	2	2	12	2	4	
Chest	3	1	9			11	19			5	3	1	3	4	6	1		3	4	5	7		19		
	4	17	9		1	8	7	1			1	5	6	2	2	5	6	4	3	4	1	7	9	2	2
	5				1	2		6	2		1	1	3	2		22	21	3					2	2	3
Abdomen	6					4		11	10			2	4				1		2	4	2	7		5	4
	7			7	2			9	9	2		11	5						1	6	9		1		4
	8				4				6									1		10	12				

value of the stimuli. This last was determined in the special affective series coming midway between the two pressure series. Since the affective values of the stimuli were established in a separate sitting, it is only right to mention the fact that affective variability working between the series may have caused some error in the table. In an endeavor to control this variability the frequency distributions contain only the localizations of those stimuli whose affective value was constant or included one judgment of indifferent. Stimuli reported as both pleasant and unpleasant in the affective series were discarded, as were those whose reported value contained two or more judgments of indifference. It was felt that to include these data would allow too great an error due to variability.

The frequency distribution, while showing clearly the tendency of pleasantness to produce pressures localized above those produced by unpleasantness, nevertheless shows considerable dispersion within each category and some overlapping between the categories. The failure of the *Os* to report a constant particular locus for the pressures accompanying either affective tone may be due to the known difficulty of localizing organic sensations. None of the *Os* found the task easy. On the other hand, due to the fact that a few *Os* occasionally reported that pressures with a dominant locus in one zone would extend over into neighboring zones, it may be that we are dealing with a general reaction whose specific elements may vary at different times. The constancy of the above-below relationship between the pressures seems to demand some such unifying factor. Even where the dispersion within each category is great, the relative position of the pressure components of one category to those of the other remains well marked.

All the *Os* reported that the two different pressure series were made under distinctly different attitudes of introspection and that the simple pressures were different from bright and dull pressure. Bright-and-dull pressure seemed to be a larger, more inclusive category, with the stress less on the actual specific pressure components and more on the general reaction involved. Four *Os* spontaneously suggested that while bright-and-dull pressure seemed to be different from the simple bodily pressures reported, these last might well be specific components of a general complex of sensations corresponding to bright and dull pressure.

CONCLUSIONS

While our results tend to confirm Horiguchi's localization of bright pressure in the general region of the shoulders and neck, and dull pressure in the abdominal area, they stress the importance of the central areas of the chest for bright pressure and the occurrence of overlapping in the lower chest regions. The localizations are within *general* areas, and specific localizations show marked variations, both in the case of single individuals and within the group. On the other hand, our data show that the relationship between bright pressures as a whole and dull pressures as a whole is remarkably constant, and we may definitely conclude that bright pressure is localized *above* dull pressure. We may also conclude that pleasantness and unpleasantness are accompanied by simple bodily pressures which maintain the same relative positions as bright and dull pressure.

It seems too early to attempt any definite explanation of these results. A suggestion, however, of the direction in which such an explanation may lie is afforded by the following passage in an article by Zoneff and Meumann. "Es sei noch bemerkt, dass auch die gesammte Haltung des Brustkorbes bei Lustgefühlen eine andere ist als bei Unlust. In Zuständen, die als 'Lust' bezeichnet werden, hebt die Versuchsperson merklich den Brustkorb, was sich in dem Steigen des Niveaus der gesammten Curve auf der Kymographiontrommel andeutet (umgekehrt bei Unlust). Es ist die Erscheinung, die man in dichterischer Sprache als 'geschwellte Brust' bezeichnet."[6]

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[6] P. Zoneff and E. Meumann, Über Begleiterscheinungen psychischer Vorgänge in Athem und Puls, *Philos. Stud.*, 18, 1901, 60.

AN EXPERIMENTAL STUDY OF 'SEARCH'

By HOWARD L. KINGSLEY, Boston University

In a former investigation a comparative study was made of perception, thinking, and that scrutinizing, exploratory type of performance called 'search.'[1] The problem was to discover the natural relations which these functions sustain to each other. The search-problem there called for the exploration of a picture to discover some specified detail. In the present study the purpose has been to secure a more complete description of this type of function by making further observations upon the processes of exploration and discovery under conditions similar to those of the previous experiment.

Method and procedure. A series of 12 pictures, each mounted on cardboard 9 x 12 in., was presented to the *Os* by means of an exposure apparatus under the following instructions:

> "At the 'ready' signal you are to fixate the center of the screen. At the 'now' signal a picture will be presented and at the same time you will be told to find some object in the picture. When you have located this object press the button and immediately report your total experience of the exposure period."

The object named by *E* was always an integral part of the picture but one not likely to be noticed in a casual glance. The *Os* were *B*, *R*, *Br*, *M*, and *T*. *B* and *R* were members of the psychology staff of Goucher College and the others were students majoring in psychology. Sixty observations were made.[2]

RESULTS

Immediately following the withdrawal of the shutter there was a hurried survey of the picture which gave *O* some notion of the locale of the search. Then came *E*'s prescribing statement, *e.g.* "Find the tablespoon," "Find the rabbit," the comprehension of which gave definite form to the search-task and thus determined the course and objective of the performance.

(*1*) *Visual exploration.* The task set up by the conditions impelled *O* to explore the picture visually until the designated object

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*Accepted for publication December 15, 1930.

[1] H. L. Kingsley, Search: A function intermediate between perception and thinking, *Psychol. Monog.*, 35, 1926, (no. 163), 16-55.

[2] The writer acknowledges his indebtedness to Dr. Ethel Bowman and to Misses Elizabeth Rutherford, Theresa Berney, Charlotte Towson, and Anna Ruth Meeks. The experiments were done in the psychological laboratory of Goucher College.

(hereafter called X) was discovered. The search was characterized by "shifting of the eyes," "scanning of various objects" in the picture, numerous "shifts of fixation" and "glancing about" from one part of the picture to another.

Thus while engaged in this exploratory quest all *O*s experienced a succession, usually rapid, of visual sensory patterns. The number and character of these experiences varied with the *O*s and with the nature of the picture. For example, *B*'s search took the form of an excursion around the picture noting one object after another and rejecting each in turn until X was apprehended. *R*, as a rule, did not check off the various objects in this manner. Discrete perceptions were less numerous in her experience and a correspondingly larger place was filled by visual imagery. Some pictures, moreover, presented a greater wealth of attention-commanding details than others. Central and conspicuous figures, brightly colored objects and novel features naturally touched off perceptions.

Fixation at first was usually on the center of the picture. From the center the eye-movements were most frequently to the upper left corner and from there across to the right side, then down to the bottom. Further than this the course of the fixations was irregular and unordered except where *O* made use of clues considering where the object was likely to be found. "Exploration was haphazard until the ship (X) was found" (*Br*). "The discovery was due to chance movement in the right direction" (*R*).

The search-task which initiated the exploratory performance was sustained by pressures and strains or verbal kinaesthesis. "Pressure in my head made me want to go on. There was a lack of satisfaction in not having found X, carried by these pressures, which urged me on" (*T*). "Verbal kinaesthesis, 'Find the bread-knife, find the bread-knife' " (*Br*). "Repeated the question to myself" (*M*).

The meaning that search was in progress was carried by strain qualities arising from muscular tension throughout the upper part of the body, especially the kinaesthesis of eye-movements and pressures due to "narrowing the eyes." "There was not much feeling of search this time. The feel of muscular contraction in the head and neck was not so pronounced as in the more difficult cases of search. In more difficult search the muscular tension is greater" (*Br*).

Visual images of X frequently appeared during the search and sometimes they materially influenced its course and outcome; though their appearance was less frequent for some *O*s than for others.

The performance was enlivened by varied affective tones and colored by unanalyzed attitudes of confidence or uneasiness as to the outcome.

Elaborative thinking sometimes obviated piecemeal examination of the picture and frequently abbreviated the process. The *Os* made use of suggestions derived from former experiences and clues discovered in the pictures. "I reasoned. I did not just let my eyes wander around. I looked for the table or furniture on which the ship model [X] would be located and so avoided aimless eye-wandering" (*M*).

(*2*) *Discovery and identification of the object.* Sometimes *O*'s idea of X prior to discovery did not coincide with the object as pictured. This lack of agreement frequently, though not always, hindered discovery and identification. *R* had difficulty in locating a bread-knife that was pictured as thrust through a loaf of bread so that only the handle and a small part of the blade were visible because she looked for a bread-knife whose entire blade was showing. Sometimes, in spite of such disparity, *O* realized at once, upon seeing X, that it was what she was to find. This usually was the case where *O* had experienced the object under various conditions or in different forms. But there were times when *O*, upon seeing X, decided that that, rather than what she had imagined, was what *E* meant. When asked to find "the miniature sailing vessel," *B* understood *E* to say, "miniature of a sailing vessel," and that meant for her a small picture of a sailing vessel. In her report she said, "Then a group of grays stood out. Verbal kinaesthesis 'Sailing vessel'; oh yes, miniature sailing vessel, not miniature of a sailing vessel."

When the perceived object did not match *O*'s idea of X and such reconciliation was not made, the object was rejected and the search continued. *M* reported that when looking for a turkey she tried to make chickens look like a turkey but this attempt proved futile so her eyes moved on.

The discovery of X was commonly announced by some such verbal response as "there," or "that's it." "Then, in fragmentary verbal kinaesthesis, 'Yes, that must be it.' Then the key was pressed" (*B*). The qualities of the concluding perceptual experience were closely integrated and highly focal. "Then I saw the bird. It stood out in the foreground. Everything else receded to the background" (*R*). When X was clearly and definitely identified there usually came a feeling of satisfaction and a relaxation from the tension of the search. "There was a pleasant feeling, a realization of something accomplished and that I had reached my goal" (*T*).

(8) *On overlooking familiar objects.* Titchener found the motor set and the visual image to be the essential factors in the discovery of a familiar object for which one is looking.[3] It appears that while there was the discharge of a verbal-motor set in our observations the discovery of the object depended not upon the type of motor set but upon the perceptual set. When the *Os* gazed at X and failed to apprehend it as the object they were to find, it was because of inadequate preparation for such apprehension. While such inadequate preparation occurred in cases where there was no visual image of X it often appeared in the form of an incorrect expectation carried by a visual image that did not 'fit.'

In his note on overlooking familar objects Ruckmick states that failure to see a familiar object before him and for which he is looking is due usually to the "unsuitable image." He adds, "Now the chief feature about the mental image that makes it inadequate seems to be its size—it is too small."[4] In our experiments the visual image differed from the sight of the object in a variety of ways. It changed during the search and differed from X in size, color, and form. But the fact that the image was not a replica of the object did not always cause the latter to be overlooked or delay its identification. When looking for a turkey where the head only appeared in the picture, *R* had a visual image of an entire turkey and was surprised to find only the head. There was no hesitation, however, in recognizing the turkey-head as the thing she was to find. Where *O* had experienced the object in many situations it was recognized readily even if the visual image did not 'fit.' In some accounts the image was described as 'generalized.' It seemed to mean the object regardless of particular mode, shape, size, or position. The important thing was the meaning conveyed by the image. If *O* expected something different from the X-picture she was likely to encounter difficulty in bringing her search to an early and successful conclusion.

CONCLUSIONS

The search-performances studied in these experiments were characterized by exploratory eye-movements, rapid shifts of attention, and corresponding changes in visual patterns. The task was sustained mainly by verbal kinaesthesis, and pressures and strains in the head. The search-meaning was carried by strains from the muscles of the eyes, head, and neck. Visual images of X contributed to the comprehension of the question and the anticipation of X. Where the meaning carried by the image agreed with the meaning of the perception, the observer identified X readily even when the image did not resemble the perceptual pattern. Disagreement between *O*'s idea of X and the picture of X did not hinder recognition when O's experience with the object was extensive. The search was sometimes unsuccessful because O's idea of X was inadequate. The search-performace was rich in affective tones and mental postures. It was facilitated by the use of suggestions and clues. Its termination came with the discovery of X in the form of a highly integrated and focalized perception, usually announced by a response in verbal kinaesthetic terms and accompanied by relaxation and satisfaction.

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[3] E. B. Titchener, The overlooking of familiar objects, this JOURNAL, 35, 1924, 304-305.

[4] C. A. Ruckmick, On overlooking familiar objects, *ibid.*, 37, 1926, 631.

SOME SEX DIFFERENCES IN FACTUAL MEMORY

By Alfred G. Dietze, University of Pittsburgh

Sex differences in memory have been studied by numerous workers with conflicting results. The findings of many lead them to assert the superiority of girls and women at all ages and with all degrees of training. Among these are to be mentioned especially Burt and Moore, Gates, Pyle, Schuyten, Shaw, and Whipple.[1] In the work of Lobsein, Lyon, Mulhall, Netschajeff, and Schramm the results cannot be interpreted as definitely in favor of the feminine sex.[2] Finally, Aall and Vos, both of whom tested for logical memory, report male superiority.[3] The question is difficult to settle decisively because many factors complicate the selection of groups and cast doubt upon their comparability. In view of the conflicting findings and of the difficulties in the way of investigating the subject, there is no justification for a dogmatic stand. The present writer offers

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*Accepted for publication February 7, 1931.

[1] G. Burt and R. C. Moore, The mental differences between the sexes, *J. Exper. Ped.*, 8, 1912, 273-284, 355-388.
A. I. Gates, Experiments on the relative efficiency of men and women in memory and reasoning, *Psychol. Rev.*, 24, 1917, 139-146.
W. K. Pyle, *The Examination of School Children*, 1913, 8-14; *The Psychology of Learning*, revised ed., 1928, 147-151.
M. C. Schuyten, Sur les methodes de mensuration de la fatigue des écoliers, *Arch. d. Psychol.*, 4, 1904, 113-128.
J. C. Shaw, A test of memory in school children, *Ped. Sem.*, 4, 1896, 61-78.
G. M. Whipple, *Manual of Mental and Physical Tests*, 1921, 211-213. This author also summarizes the results of other investigators, cf. 179-184, 213 f.

[2] A. H. Chamberlain, A memory test with school children, *Psychol. Rev.*, 22, 1915, 71-76.
M. Lobsein, Experimentelle Untersuchungen über die Gedächtnisentwickelung bei Schulkindern, *Zsch. f. Psychol.*, 27, 1901, 34-76.
D. O. Lyon, The relation of quickness of learning to retentiveness, *Arch. Psychol.*, 1916 (No. 34), 1-60.
E. F. Mulhall, Tests of memories of school children, *J. Educ. Psychol.*, 8, 1917, 294-302.
A. Netschajeff, Experimentelle Untersuchungen über die Gedächtnisentwickelung bei Schulkindern, *Zsch. f. Psychol.*, 24, 1900, 321-351.
F. Schramm, Zur Aussagetreue der Geschlechter, *Zsch. f. ange. Psychol.*, 5, 1911, 355-357.

[3] A. Aall, Zur Psychologie der Wiederzählung, *Zsch. f. ange. Psychol.*, 7, 1913, 185-210.
H. B. L. Vos, Beiträge zur Psychologie der Aussage, *Zsch. f. ange. Psychol.*, 4, 1911, 375-378.

some results which seem to support the third of the above positions in indicating a superiority of boys over girls in tests of factual memory given to children of grades VII to XII inclusive.

Method. Pyle states that experimenters reporting male superiority in memory tests make their comparison by grade and not by age. Since in the same grade the boys tend to average higher in age than girls, this is not an adequate basis for comparison. An adequate sex comparison should be made by age, not by grade.[4] In keeping with Pyle's suggestion, the present writer sought to avoid both difficulties by pairing the *S*s into parallel sex groups by both age and grade. The *S*s were chosen from a much larger number of another experimental group in such a manner that every boy had opposed to him in the other group a girl in the same grade and of the same age within two months. Three sets of such paired groups were used.

The *S*s were given printed pamphlets containing a short article approximately 1000 words in length and were instructed to read it carefully a single time. When they finished reading they were given a comprehensive test covering the facts contained in the article and were requested to answer the questions as carefully as possible. A different article was used for each set of paired groups, and the tests covering them contained from 100 to 125 multiple-response type questions. The articles are entitled *Radium: The Magic Metal*, *The Early Germans*, and *Sir Richard Arkwright*, and will herein be referred to by their key words. The articles are of suitable difficulty for pupils in the junior and senior high schools, and the tests possess a highly satisfactory degree of objectivity, reliability, and validity.[5]

Results. The means for the respective groups, the means of all the *S*s of a given sex reading an article and their standard deviations, and the data on the differences of the means are given in Table I. The scores are in terms of percentage of items answered correctly.

A study of the table reveals that, in the case of *Radium*, 8 of the 9 comparisons of age groups favor the boys and 1 the girls; in the case of *Germans*, 6 out of the 9 comparisons favor the boys and 3 the girls; and in the case of *Arkwright*, 6 out of 8 comparisons favor the boys, 1 the girls, and 1 ties. Thus in 20 out of 26 comparisons the boys have the higher average scores, in 5 the girls are superior, and in 1 no advantage lies with either sex.

Comparing the averages for the total groups, we find the mean of the boys to exceed that of the girls in *Radium* by 3.7. Since the standard deviations of the two groups are 12.0 and 11.9 respectively, the probable error of this difference is .74, and the difference is 5.04 times its probable error and completely reliable. Similarly the differ-

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[4] *Psychology of Learning*, 1928, 150.

[5] Cf. A. G. Dietze, Factual memory of secondary school pupils for a short article which they read a single time, *Univ. of Pittsburgh Bull.*, *Graduate School Abstracts*, 6, 1930, 39-46.

TABLE I

AVERAGE IMMEDIATE FACTUAL MEMORY OF PARALLEL SEX GROUPS PAIRED BY AGE AND GRADE

Age	Radium (243 *Ss*)			Germans (220 *Ss*)			Arkwright (198 *Ss*)		
	No.	Boys	Girls	No.	Boys	Girls	No.	Boys	Girls
11	6	64.2	75.0	7	63.9	49.6	6	50.0	45.8
12	40	72.4	70.1	31	59.1	60.7	14	54.6	50.4
13	35	70.4	68.1	36	59.0	54.4	19	54.3	50.1
14	42	75.6	69.0	51	61.8	63.0	26	50.9	50.2
15	46	78.0	73.0	28	62.5	62.2	52	53.8	49.3
16	32	80.0	75.2	37	70.7	71.3	23	62.3	54.0
17	30	81.2	77.3	14	80.0	73.2	38	62.8	63.2
18	11	80.2	78.9	11	74.8	68.4	20	62.5	62.5
19	1	82.5	57.5	5	77.5	70.5			
Av.		75.8	72.1		64.9	63.3		57.0	54.3
S.D.		12.0	11.9		15.4	16.6		13.3	13.3
Diff.		3.70			1.60			2.70	
P.E.$_{\text{Diff.}}$		0.74			1.03			0.90	
Diff./P.E.$_{\text{Diff.}}$		5.04			1.55			2.99	

ence of 1.6 in favor of the boys in *Germans* is 1.55 times its probable error, or little better than chance; and the difference in *Arkwright* is 2.99 times its probable error and, therefore, fairly reliable.

Conclusions. We need not necessarily interpret the above results as contradictory to Pyle's statement "it seems clear . . . that if we rule out the influence due to differences in experience and interest, the retention of girls is better than that of boys."[6] In the case of *Radium* the boys did, perhaps, have the advantage of greater interest. The other materials, however, probably had equal appeal. In view of the conflicting results and the small differences commonly observed between the sexes, it is difficult to accept as final Pyle's conclusions that "retention of girls is better than that of boys because of their more rapid development."[7] The correlation of the results of the boys and girls in the case of *Radium*, where the difference is most clearly marked, gives a biserial r of 0.196 ± .037.[8] This is obviously a very low relationship between memory and sex even though it is four times its probable error and even though the difference between the means is statistically reliable.

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[6] *Op. cit.*, 150.

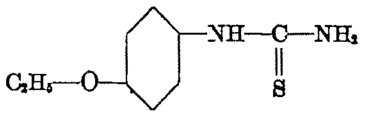
[7] *Op. cit.*, 146-151.

[8] For interpretation of biserial r see K. J. Holzinger, *Statistics for Students of Education*, 1928, 271-273.

A PRELIMINARY STUDY OF TASTE DEFICIENCY

By Samuel W. Fernberger, University of Pennsylvania

During the last several months a number of short papers and abstracts have appeared about the taste reactions and taste deficiencies of a complex chemical substance developed by Dr. Arthur L. Fox of the E. I. duPont de Nemours and Company laboratories. Fox calls the substance para-ethoxy-phenyl-thio-carbamide. It is the sulphur analog of dulcin, which is a substance described as about 300 times sweeter than sugar. The chemical formula of the substance in which we are interested is as follows:



As reported by Science Service,[1] Fox discovered that 60% who ingested this substance found it exceedingly bitter and the other 40% found it tasteless. No mention is made of the number of cases tested. Fox also reports what he calls 'taste-blindness' for other compounds in the thio-urea group. But everyone apparently finds dulcin in this chemical group sweet.

Blakeslee and Salmon publish the most comprehensive report on this problem.[2] Blakeslee prefaces this discussion with an observation on 'odor-blindness' made in 1927 which is not without interest. In his garden he had two verbena plants of different colors. On the basis of testing 40 persons he reports that 66% found plant A fragrant and B odorless while 33% found A odorless and B fragrant.

Using Fox's compound and working for taste thresholds Blakeslee and Salmon used 5 dilutions, the most dilute being tasteless to all subjects. They also used dry crystals. A total of 239 individuals were tested with the dilutions for threshold and 283 with the crystals. The primary interest of these authors was in testing families to discover a possible hereditary factor. They make, however, a number of general observations. The substance may sometimes be tasted but the taste is described as other than bitter. Working with the dry crystals it was found that of 152 men 38.2% and of 131 women 25.2% found it tasteless. Most of those who were able to taste it described the taste as extremely bitter. The authors say "the tables suggest that females are somewhat more acute tasters than the males but larger numbers will be necessary to show whether these differences are significant."

With regard to the question of inheritance, Blakeslee and Salmon find, from their analysis of family data, that the taste deficiency is apparently due to a single recessive gene. Their statement is emphatic. "Enough is known" they

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*Accepted for publication January 18, 1932.

[1] *Science*, 73, 1931 (no. 1894), 14.

[2] A. F. Blakeslee and M. R. Salmon, Odor and taste blindness, *Eugenical News*, 16, 1931, 105-109.

write, "in regard to the inheritance of this specific taste 'blindness' to enable one to make predictions. If both parents are negative the children will all be negative. If one parent tests 0 (negative) and a child is found to be a taster, the other parent must be a taster."

Snyder,[3] on the basis of testing 100 families with this same compound, arrives at the same conclusions with regard to the inheritance of the deficiency. His percentages of tasters and non-tasters agree closely with those of Blakeslee and Salmon, being 68.5% and 31.5% respectively. Without giving his statistical basis in this report, he states that the taste deficiency "is not a matter of age, sex, nor race." This conclusion is not only at variance with the tentative findings of Blakeslee and Salmon with regard to sex but with regard to age as well.

Two more short reports of papers, read at the meetings of the National Academy of Sciences, have been published. Fox reports similar taste reactions in "a large number of related mono-aryl-thio-carbamides" and "symmetrical di-aryl-thio-carbamides."[4] Blakeslee,[5] continuing his threshold work, reports that "acuteness of taste for the chemical is inherited since there is an obvious correlation between the thresholds of parents and children." Suggestions are made for administering the dry crystals. Taste confusions for this and other substances are reported. Unfortunately the full reports of both of these papers have not appeared at the time of this writing.

In view of the results obtained with this compound, it seemed worth while to extend the experiment along psychological lines.[6] Dry crystals were administered to 1025 adults at the college student level. Of these 528 were men and 497 women. They were mostly students in psychology at the University of Pennsylvania and had a background of a discussion of fundamental taste qualities. They were told to swallow the substance, so as to stimulate the root of the tongue, and then report the experience in terms of one of the five following categories: tasteless, slightly bitter, bitter, very bitter, and extremely bitter. The category of 'tasteless' more or less defines itself; 'extremely bitter,' the other limiting category, was defined in terms of raw quinine, which unfortunately in these days is only known to less than half of the group. The middle term, 'bitter,' was defined in terms of the white of the rind of grapefruit. The *Os* seemed to have no difficulty in making judgments in terms of these categories. For the first 200 *Os*, litmus tests were made of the saliva before stimulation but this was discontinued inasmuch as there was obviously no relation between taste and the acidity or alkalinity of the saliva.

The results will be found in Table I. In order to more readily visualize these reactions the results are also produced as curves in Fig. I. The results rather closely conform to those of Blakeslee and Salmon and of Snyder with regard to the percentage of tasters and non-tasters, although our results are somewhat lower for non-tasters than both of the other sets. The relatively larger percentage of non-tasters among the men as compared with women, as reported by Blakeslee and Salmon, is confirmed by our results.

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[3] L. H. Snyder, Inherited taste deficiency, *Science*, 74, 1931, 151-152.

[4] A. L. Fox, The relationship between chemical composition and taste, *ibid.*, 607.

[5] A. F. Blakeslee, Genetics of sensory thresholds: Taste for phenyl thio carbamide, *ibid.*, 607.

[6] Dr. Fox kindly supplied me with the para-ethoxy-phenyl-thio-carbamide used in this experiment.

TABLE I

NUMBERS AND PERCENTAGES OF JUDGMENTS OF TASTE DISCRIMINATION

Judgment	Men		Women	
	No.	%	No.	%
Tasteless	158	29.9	116	23.3
Slightly bitter	72	13.5	30	6.0
Bitter	54	10.2	57	11.5
Very bitter	120	22.7	118	23.7
Extremely bitter	124	23.5	176	35.4
Total	528		497	

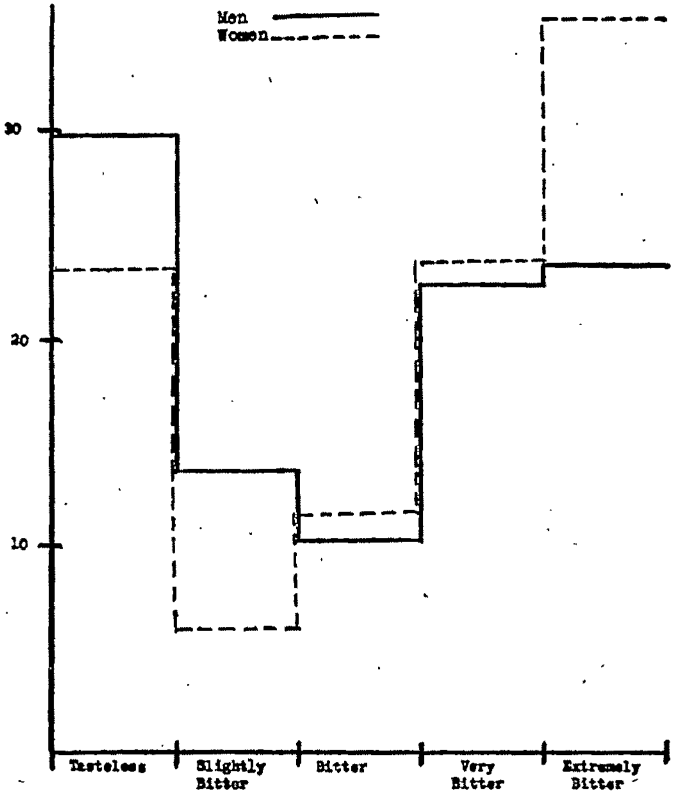

FIG. 1. PERCENTAGES OF JUDGMENT OF TASTE DISCRIMINATION FOR MEN AND WOMEN

An examination of the curves in the figure indicates, that for both men and women, they are bimodal. The percentages are high for 'tasteless' at one end and again for 'very bitter' and 'extremely bitter' at the other end. The percentages are low for both 'slightly bitter' and 'bitter' in the middle of the curves. Nevertheless one finds, what one would expect, that individuals exhibit all degrees of taste from excellent discriminations to total indiscrimination. Although the curves for men and women are of the same bimodal form still they are considerably and significantly different. The percentages for men are considerably higher at the indiscrimination end for both 'tasteless' and 'slightly bitter.' At the other end the percentages for women are invariably higher; only slightly higher for 'bitter' and 'very bitter,' but considerably larger for 'extremely bitter.' These differences in the form of the curves emphasize the sex difference to taste acuteness even more than the mere statement of the percentages of tasters and non-tasters. They amply verify the statement of Blakeslee and Salmon that women are more acute tasters than men.

TABLE II

FRACTIONATED NUMBERS AND PERCENTAGES OF JUDGMENTS OF TASTE DISCRIMINATION

	First Group				Second Group			
	Men		Women		Men		Women	
Judgment	No.	%	No.	%	No.	%	No.	%
Tasteless	104	30.8	49	22.8	54	28.4	67	24.6
Slightly bitter	53	15.7	17	7.9	19	10.0	13	4.8
Bitter	30	8.9	26	12.1	24	12.6	21	7.7
Very bitter	64	19.0	51	23.7	56	29.5	67	24.6
Extremely bitter	87	25.7	72	33.5	37	19.5	104	38.3
Total	338		215		190		272	

In order to further verify this sex difference we have fractionated our results into two groups in the order in which they were taken. These results are given in Table II. Group I includes 553 *Os* (338 men and 215 women), and Group II includes 462 *Os* (190 men and 272 women). The results were obtained from classes and it seemed more proper to treat all results of a given class as a unit in its group even though the groups were of unequal size and the relative proportion of men and women was different. The curves for these two groups are very similar. In both groups the percentages for men are higher at the indiscrimination end for both categories of 'tasteless' and 'slightly bitter' and the percentages for women are higher at the other end for 'extremely bitter.' Inversions are found for the intermediate categories of 'bitter' and 'very bitter' but these middle categories are the only ones where the results are similar in any case for the two sexes.

It may be of interest to add the observation that a few *Os* noted taste confusions. These were classed as non-tasters when it was further ascertained that under no circumstances could the taste be described as bitter. These observations are too few to be given statistical treatment. The small number of such confusions may be due to the setting of our conditions with the definition of the four bitter categories which mitigated against the frequency of the reporting of other taste qualities.

All of the previous authors have spoken of the situation of indiscrimination of this compound as "taste blindness" apparently on the analogy of color-blindness. In certain respects the two are analogous: both involve indiscrimination of a particular sensory quality for some individuals while the same stimulus arouses intensive sensation in others; both show degrees of sensory weakness from full sensitivity to total indiscrimination and discrimination in both cases has a hereditary basis. The analogy would perhaps be complete except for two conditions. In the case of color-blindness to certain qualities it has been found that the indiscrimination covers all variations of the fundamental quality. If one is color-blind to red and green there is difficulty with *all* reds and greens. In the present case it is reported that individuals who find this substance utterly tasteless are nevertheless able to taste other bitters. Also in color-blindness there seems always to be a confusion of qualities. A given sensory quality is not seen as the *normal* quality but it *is* seen. And relatively few cases of such confusion are reported by previous workers or in the present experiment. Apparently in the case of this compound one either reports the taste of bitter or one fails to taste it at all. The reporting of some different taste seems to be relatively unusual. Indeed in this respect the situation seems more like that of the auditory indiscrimination of tonal islands.

But it would seem that the use of this and similar compounds offers an unexpectedly excellent opportunity for the psychological study of sensory indiscrimination. There are all too few cases of color-blindness and of tonal deafness. But here in taste approximately one-third of the people tested show indiscrimination.[7]

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[7] Since writing the paper the following have appeared: A. L. Fox, The relationship between chemical constitution and taste, *Proc. Nat. Acad. Sci.*, 18, 1932, 115-120, and A. F. Blakeslee, Genetics of sensory thresholds: taste for phenyl thio carbamide, *ibid.*, 120-130. Fox, after investigating the taste of a large number of compounds of similar composition, comes to the conclusion that the taste of the C=S linkage is the basis of the bitter quality. He advances the theory for the failure of some individuals to taste this as due to the relative degree of solubility of the compounds in the saliva. Blakeslee, working with our present compound, determines the threshold values and finds that, for this compound, these have no relation to the thresholds for other bitters (quinine and picric acid) and for sour, sweet and salt. There is also no relation between the bitterness and the acidity of the saliva as measured by the hydrogen ion concentration.

A COMPARATIVE STUDY OF THE ERRORS OF LOCALIZATION ON THE FINGER-TIPS

By KARL M. DALLENBACH, Columbia University

This study is intended as a check or control of the experiment on localization that Professor Harry Helson and the writer are performing in connection with a study of nerve regeneration.

In 1924 the writer cut all the nerves, subcutaneous as well as cutaneous, that led to the radial half of the first phalanx of the left index finger.[1] Study of the errors of localization was one of the ways in which regeneration of the nerves and return of sensitivity were investigated. As the incision was accidental and the experiment was not undertaken until after that event, we could not obtain the normal error of localization for the experimental tissue. Since some idea of the normal error was necessary for the precise evaluation of the results, we attempted to gain the desired information indirectly by studying the errors of localization on normal, comparable tissues. We, therefore, repeated the localization experiment upon similar areas on the right index and left great fingers.

The radial side of the first phalanx of the right index finger bears the closest resemblance anatomically to the experimental tissue; but experiments upon it alone would not yield a norm which we could accept as representative of the tissue on the left index, because *O* is right-handed. His right-handedness may mean (1) that the right index finger is more sensitive than the left—in which case the norm obtained from the right would be too small for the left; or (2) that the localizations, since they were made with *O*'s left hand, are less accurate and precise than the right-handed localizations made on the left index—in which case the norm obtained would be too large. The extent to which these opposite and antagonistic factors are effective can not be known short of a check or control experiment upon *O*s with normal tissues.

We sought, nevertheless, to eliminate one of these factors—differences in the accuracy and precision of localization caused by handedness—by repeating the experiment upon the radial side of the first phalanx of *O*'s left great finger. Here *O* made his localizations, as he had made them on his left index, with his right hand. Still, the norm obtained here can not be accepted as representative of the experimental tissue because of possible differences in the sensitivity of the two areas. How the radial areas of the left index and left great fingers differ can again not be known until a series of control experiments have been made.

Problem. The problem of the present study is a comparative one. We wish to know how, with normal skin tissue, the errors of localization upon the radial side of the first phalanx of the left index, the right index, and the left great finger compare.

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*Accepted for publication January 11, 1931.

[1] For the localization and extent of the incision, cf. K. M. Dallenbach, The bidimensional reproduction of the surface of a tridimensional object, this JOURNAL, 43, 1931, 127.

Observers. Six *Os*—3 men (*A*, *B*, and *C*) and 3 women (*D*, *F*, and *G*)—served in this experiment. All were undergraduate students of Cornell University and members of the class in qualitative laboratory psychology. They had all completed the routine localization experiment at the time that they served in this study.

Method and procedure. The errors of localization were determined for every one of the *Os* for the radial side of the first phalanx of (a) the left index finger, (b) the right index finger, and (c) the left great finger. The usual procedure of localization experiments was followed. *O* was blindfolded and seated in a comfortable position at a table, with the experimental area held in a fixed position by a plaster-cast hand-rest. *E* then marked with ink 10 spots, distributed haphazardly over the experimental area. Every spot was stimulated with an aesthesiometer in chance order, but in turn, 10 times. The point of the aesthesiometer, which was rounded and made of gutta-percha, was 1 mm. in diam. It was applied after a 'ready,' 'now' signal, on the all-or-none principle, with a pressure of 12 grm.; and it was allowed to rest upon the skin for approximately 1 sec.

As soon as *E* removed the stimulator, *O* tried to locate the spot touched, using a gutta-percha pointed stylus, which was similar in weight and dimensions to the aesthesiometer used by *E*. He was allowed to move it freely over the surface of the skin until he was satisfied with his localization. He then pushed gently upon the stylus so as to leave, when the stylus was removed, a slight indentation. This mark was used by *E* in measuring the error of localization. Measurements were made by *E* with the aid of dividers and a millimeter scale.

Before every experiment *E* brought *O*'s stylus, which was held in *O*'s free hand, to a fixed point upon the table. The gross movements that *O* had to make in localizing the spots were consequently the same in every experiment.

The experiments (100 in number) upon an area were completed during a single observational period. The order in which the different experimental areas were considered was varied, so as to reveal the effects of practice, if any, for the different *Os*. The order for *A* and *B* was left index, right index, and left great; for *C* and *D*, right index, left great, and left index; and for *F* and *G*, left great, left index, and right index. Every area was, therefore, experimented upon as frequently in the first as in the second and third positions.

The experiments were conducted on all the *Os* at the same hour of the day, *i.e.* between 2:00-4:00 P.M.

The extent and direction of the error of localization, *i.e.* whether the error was peripheral (P), central (C), dorsal (D), ventral (V) or intermediate between these cardinal points, was recorded by *E* in the gross data sheets.

Results. (*a*) *Extent of the average error.* Table I gives, for every *O* and for every experimental area, the average error of localization together with its m.v. It shows that the average is, for all the *Os* except *G*, smallest for the left index finger, largest for the left great finger, and intermediate in size for the right index finger. For *G* the left index finger gives the largest average error, the right index the smallest, and the left great finger an average error intermediate in size.

In spite of the approximate uniformity of these results, the differences among these experimental areas (shown in Table II) are so small, and the m.v. of the averages (shown in Table I) are so large—though not larger than is usual in work of this kind—that the mathematical significance of the differences is very slight.

TABLE I

The Average Error of Localization with Its Mean Variation for Every *O*, for Every Experimental Area

(The rank of the different averages among the three for every *O* is given in parenthesis after the value)

		Experimental Area		
O	Sex	Left Index	Right Index	Left Great
A	M	0.94± .58 (1)	0.97±.61 (2)	1.17± .56 (3)
B	M	1.38± .81 (1)	1.46±.64 (2)	1.62± .95 (3)
C	M	1.61± .82 (1)	1.66±.96 (2)	1.74±1.10 (3)
D	W	1.85± .53 (1)	2.09±.60 (2)	2.23± .52 (3)
F	W	1.62± .32 (1)	1.68±.26 (2)	1.72±1.00 (3)
G	W	1.83±1.30 (3)	1.63±.70 (1)	1.68±1.00 (2)

TABLE II

The Differences, for Every *O* and for Every Experimental Area, among the Average Errors of Localization

	Difference between		
O	Left Index and Right Index	Left Index and Left Great	Right Index and Left Great
A	−.03	−.23	−.20
B	−.08	−.24	−.16
C	−.05	−.13	−.08
D	−.24	−.38	−.14
F	−.06	−.10	−.04
G	+.20	+.15	−.05

The greatest differences among the average errors of localization are shown by *D*, and the m.v. of her averages are among the smallest obtained. Her data, of all our data, are, therefore, the most favorable for showing a difference among the experimental areas used in this study. Computation of the significance of her differences yielded the following results. The difference between the average errors of localization on the left and right index fingers is 0.24 mm., the σ diff. is 2.30, and the D/σ diff. is 0.10; the difference between the left index and the left great finger is 0.38 mm., the σ diff. is 2.44, and the D/σ diff. is 0.15; and the difference between the right index and the left great is 0.14 mm., the σ diff. is 2.33, and the D/σ diff. is 0.06. These data mean that these differences might be expected to occur 54, 56, and 52 times, respectively, in 100 trials—*i.e.* practically with chance frequency. Since *D*'s data do not show reliable differences, we feel justified in concluding that the differences of the other *O*s are likewise due to chance.

The results of this study indicate, therefore, that the average error of localization obtained from any one of the three experimental areas used in this experiment may be used as norms for the other two.

TABLE III

THE NUMBER OF TIMES, FOR EVERY *O* AND FOR EVERY EXPERIMENTAL AREA, THAT AN ERROR WAS MADE IN EVERY ONE OF THE CARDINAL DIRECTIONS

(Italics show for each dimension the direction giving the greater number of errors)

Area	*O*	Direction *P*	 *C*	 *D*	 *V*
Left Index	*A*	*41.5*	20.0	6.5	*22.0*
	B	*32.0*	17.5	18.0	*23.5*
	C	22.5	*52.5*	6.5	*23.5*
	D	5.5	*42.0*	1.0	*41.5*
	F	26.0	*27.5*	6.0	*26.5*
	G	*35.0*	13.5	9.5	*20.0*
Right Index	*A*	*32.0*	11.5	7.5	*33.0*
	B	*37.0*	11.5	17.5	*23.0*
	C	10.0	*29.5*	7.0	*43.5*
	D	11.5	*42.5*	12.0	*23.0*
	F	16.5	*37.0*	12.0	*21.5*
	G	*46.0*	10.5	9.5	*15.0*
Left Great	*A*	*37.0*	20.5	12.5	*19.0*
	B	*38.5*	11.0	19.0	*21.5*
	C	21.0	*29.5*	17.0	*19.5*
	D	5.0	*61.0*	10.5	*15.5*
	F	19.5	*24.5*	10.5	*21.5*
	G	*36.5*	16.0	7.0	*26.5*

(*b*) *Direction of the error.* Table III gives for every *O* and for every experimental area the number of times an error was made in the peripheral, central, dorsal, or ventral direction. The gross data sheet divided the errors into 8 groups, the cardinal directions mentioned and the 4 intermediate ones, *i.e.* PD, PV, CD, and CV. The data were, however, consolidated so as to bring out more clearly the directional aspects of the errors of localization. The consolidation was accomplished by dividing the number of errors listed under the intermediate directions between the two cardinal directions concerned; for example, if 11 PD errors were indicated for an *O*, these were divided equally between P and D, *i.e.* 5.5 errors were added to the peripheral and dorsal errors.

Three of the *O*s (*A*, *B*, and *G*) made more peripheral than central errors in their localizations in all of the experimental areas, and three (*C*, *D*, and *F*), contrariwise, made more central than peripheral errors in all of the areas. These results show that central or peripheral disposition is an individual matter, and, furthermore, that as an individual is disposed in one of our experimental areas he will very likely be disposed in the others.

All of the *O*s made, in all of the experimental areas, more ventral than dorsal errors. The constancy of this result may be due to the fact that the *O*s always approached the experimental areas in making their localizations from a ventral direction. *E*, it will be recalled, placed *O*'s stylus before every experiment at a fixed position which was ventral to the experimental area. Whether this is the explanation of the predominant ventral tendency shown in our results, future experiments alone can determine. It suffices for the purpose of this study to note that there is no difference in this respect among our experimental areas. A ventral error in one of our experimental areas is correlated with ventral errors in the others.

Conclusions. From the results of this study we may conclude that there are no reliable differences either in extent or direction of errors of localization among the experimental areas used in this study. The norm obtained from one of them will serve as a norm for the other two.

We may, therefore, in the localization test of nerve regeneration that Professor Helson and the writer are making, safely use the average values from our *O*'s right index and left great fingers as norms for his left index finger.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

V. PHENOMENA ACCOMPANYING THE INHIBITION AND CONTROL OF BREATHING

By RUDOLF VOGELER and J. P. GUILFORD

A previous study was concerned with the ability of the average subject to inhibit breathing and to breathe slowly, and with the rate of learning either to inhibit or to control breathing.[1] Two groups of *S*s were trained for an hour a day over a period of 15 days, and kymograph records were kept of every minute of practice. These records reveal several interesting phenomena which seem worth reporting. The experimental procedure was fully described in the previous report and so will not be repeated here.

The present study deals with the following questions to which our data give at least a partial answer:

(1) What is the difference, if any, in the return to normal breathing after holding the breath, and after slow breathing?

(2) What is the relation between the breathing phase at the signal to 'hold' and the ability to hold the breath? In other words, does 'holding' at different stages of the inspiration-expiration cycle have an effect upon the length of the hold?

(3) What is the difference, if any, in the physiological consequences due either to 'holding' or to 'slow breathing'?

(4) What factors, favorable and unfavorable, influence 'holding' and 'slow breathing'?

An examination of the kymograph records shows at once (a) that immediately after the end of a period of inhibition or of slow breathing, there is always a compensatory period of very deep breathing and a more or less gradual return to normal breathing; and (b) that there is a distinct difference between the two cases, inhibition and slow breathing.

The curves designating the return to normal were measured as follows. Five series of breathing curves which appeared to be typical for every *S* were chosen from the kymograph records obtained on different days. The same general appearance of the curves chosen was exhibited throughout the entire training period. Each one of the first fifteen successive exhalations in each series was measured from a line bisecting the curves, approximately in the center, to the peak of each curve. Only the exhalations could be measured because the entire amplitude of these curves could not be recorded. During very deep breathing, the lever of the recording apparatus was stopped by the edge of the disk of the tambour, thus

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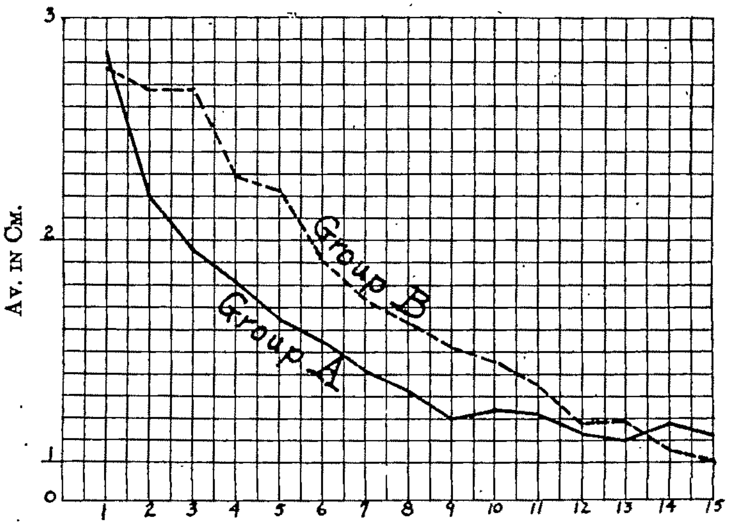
*Accepted for publication February 15, 1931.

[1] R. Vogeler and J. P. Guilford, Learning to inhibit and control breathing, this JOURNAL, 43, 1931, 624-630.

preventing the registration of the complete inhalation. We have assumed that the exhalations exactly balance the inhalations, and the records tend to show that this assumption is correct. By averaging each curve of the five series chosen, the differences in height for the fifteen successive peaks were obtained. Then the averages for each group of 5 *Ss* were computed and plotted as in Fig. 1.

Fig. 1 shows the average height of each curve in each series. One *S* in each group showed such a distinct departure in the size of the first few exhalations as compared to the other *Ss* in the group, that these averages were not included.



Successive Breaths

Fig. 1. Return to Normal Breathing
Group A after 'holding'; Group B after 'slow breathing'

As it is, averaging the results of 4 *Ss* tends to smooth out the differences between the two groups, because some *Ss* tend to return to normal more quickly than others. It was difficult to measure the exact center of each series and consequently only the relative differences are obtainable.

Fig. 1 indicates a decided difference between the two groups. The curve for Group A (the *Ss* who held their breath), which starts above that for Group B, drops very rapidly at the start and begins to flatten out at the ninth breath. The curve for Group B (the *Ss* who breathed slowly) drops more gradually at first, then more rapidly starting at the fifth breath, beginning to flatten out at the twelfth breath. Group A apparently comes back to normal sooner than Group B, as indicated by (1) the more rapid initial descent, and (2) the more rapid flattening out of the curve. That the above is a true description of the two types of returns to normal for both groups is borne out by the fact that the individual curves show the same phenomena in every case.

The time element may be the factor causing this difference, since 'slow breathing' practice periods lasted longer, comparatively, than 'holding' in most cases.

The longest 'holding' period lasted 93.3 sec., whereas the longest 'slow breathing' period lasted 746 sec. In other words, the amount of work expended by Group B was greater than that of Group A, thus demanding a greater amount of subsequent ventilation.

It is interesting to note that this greater ventilation in Group B compensated for the rapid decrease in the curve of Group A, thus enabling both groups to approximate normal breathing at about the same time. But the continued decrease in ventilation for Group B continues beyond the point of normal breathing! This seems to indicate a 'carry-over' effect of 'slow breathing' practice to continue beyond the normal. This phenomenon is brought out by the kymograph records in several cases. Some of the introspections bore this out also. For example, one *S* reported that "slow breathing becomes almost automatic"; another *S* that during the day he found himself "taking a deep breath" as though he "were not getting enough air." The latter phenomenon might certainly indicate an automatic return to 'slow breathing' followed by a deep compensating inhalation.

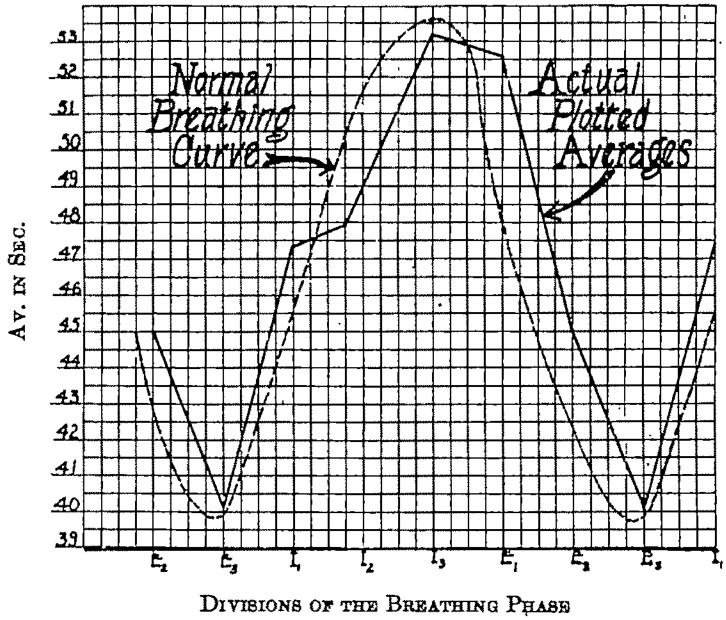


Fig. 2. Relation of the Breathing Phase to the Length of the 'Hold'

(I_1 represents the first third of inspiration; I_2, the middle third; and I_3, the last third; E_1 represents the first third of expiration; E_2, the middle third; and E_3, the last third.)

The relation between the breathing phase at the signal 'hold' and the ability to 'hold' proved to be very interesting. This relation was determined as follows: the entire breathing cycle was divided into six parts, during any one of which the signal might have been given.

In Fig. 2 the dotted line represents the breathing cycle. I_1 represents the first third of the inspiration, I_2 the middle third, and I_3 the last third or the point of greatest inspiration, E_1 the first third of the expiration, E_2 the middle third, and E_3 the last third or the point of greatest expiration.

Upon examination of the kymograph records it was determined at which of these six phases the 'hold' began. Reverting to the daily records, the lengths of this 'hold' were tabulated. There were 77 measurements for I_1; 35 for I_2, 112 for I_3, 39 for E_1, 58 for E_2, and 45 for E_3. All the measurements for each phase were averaged. The results show that the length of the 'hold' is directly proportional to the amount of air in the lungs.

It is interesting to note that the plotted averages, (see Fig. 2), form a curve which approximates very closely the normal breathing curve. The rise of the curve is gradual, corresponding to the inspiration in normal breathing, whereas the fall is sharper, corresponding to the more rapid expiration. From the appearance of the curve it will be noticed that the plotted points denoting the last third of the inspiration and the first third of the expiration are almost equal. At these points the amount of air in the lungs is approximately the same.

It has been shown previously that there is no relation between vital capacity and normal breathing, nor any relation between the vital capacity and the return to normal.[2] Nevertheless, there is a distinct difference in the physiological results obtained due to 'holding' and 'slow breathing' as reported by the *Ss*. The most outstanding of them for Group A were as follows: (1) oppression in the chest region, (2) feeling as though lungs and diaphragm were fluttering, (3) accentuated throbbing of the heart, (4) yawning and drowsiness, (5) soreness in the chest region. For Group B they were: (1) fatigue in the abdominal region, (2) contractions in the abdominal region, (3) increased bodily temperature, (4) restlessness, (5) sensation of choking. The effects, regarded collectively, are indicative of asphyxia in Group A, and of fatigue in Group B.

The connection between 'holding' and asphyxia, and between 'slow breathing' and muscular exertion and effort, may be made more convincing if the two cases are discussed separately. The 'holding' group will be treated first. The oppression in the chest region would seem to indicate that the stimulus for the breathing reflex is located to some extent, at least, in the peripheral organs. The impressions of 'fluttering' lungs and diaphragm may be real abortive automatic attempts to breathe. The accentuated throbbing of the heart is apparently a compensatory measure to make up for a vanishing source of oxygen in the lungs. There is little doubt that the prolonged inhibition with its accompanying deprivation of oxygen caused subsequent yawning and a feeling of drowsiness. The "soreness in the chest region" still remains to be explained. It must be remembered that the complete inhibition, which is being discussed, is in some respects the same as maintaining a postural position for a long time, in which the muscles assume a state of static contraction. Upon the strength of this explanation the "soreness" is a real fatigue-effect, but it is more properly regarded as an after-effect, not as an accompanying factor.

In 'slow breathing' the factor of "fatigue in the abdomen" is reported, but not described very fully. If the analogy of 'slow breathing' to a working muscle is valid, the fatigue is easily explained. This analogy is strengthened by the fact that the bodily temperature was apparently increased as shown by the appearance of such physiological indications as perspiration and flushing of the face, probably due to an increased rate of oxidation. A factor similar to the 'diaphragm fluttering' of Group A is found in Group B in which "contractions in the abdominal region" were reported. This seems to be merely an attempt of the diaphragm to react in its rhythmic way, but through the voluntary control the automatic or reflex excitation is inhibited immediately after or soon after it has started. It should be mentioned that this phenomenon was reported oftener in Group B than in Group A.

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[2] *Op. cit.*, 626.

The factor of 'restlessness' such as the moving of hands and feet, seems to indicate an innervation of the surrounding musculature in the attempt to breathe adequately. This is still further brought out by the reports of several *Ss* who claimed that 'strained feelings' were spread into the arms and back, causing them at times to shift their posture. The symptom of sensations of choking is somewhat more indicative of asphyxia than of fatigue, however.

Our previous discussion of the relation between the breathing phase and the length of the 'hold' certainly bears out the fact that "starting with full lungs" is a favorable factor. The reports that "reading interesting material" and "forgetting about the breathing" were favorable factors, are inter-related. Bair's work on voluntary control shows the advisability of removing the attention from the movement to be effected.[3] This makes for relaxation which in turn results in a greater ability to inhibit. This strengthens the conclusions to be drawn from the report on physiological results in which relaxation seemed to be a favorable factor.

The analogy of 'slow breathing' to a working muscle is strengthened by the report of the *Ss* that "fatigue in the abdominal region" was an unfavorable factor. One *S* went so far as to report that "it is not a lack of air, but rather fatigue in the abdomen" which made him stop "breathing slowly."

The mental attitude of *S* helped very much to determine the results on many occasions. The presence or absence of determination and confidence, for example, were to some extent responsible for a corresponding increase or decrease in the records.

Eating shortly before the practice session usually resulted in a decreased average for that session. The rate of metabolism being increased under these conditions, a decrease in the supply of oxygen could not be maintained for a very long time. In other words, an additional load above the average was being carried by the inhaled oxygen in fulfilling its additional work of oxidation during the process of digestion. The pressure exerted by the full stomach upon the diaphragm is another factor which must be considered under these circumstances.

Consciously or forcibly expanding and contracting the thoracic and abdominal regions while 'holding' was reported as favorable for a longer 'hold.' This might make it possible to use to maximum advantage the residual air in the lungs; or it may be that a voluntary action inhibits a reflex which it resembles or which employs the same muscle groups.

For 'slow breathing' two individual devices were reported: (1) tensing the abdominal region, and (2) smoothing out the breathing. The 'tensing' seems to be a conscious attempt to inhibit the reflex or rhythmic contractions which were felt at different times during the practice. "Smoothing out the breathing" is for the 'slow breathing' group what 'relaxation' is for the 'holding' group. Further experimentation using designated types of favorable devices should prove interesting and more conclusive.

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[3] J. H. Bair, Development of voluntary control, *Psychol. Rev.*, 8, 1901, 474-510.

Conclusions

(1) The return to normal breathing after holding the breath shows a very rapid initial drop, the curve flattening out at about the ninth breath, whereas the return to normal after breathing slowly shows a more gradual curve, the rapid fall starting at about the fifth breath and the curve flattening out at about the twelfth breath.

(2) The length of time one can hold the breath is directly proportional to the amount of air in the lungs.

(3) The physiological effects of holding the breath approximate asphyxia, whereas the effects of breathing slowly approximate fatigue.

(4) Favorable factors for holding the breath include (a) starting with full lungs, (b) diverting the attention from the breathing, (c) mental set, such as determination and confidence, and (d) forcibly expanding and contracting the thoracic and abdominal regions. Unfavorable factors include the converse of the above plus the factor of eating just before the practice session.

(5) Favorable methods reported for 'holding,' were to relax generally and to tense the abdominal region; and for 'slow breathing,' to smooth out and to prolong the respiratory activities.

APPARATUS

AN AUTOMATIC TECHNIQUE FOR ESTABLISHING CONDITIONED REFLEXES

ROGER BROWN LOUCKS,[1] Johns Hopkins University

A serious limitation of the usual conditioned-reflex techniques is the small number of animals which can be used by a single experimenter. To increase efficiency, a system has been developed in which the administering of stimuli and food and the recording of reactions are entirely automatic. Advantages of this procedure are: (a) that it offers the possibility of working simultaneously with several dogs (*i.e.* where two or more sound-shielded rooms are available); (b) that training which involves merely the repetition of stimuli will not necessitate the experimenter's concentrated attention but will leave him relatively free for other work; and (c) that, when the need arises, such training can be left to the supervision of a laboratory assistant.[2]

Motor-driven timer or circuit-closer. It is frequently desirable in conditioned-reflex investigations to utilize rhythmical stimuli, *e.g.* a flashing light, an intermittent tactual prick, or a metronome beat. In Pavlov's laboratory the first of these stimuli is produced manually by closing a switch at the desired rate; and the intermittent tactual stimulus is administered by pressing a bulb. Pneumatically started metronomes provide rhythmical auditory stimuli, separate instruments being used for different rates. In the present arrangement, all intermittent stimuli are controlled by one device, a motor-driven timer. A brass disk driven by an electric phonograph motor is studded with small rivets arranged in concentric circles.[3] A small rider can be set to trail over rivet-heads in any one of these circles. Each time the rider passes over a rivet-head it is lifted, thereby causing two flexible strips to touch. In a circle with 30 rivets, 30 contacts are made per revolution, and if a light is connected in the circuit it will flash 30 times. (See Fig. 1, B.)

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[1] National Research Fellow in the Biological Sciences. This technique has been worked out in the Conditioned Reflex Laboratory of the Henry Phipps Psychiatric Clinic, Johns Hopkins Medical School. The writer is greatly indebted to Dr. Adolf Meyer for extending the facilities necessary to carry on the project. He is also greatly indebted to Dr. W. Horsley Gantt for clarifying numerous questions regarding Pavlov's training procedures in the light of Dr. Gantt's intimate contact with the Russian work. Dr. Gantt, who established the Laboratory, has elsewhere described the sound-shielded room. The present paper should be read in conjunction with Dr. Gantt's more comprehensive description (forthcoming).

[2] In Pavlov's laboratory, work is carried on with the aid of a large number of medical students who train animals for credit toward a degree. Pavlov, or more accurately, one of his principal collaborators, takes these students in hand and sets them to work with one or two dogs on a specified problem. At the end of the training period, Pavlov comes and spends perhaps as much as a fortnight in personally verifying the results.

[3] A synchronous motor was not used because the city current was not at first telechronized.

Apparatus for administering tactual stimuli: "Thigmoton." Tactual stimuli are given by an electromagnetic-pneumatic apparatus at a rate controlled by the motor-timer described above. Closing the electrical circuit in the experimental room causes current to flow through a solenoid in a room on the floor below. When the plunger of the solenoid is actuated it causes the rubber dam of a tambour to be struck. The wave of compression resulting from this blow passes up a rubber hose to the sound-shielded experimental chamber. By opening the proper stop-cock valve, the pressure wave can be directed into any one of 7 mechanical prickers fastened to the dog's side. These consist of brads set in solder and glued to the rubber dam of a small tambour. The tambour is supported in an aluminum frame which is stuck on the dog's side by means of a low-temperature wax.

Methods of producing sound: (a) Clicks. The motor-timer is used in conjunction with a telegraph sounder to produce a variety of 'metronome' clicking rhythms.

(*b*) *Bubbles and hisses.* A magnetic valve is connected with a compressed-air line. When the electrical circuit is closed, the released air passes through a conduit into the sound-shielded chamber and audibly bubbles through water or hisses from the orifice of a pipe, according to the setting of stop-cock valves. When a bubbling noise is desired, the stop-cock valve leading to the bottle of water is manually opened before closing the magnetic-valve circuit.

(*c*) *Tones.* Tones are generated by a very inexpensive vacuum-tube dynatron-oscillator suggested by Lampkin's circuit.[4] The tuning inductances consist of a Western Electric 5B Auto-Transformer with 12 taps, which provides the middle range of frequencies, and a honeycomb coil #1250, which supplies the higher tones. Fixed condensers ranging in steps from 0.001 to 0.5 mfd can be switched into the circuit either singly or in combinations. Pitch is shifted by altering either the inductance or the capacity. Volume can be varied independently of pitch. Baldwin ear-phones are connected through a 500,000-ohm potentiometer to the output, one receiver being placed inside the sound-shielded chamber, the other near the experimenter. As now constructed, the oscillator has a range from approximately 32 d.v. to a value well above the upper limit for man. The higher tones appear satisfactory as regards freedom from harmonics; and it seems probable that, with a dynamic-type telephone-receiver to transmit the tone, the lower tones, which now carry an undesirable number of overtones, will be improved. Further details will be given as studies utilizing the tone-generator are published.

Food-box. The food-box now in use consists of 12 hinged shelves mounted one above the other in a chute. On a level with each shelf is an electric door-bell with the gong removed. The clapper arm or armature supports the shelf at its outer edge. When the coil of the bell is energized the armature is attracted and the shelf drops.

Tape-recording chronograph. A Gaertner 4-pen tape-chronograph, which marks on a waxlike-coated paper, is used for recording. It is imperative that no part of a record be illegible; and this instrument has proved singularly reliable. The range of speeds was shifted by adding a light in series with the motor. Clicking of pen-armatures was reduced to a minimum by placing rubber washers on the pole-pieces. One pen records the duration of the stimulus, a second indicates the point at which food is given, a third the drops of saliva secreted by the dog, and the last, the time in seconds.

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[4] G. F. Lampkin, A dual-frequency audio source for general laboratory use, *Electronics*, 1, 1930, 417.

Time-clock. When the frequency of the city current was telechronized a common electric house-clock was fitted up to replace the Harvard time-clock, notoriously erratic, which had been employed thus far. This device is very dependable; and the marker can be started by merely throwing a switch.[5] A brass gear with sixty teeth was turned down to 1/16-in. in thickness, and the hub removed. It was then fastened by sealing wax to the inner surface of the clock's glass cover. A coil-spring was soldered to the second-hand so as to touch lightly the teeth of the gear, thus making sixty contacts per revolution. The circuit in Fig. 3 indicates that at each contact the grid current of the vacuum tube is increased: the coincident increase in plate current actuates a relay. The Philco Relay, rewound with #27 wire to increase the impedence, can be omitted where it is feasible to rewind the marker.

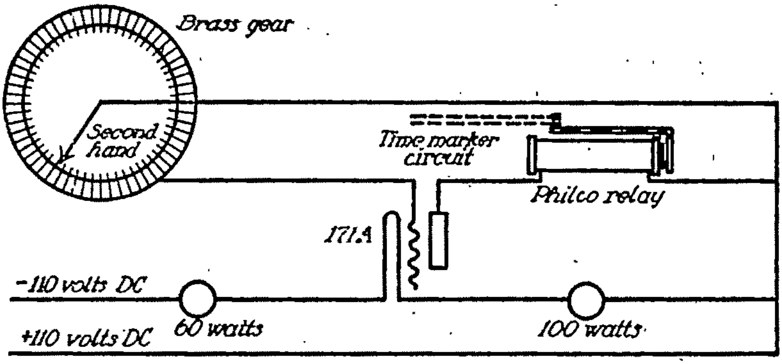

FIG. 3. CIRCUIT OF THE TIME-CLOCK

Drop-recorder. For the past eleven months, records of salivary secretion have been taken by means of a very simple but reliable vacuum-tube drop-recorder. This method of recording differs from that of Hannicke, in Pavlov's laboratory, in that no electrolyte is necessary and that drops are removed from the electrodes by surface-tension rather than by suction. Furthermore, in the Russian arrangement the conduit is filled with air as far as the electrolyte-bottle, whereas in the present system the pipe is entirely filled with water. Secretion of saliva displaces water in a conduit connecting the glass receptacle fastened over the dog's parotid fistula with a dropper outside the sound-shielded chamber. See description by Gantt (note 1).

Recently, Harris has described the vacuum-tube drop-recorders used by English investigators.[6] The open-grid circuit described in the present paper

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[5] This device is adaptable for large laboratory classes, the only modification necessary being the addition of a Philco Relay to each group of student desks. Philco Relays cost 75c, the vacuum tube an equal amount, and electric clocks can now be purchased for as little as $1.00.

[6] D. T. Harris, Thermonic drop-recorder, *J. Physiol.*, 71, 1931, xxii.

(Fig. 4) is particularly suitable where the dropping rate varies over a wide range, as is the case with conditioned secretions. Due to the fact that each drop decreases the plate current through the relay solenoid, thereby releasing the armature, the excursion can be very rapid and slight, and yet break the marker-circuit effectively. Since the grid bias is an all-or-none affair, the adjustment is not critical and does not require attention.

The orifice of the dropper is a glass pipe drawn to 0.977 mm. outside diameter. The upper electrode, a wire .202 mm. diam., is brought into the pipe through an opening several centimeters above the orifice, and extends down the capillary tube to the outlet. The lower electrode consists of a platinum wire so bent as to hold a strip of paper-toweling approximately 2 mm. wide and 15 cm. long. The paper provides a surface film of water which snaps away each drop at the instant of contact; and its lower end dips into a wide basin of water. The size of drop varies according to the distance of the lower electrode from the orifice, a drop of 0.00125 cu. cm. being accurately counted. Rates of over 1000 drops per min. have been reliably recorded.

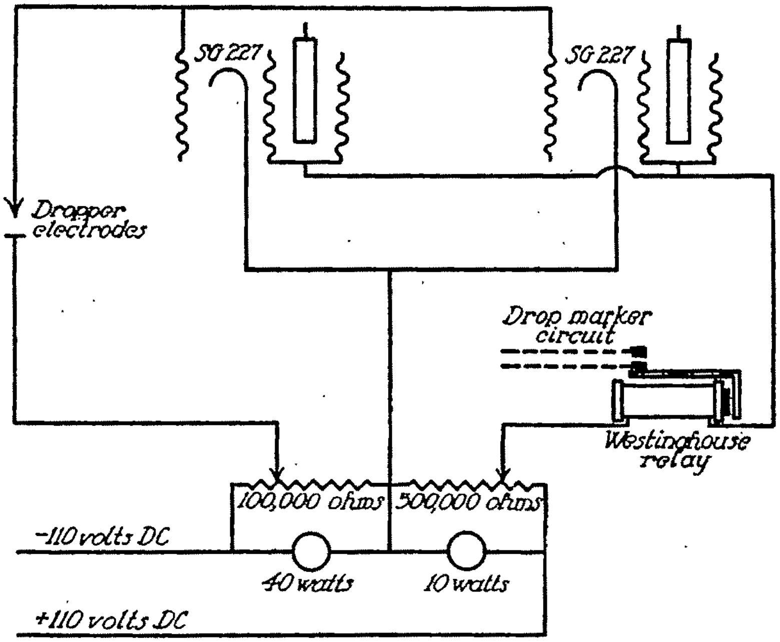

FIG. 4. CIRCUIT OF THE DROP-RECORDER

Master-control or robot. A robot takes the place of the experimenter in tripping switches that start and stop the tape-recording chronograph, administer stimuli and food. It will close or open a series of electrical circuits over a time-interval of approximately two hours. (The daily experimentation averages about one hour for each dog.) The switches can be adjusted so as to trip either simultaneously or within 5 sec. of each other, or at any greater interval on up to the full 2 hr. span. The apparatus contains 72 switches at present, although the number of circuits that can be controlled may be increased indefinitely. The various stimuli noted above may all be administered by the robot. The tape-recording chronograph starts before each stimulus and stops after each secretory reaction. A permanent record is thus obtained of the points at which the stimulus began and ended, the point at which the food was given, and the number of drops of conditioned secretion (see Fig. 2).

The arrangement is as follows. Twelve arms are mounted one above the other on a shaft, and turn freely within 12 respective hoops of slightly greater diameter. The steel hoops are fixed to a supporting frame-work and do not turn. Small toggle switches are so mounted upon the hoops that they may be slid along the circumference and fastened at any point by means of a set-screw. The arms are driven by a motor, through a reducer, and turn in cumulative sequence. As the bottom arm turns about within its respective hoop it trips the switches at the periphery. Upon the completion of one revolution it catches the arm next above, carrying this second arm around with it during the second revolution. In succession each arm catches the one directly above, the last added arm tripping the switches in its hoop. At the twelfth revolution all arms are turning as one, the top arm alone tripping switches.

General procedure. Mechanization of the training involves certain basic principles which may be briefly considered. In Russian studies of conditioned reflexes, there has been a marked tendency to emphasize the individuality of each dog. Pavlov looks upon his animals as exhibiting definite 'temperaments.' Accordingly, the training procedure to be used with one dog differs from that most adequate for another. Furthermore, considerable flexibility is exercised in the routine employed with one and the same dog. For example, on one day 4 stimuli or trials may be given, the next day, 12; on one day a flashing light, a bell, and a metronome may be administered, the next day, 4 tactual stimuli. In some instances two new stimuli may be introduced on the same day. The interval between the beginning of the stimulus and the giving of food is continually varied.

It does not follow, of course, that because rats can be trained in a rigidly fixed routine, such a procedure is the most adequate for dogs. And while the great majority of studies in the animal field strive to keep the number and nature of stimuli uniform, Pavlov has offered considerable evidence to show that by the constant repetition of one stimulus a dog may be ruined for further work. In consideration of these two opposing points of view, the writer has followed the principle of using more than one stimulus during the day's experiment, but of administering the stimuli in a fixed order. For example, although a particular experiment is primarily concerned with the tactual analyzer, two other stimuli would be included in the daily sequence as follows, bell, bubbling sound, tactual: bell, bubbling sound, tactual: bell, bubbling sound, tactual. (The significance of position and time-interval may be partialed out experimentally when it is so desired.) In the above instance the bell and bubbling sound are added in order to give the variety considered necessary to prevent the dampening out of the reaction to the tactual stimulus.

Because the modern work in animal behavior has emphasized the variability of reaction—and there is abundant evidence that marked fluctuations are commonly observed in Pavlov's dogs—it seems imperative that an 'experiment' involving quantitative values should include sufficient measurements to permit an estimation of the range of normal variability. The writer does not advocate continuing a stimulus in the event that it becomes inhibitory, merely for the sake of uniformity, but he would emphasize the desirability of keeping the training-order fixed over relatively long periods in order to obtain a fair sample of chance variations in the dog's secretions.

Summary

An automatic technique has been developed for establishing conditioned reflexes in dogs. A master-control device or robot administers the various stimuli and food, starts and stops a tape-chronograph which records the duration of the stimulus, the point at which food is given, and the drops of salivary secretion. Additional apparatus comprise the following pieces: Motor-driven circuit-closer, for timing intermittent stimuli; electromagnetic-pneumatic device for producing tactual stimuli; time-clock on house-current; electrically controlled food-box; and vacuum-tube drop-recorder.

AN INEXPENSIVE MEANS FOR TIMING OSCILLOGRAPH RECORDS

By Wilmer T. Bartholomew, Peabody Conservatory of Music

In the preparation of apparatus for an extended series of oscillograph records, the need of a reliable timing device presented itself.[1] In oscillograph practice, when it is necessary to determine with precision the time-relations of phenomena to be recorded, it is customary to pass an A.C. current of known and constant frequency through an auxiliary vibrator, photographing the resulting time-line simultaneously alongside of the record of the phenomena. For frequencies in the musical range, however, the usual commercial currents would have been of no value, since in order to separate the sound waves sufficiently for proper analysis the film speed has to be increased so much that a low frequency of say 60 cycles if used for a time-line would give so broad a crest in the photographic record as to be unreadable with precision. This is particularly true in narrow-width films, where the most of the available space is reserved for the sound record itself, and the time-line, if of low frequency, must therefore be very 'flat.' Recourse is then usually had to a special generator delivering 500-cycle current, or to some sort of vacuum-tube oscillator. These latter are very satisfactory when good, but if they are as accurate as we like them to be, they are rather expensive.

Many investigators use the electrically-driven tuning-fork, but these are neither accurate nor reliable, in our experience. Contacts heat up, and require constant adjustment, sometimes at crucial moments. More serious, this changes the frequency characteristics. Even the microphone-driven fork is not entirely free from these objections. Probably the most precise means known is the combination of a tuning fork and an oscillator in such manner that the fork controls the frequency of the oscillating circuit, and is itself maintained in vibration thereby. This, however, is expensive and complex, and a simpler method was developed as shown in Fig. 1:

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[1] From the Department of Research of Peabody Conservatory of Music, Baltimore, Maryland.

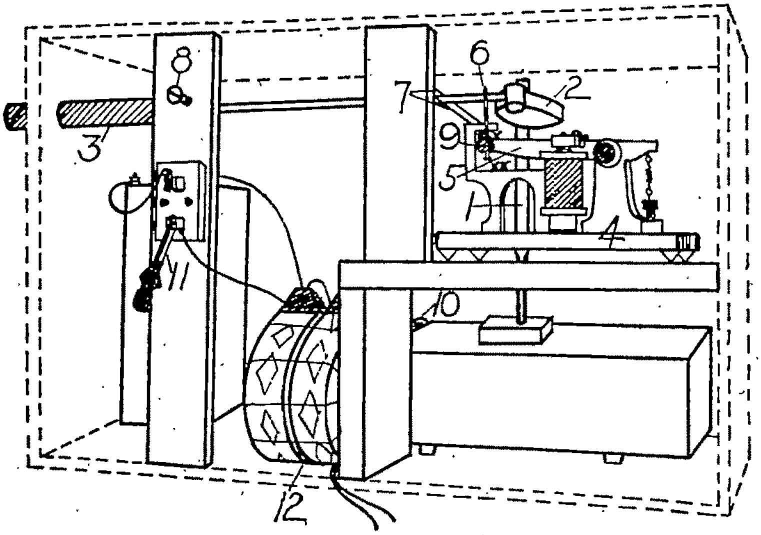

Fig. 1. Apparatus for Timing Oscillograph Records

A tuning fork (1) mounted on its resonator box, is placed so as to be struck by the felt hammer (2). The handle (3) of this hammer is rigidly secured in the wall of the box which contains the whole set-up. A telegraph sounder (4) has been modified and placed on a shelf so as to serve as a trigger to release this hammer. When the sounder-arm (5) is pushed down, the hammer can be bent back past the point of the nail (6). The sounder-arm can then be released, lifting the nail sufficiently to hold back the hammer-shank, which will bear against the nail because of the spring of the bamboo wood of which the shank is made. Obviously then, when the sounder-arm is depressed magnetically, the nail releases the hammer, which flies against the fork. To prevent movement of the hammer in the vertical plane, guides (7) have been provided. The 'orange-sticks' used by dentists proved satisfactory here, though any wood strip would do. Twine would doubtless be satisfactory here also. Wires were used at first, but proved unsatisfactory, because they were set into vibration themselves to a greater extent than the wood. A screw (8) bearing on the bamboo shank regulates the tension, and thus the strength of the tone. A set-screw (9), extending out from the sounder-arm, adjusts the height of the nail. Noise (which, if it reached the oscillograph microphone would invalidate the records) has been minimized by padding the striking points of the sounder-arm with felt, at the points marked X. In addition, the device (and in fact all of the recording apparatus except the microphone, amplifiers, and control switches) is located in a dark room soundproofed from the studio where the singer or player performs.

The sound of the fork is picked up by an ordinary telephone transmitter (10) placed at the opening of the resonator-box, and concealed from view in the figure by the coils. The current from one dry cell, controlled by the knife switch (11), passes through this transmitter and through the primary of a small transformer (12). The transformer is necessary in order to keep D.C. out of the oscillograph vibrator. The secondary of this transformer is connected directly to the time-recording vibrator of the oscillograph. Various current strengths and transformer ratios may have to be tried before the proper amplitude be secured in the timing-wave. The vibrator used for this purpose in our set-up is a high-sensitive one, a product of the Westinghouse Company, requiring only 8 milliamperes per inch deflection on the film. Thus it is necessary to use only one dry cell, even when the tuning fork is struck fairly softly. It is advantageous to strike the fork lightly, as a greater constancy of vibration is secured. Our transformer was easily constructed by tying together two of the 'honeycomb' coils once popular in radio sets, although considerable trial-and-error was necessary to find the proper combination of sizes to give the best amplitude. A one-to-one ratio, using coils "L-200" or "L-300" was found to give best results.

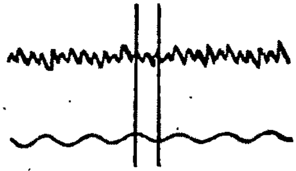
FIG. 2. TYPE OF RECORD OBTAINED

This device can be operated in any situation where an electric impulse is available to trip the hammer. In the set-up in use at Peabody at the present time, a long belt of light material, with photographic recording paper fastened thereon, is driven past the oscillograph window. By means of a remote control switch in the studio, at the time when the record is to be made, a wire on this belt completes momentarily a circuit across two brushes sliding thereon, and this circuit by means of a relay lights the oscillograph lamp, starting the exposure. Another relay breaks the lamp circuit one belt-revolution later. By inserting the telegraph sounder that releases the tuning fork hammer in the circuit of relay No. 1, the time-line is started automatically whenever a record is started.

Fig. 2 illustrates the type of record obtainable. The complex wave here is of a violin tone, while the sinusoidal curve points off the particular frequency of the fork used. If a 500 dv. fork is used, the time-line will measure thousandths of a second from crest to trough.

The method has the following advantages. (1) Tuning forks vibrating freely at small amplitudes are extremely constant in frequency as compared with contact-driven or even microphone-driven forks. (2) The device is inexpensive. It was made entirely of odds and ends in our laboratory. Also, cost of operation is practically nil. Being simple, adjustments are few and easy. (3) Forks of any desired frequency may easily be substituted, giving any desired time-line. (4) The usefulness of the set-up is not restricted to the measuring of sound-wave records, for it can be used as a very accurate chronograph to measure the time-relations of any phenomena which can be recorded electrically —reaction-times, for example.

A DEMONSTRATIONAL COLOR MIXER

By G. L. Freeman, Northwestern University

There is one outstanding disadvantage in the use of rotating disks for color mixing; that is the confusion which often results when the student attempts to visualise the laws of mixture in terms of the color pyramid. A possible means of overcoming this would be to mix the colors directly upon a model of the pyramid. The following device indicates the feasibility of such a plan.

Fig. 1 shows an apparatus whose general form resembles the base of the color pyramid. The face of the 36 in. sq. frame is covered with black velvet, and the four terminal or primary colors are painted on the tin reflectors in each corner. Each reflector carries a 100 watt electric light bulb with an appropriate yellow, green, red, and blue glass filter. A hinged cover in front of each filter makes it possible to shut off the illumination from any lamp without switching off the current. The frame is mounted in an upright position so as to face the audience. It can also be revolved about the axis at its center.

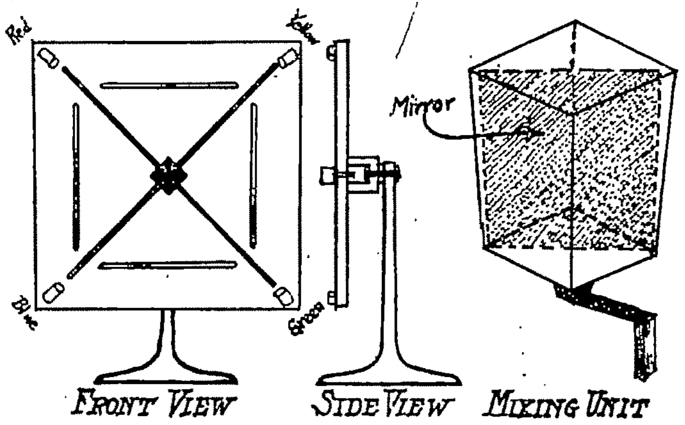

FIG. 1. A DEMONSTRATIONAL COLOR MIXER

So far, the apparatus resembles the traditional frame for the demonstration of shadow contrast.[1] The innovation comes with the introduction of a mixing-unit. This consists of a square glass box with a frosted top (i.e. the plane exposed to the audience)[2] in which a mirror is placed at a slight angle so as to reflect the light outwards. The mixing-unit may be used at the center of the frame or at various other positions, as slits for the holder of the mixing-unit have been cut, as Fig. 1 shows, along the diagonals and parallel to the edges of the frame.

The following demonstrations may be made with this apparatus.

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[1] Cf. E. B. Titchener, *Experimental Psychology, Qualitative, Instructor's Manual,* 1901, 36; and E. C. Sanford, *Experimental Psychology,* 1894, 155.

[2] We used a square, frosted lamp with a ground glass plate fastened to the top.

(*1*) *Base of the color pyramid.* Take off the mixing unit and place a sheet of white cardboard over the entire frame. The lights reflected by this surface will fuse in a manner which suggests how the base of the pyramid would look.

(*2*) *Mixture of antagonistic colors.* (a) With the white cardboard removed and with the yellow and blue lights covered, move the mixing unit diagonally across the board between the green and red light-sources. Viewed from the front, the ground glass plate of the mixer will be seen gradually to lose in saturation of green, to become gray, and then to gain in saturation of red. The procedure may be repeated for yellow and blue light, using the other diagonal of the frame. It should be noted that in this demonstration it is necessary to keep the mirror of the mixing unit in line with the direction of movement: otherwise it will present an obstruction for the mixture of transmitted light. (b) Place the mixing unit at the center of the frame and leave all four lights uncovered. The ground glass plate appears gray. When each light is now covered in turn, the antagonistic color will show up in the mixing unit.

(*3*) *Intermediate color mixtures.* (a) Place the mixing unit in one of the slits parallel to the edge of the frame. Cover all lights except those adjacent to this line. Turn the mixing unit so that the plane mirror faces the sources of light. Move the mixing unit along the slit between the two lights, for example, blue and red. The audience will note the changes in that half of the ground glass plate which is illuminated by the reflected light from the mirror. The procedure may be repeated for other color-combinations. (b) Place the mixing unit in the center of the frame. Leave all lights uncovered. Keep the position of the mixing unit constant and slowly revolve the frame about it. The audience will note the changes in the color of the mixer as the various lights are reflected by the mirror. It may be explained that this demonstration is essentially the same as if one moved the mixing unit around the base of the color pyramid.

(*4*) *Simultaneous contrast.* This demonstration is much like that which is usually given with shadows. All lights are covered except one. The mixing unit is placed at the center of the frame so that the back of the mirror intercepts the light. This shadow is cast upon a piece of cardboard held behind the mixing unit. The audience will note the changes in the color of the shadow under different illuminations.

The pedagogical advantages of these demonstrations are obvious. With regard to their practicality, we can report that we have used the mixer with success even in partially darkened rooms. The cost of construction is minimal.[3] The chief difficulties come in the selection of appropriate filter glasses. A great deal of trial and error is necessary here in order to produce fairly well saturated light. We have found that a combination of colored glass and gelatin paper is satisfactory. For example, if the blue glass transmits a purplish light, use a sheet of green gelatin tissue to cancel the effect. Yellow usually presents the greatest problem and it may be found necessary to cut down the illumination of this lamp in order to equate intensities. Sliding rheostats might be introduced for all lights.

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[3] We used tin coffee cans as reflectors.

A DEVICE FOR DEMONSTRATING APPARENT VISUAL MOTION

By Georgene H. Seward, Barnard College

The device described here provides a simple and inexpensive method of demonstrating the various types of apparent visual movement and the factors affecting them.[1] It is suitable for either individual or group observations, and is designed to form a permanent part of the laboratory equipment so that demonstrations may be made by different experimenters and at different times under constant conditions. The apparatus consists of three parts: a large wooden screen, a light source, and a timing mechanism. The screen as is shown in Fig. 1, is hinged to side supports so as to operate like a door. In the center of this screen there is an aperture 9 by 18 cm. The inner side of this opening is fitted with a card-holder for the demonstration slides. Fig. 1 shows a slide (A), with two

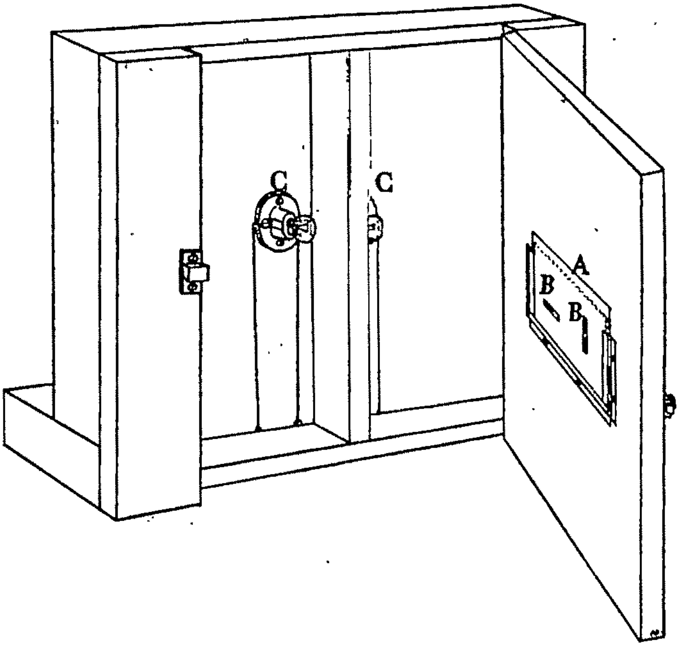

Fig. 1. Diagram of Demonstrational Apparatus for Apparent Visual Motion

Demonstration card (A) with two rectangular windows (B) is indicated in the card-holder. The lights (C), separated by a partition, are seen mounted upon a backboard. Wires are shown extending from the lamps through openings at the base of the backboard to connect with the batteries and metronome.

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[1] The writer wishes to acknowledge her indebtedness to Professor H. H. Keith of Hunter College and to Dr. J. P. Seward, Jr., of Columbia University, for helpful suggestions given her during the construction of this apparatus.

rectangular windows (B)—one horizontal and one perpendicular. Two 2.5 volt bulbs, (C), mounted upon a backboard directly behind the windows, constitute the light sources. Uniformity of the illumination of the stimulus-fields and the obscuring of outlines is attained by placing a piece of flashed opal glass behind the slide. The device is operated in the Barnard Laboratory by means of a mercury-cup metronome, but any timing apparatus which alternates the current to the two lights will suffice.

A partition between the bulbs prevents diffusion of light from one window to the other, while side and top boards prevent general diffusion. The outside of the apparatus is painted black. For the best effects the demonstrations should be made in a darkened room. Under these conditions 'movement' appears without the distraction involved in the actual movement of any part of the apparatus. This device, since it works automatically, eliminates the experimenter's reaction-time, and leaves him free to give whatever suggestions or explanations he finds necessary and desirable.

We may clearly demonstrate the various 'movement' types by using different slides. By varying the rate of the metronome, the effect of changing the temporal interval between successive flashes of light may be shown. Differences in intensity of illumination may be studied by increasing the voltage or using larger lamps. We may show the effect of hue by covering the flashed opal glass with colored gelatine filters. It is impossible with the present set-up, since the distance between the lights is fixed, to demonstrate the effects of spatial separation. This limitation, however, might easily be overcome by mounting the lamps upon small blocks of wood fitting into grooves along which they could be moved to coincide with windows placed at varying distances apart. If the above suggestions are followed this new apparatus will provide convincing laboratory demonstrations of the principal factors involved in apparent visual motion.

NOTES AND DISCUSSIONS

INSIGHT

The term under discussion is one the common meaning of which is understood by all. "Sight into: thorough knowledge or skill: power of acute observation;" these are the definitions given in a small dictionary. But can psychology use such a term, with such meanings, in what it chooses to call a scientific context?

Köhler, who did use the word "insight" in his description of the mentality of apes, was drawing a sharp contrast between certain performances of these animals and the behavior reported by other investigators of animals in mazes and puzzle-boxes. Köhler found that his apes could discover a way of doing something quite suddenly and without a previous period of 'trial and error.' In a simple instance, an ape uses a stick for the first time to get a banana lying beyond its reach. Not only did the chimpanzee use the stick, which he then saw stretching in his line of vision towards the desired fruit, he used it appropriately, and he retained thereafter a special interest in sticks and other longish things in all similar situations.

This relationship between the stick and the fruit Köhler calls "a material inner, relation, . . . an interconnection based on the properties of these things themselves, not a 'frequent following each other' or 'recurring together.' "[1]

Köhler at least tried to give an unprejudiced description of the observable facts; but he went farther, and challenged us to account for this behavior strictly in terms of the principle of association.

In his more recent book—*Gestalt Psychology*—Köhler writes as follows:

> When I used this term in my treatment of intelligent behavior in apes, I ran the risk of an unfortunate misinterpretation. Since that behavior comprised extraordinarily conspicuous accomplishments which we did not expect to find in animals, insight would be readily misinterpreted as some special and supernatural faculty producing admirable and otherwise inexplicable results. As I used and intended the term, nothing of that sort should be implied in it. To avoid a similar misunderstanding this time, I have introduced the concept here in application to very common and simple facts.[2]

These "common and simple facts" are just those which distinguish "thorough knowledge" and "acute observation" from insufficient knowledge and vague or confused observation. But here many psychologists will object that "common and simple facts" lack a scientific status; indeed, that they are not facts at all, but meanings which, while they may serve well enough in a dictionary, cannot properly be counted as existential data either of consciousness or of behavior.

At this very point the Gestalt-theory parts company both with introspective existentialism and with a so-called objective behaviorism. I permit myself the use of the derogatory "so-called" in referring to behaviorism because, in my opinion, Gestalt-theory has more to offer at present to behaviorism than it has to Titchenerian existentialism.

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[1] W. Köhler, *The Mentality of Apes*, 1925, 219.

[2] *Op. cit.*, 1929, 371.

To consider Titchener's position first, I find nothing in it to which one can seriously object, if one is willing to accept Titchener's premises.

In the last analysis the validity of any set of premises is pragmatic. In Titchener's case the pragmatic validity of his postulates is to be found in his desire for a "pure" science of experience as such. I do not see how any fault can be found with a systematic attempt to study experience at its face value, or to classify it in accordance with its observable attributes or properties. Whether such an attempt will prove successful is another question—a question to which a negative answer has been given by all other schools of psychology.

A psychology of existential experience does not become a science simply because its theory is valid, but only when its data are acceptable as the pivotal facts of a system. It is well-known that these data are not acceptable to the behaviorist. If they were, he would have no just grounds for attacking their validity. "Logically," wrote Titchener, "a strict behaviorism can have no quarrel with an existential psychology, since there is no point of contact between the two disciplines."

Yet others beside behaviorists have attacked the validity of existential experience. Since Brentano's time the functionalists have insisted that the data of experience are but indications of the way in which minds work. The introspective observations of the Würzburg school, in their turn, revealed data which were not describable as existential contents; and finally the Gestalt-psychologists have shown that the attribution of uniformity to existential contents is untrustworthy, since it rests, not upon the actual contents of experience, but upon a figure-ground pattern which shifts markedly with the total behavior of the observer.

The insightless, meaningless, facts of existential psychology stand or fall with the possibility of demonstrating their existentiality. Logically one can make out a perfectly good case for their putative existence. But while logic can predict conscious contents, observation alone can demonstrate their existence as pivotal facts.

If we turn now to behaviorism, it is not so easy to say what this discipline can do with "insight, thorough knowledge or skill: power of acute observation." There are too many behaviorists to compare with the single Titchener, and their views obviously lack the consistency of a single highly gifted individual.[3] As for the views of the father of the movement, John B. Watson, we should agree, I think, that they have become a little warped by the method which this brilliant idealist has adopted in their presentation. I am also doubtful if Watson would seriously defend in private certain of his more striking public utterances.

If I must fall back upon my own notion of a behaviorist to answer my question what use he has for the concept of insight, I do so with the knowledge that no person is likely to step forward and identify himself with my picture. Since, however, my aim is to make real behaviorists of those who accept the name, I shall perhaps be able to escape the charge of setting up a straw man whom I can easily knock down.

My behaviorist, then, is as yet unaware of the use to which insight can and should be put in his system, and this, primarily, because he has not weaned himself away from the existentialism of the Titchenerian doctrine. My behaviorist began his career by revolting against two things (1) the contents of consciousness and (2) the teleology of functionalism. In these respects he has much in common with Watson because the two systems of psychology with which Watson was best acquainted were those of Titchener and Angell.

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[3] Cf. K. A. Williams, Five behaviorisms, this JOURNAL, 43, 1931, 337 f.

My behaviorist has rid his doctrine of conscious states which can be described and ordered in terms of their own properties. Instead, he proposes to study what man and other animals do, and why they do it, in terms of their situations and responses. But although he has meant to reject the more important data of the Titchenerian system, he has only translated them to the biological level upon which Titchener, himself, made all conscious contents depend.

Titchener has described the major sciences in the following way:

> Psychology is the science of existential experience regarded as functionally or logically dependent upon the nervous system (or its biological equivalent); biology is the science of existential experience regarded as functionally or logically dependent upon the physical environment; and physics (including chemistry and physical chemistry) is the science of existential experience regarded as functionally or logically interdependent.[4]

My behaviorist rejects the first clause of this statement, and proceeds to differentiate his psychology from other biological disciplines by asserting for behaviorism a very special, if by no means unique, interest in the organism as a whole. While "existential experience regarded as functionally or logically dependent upon the nervous system" is superfluous to him, "existential experience regarded as functionally or logically dependent upon the physical environment" is his chief concern. The experience with which he deals is that of measurable changes or movements of the organism correlated with equally measurable changes or movements in the physical environment. Both the situations and responses are physical data, observed or experienced, and therefore quite as positivistic as Titchener's description requires them to be.

My behaviorist understands this epistemological implication. He knows that he is dealing only with the data of experience, mine or yours, recorded and measured by this or that instrument of precision. I am not sure that all behaviorists are so acute. Some of them talk as though they could observe data without the mediating agency of experience. But at least they all know, I take it, that they are not dealing with experience as an existential quality for itself. In this respect their revolt against the Titchenerian position is complete.

In confining himself to positive data gained through the medium of experience, my behaviorist is limited to the correlation of what he observes of the situation that confronts his experimental animal, and what he observes of the animal's response. Thus he limits himself unduly, and thereby destroys his concept of behavior as the total-response of the organism to its situation. He limits himself unduly because his fear of becoming introspective leads him to ignore what is taking place between the stimulation and the response. His method therefore, reduces itself to a kind of statistical correlation of situations and their prepotent stimuli, with responses and their specific muscular contractions and glandular secretions.

But behavior, both as the behaviorist would have it, and as the biologist finds it, is no statistical construct but a fundamental fact of organic life. With respect to development, the creature's special organs are more a product of its behavior than a set of original parts which when assembled begin to behave. If we thus take behavior seriously, not as something to be explained in terms of something else, but as the fundamental premise of our science, it follows that "material inner relations" based on the properties of behavior itself will bridge the gap which otherwise appears to exist between situation and response.

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[4] E. B. Titchener, *Systematic Psychology: Prolegomena*, 1929, 32 f.

This bridging the gap requires another description of behavior than that which has usually been employed in our science. In Köhler's words:

> It is the layman's belief that, in general, he himself directly feels *why* at one time he has one attitude, and later on another; also that, for the most part, he knows and understands directly *why* he is inclined to do one thing in a certain particular situation and *why* a definitely different thing under subsequent different conditions. In his view, then, he is experiencing directly and truly much of that dynamical context, the development of which constitutes mental life. Opposed to this belief and altogether foreign to it, we have the view of most learned psychologists at the present time. From their viewpoint, one is inclined to do one thing now and then another, because, in the first instance, certain nerve-paths are most available and, in the second instance, certain other paths are most open. Fortunate those people in whom the most permeable nerve-paths in practice are usually the right and appropriate ones! From the psychologist's point of view it would be a pure mystery if a person's behavior should ever be determined, as the layman believes, directly by the concrete properties of the actual situation. The layman's belief is in full agreement with everyday experience; the learned view harmonizes with what we suppose to be the viewpoint of natural science. I shall confess at once that I choose the layman's belief whole-heartedly.[5]

But in choosing the layman's belief it is necessary to do more than speak the word insight. How Köhler justifies the use of the term can be learned from the last chapter of his book, from which my quotation is taken. In our present context, I wish to consider the validity of the layman's belief that he knows, understands, in short has *insight* into what he is about.

No one doubts, I suppose, that there are intelligent and unintelligent forms of behavior, and that insight as the layman uses the term belongs to intelligence and not to stupidity. But we cannot say that insight is given whenever "things possess an inner material relation" . . . "based on the properties of these things themselves."

There are two points to the argument. The first is that "inner material relations" do exist in behavior, that they are of its very structure, and that they are not to be confused with a logical distinction of terms *and* relations, which would make the terms primary, and their relations secondary adjuncts. The premise we urge our behaviorist to adopt is that behavior is a going concern, a dynamic actuality, in which the organism incorporates its situation in its response, and likewise models its response in the likeness of its situation. In a word, the behaviorist should deal, not with situations and responses, but with *situation-responses*; for the one has no meaning without the other, whereas together they provide the fundamental meaning of everything the organism is and does.

The second point is that these material interconnections of the properties of behavior, while they constitute the grounds for intelligence and insight, do not reveal themselves as such until the organism is able to 'grasp' them. Now grasping requires one who grasps—one who knows and understands what he is about.

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[5] *Gestalt Psychology*, 1929, 349-50.

In the case of Köhler's ape insight is inferred from the manner in which the animal seizes the stick and reaches for the banana. It is not otherwise in man. But not all of man's behavior shows insight, any more than does the ape's. In neither case can the distinction be clearly made in terms of 'good' or 'bad' performance as determined by some objective standard of comparison. The criterion which the layman employs, and which Köhler so whole-heartedly accepts, is the way one 'feels' about it. This would seem to put us back into the realm of consciousness from which the behaviorist has escaped. However, it is not the existential experience of this feeling that matters, but the indications thus given of one's own or another's behavior. This use of experience as a means of determining the nature of something other than experience is in no wise different in principle from what is called 'objective' observation. It is not the feeling itself that makes insight, but the feeling that this concrete thing belongs to that concrete thing in the process of behavior. Feeling, in other words, is a mode of observation which reveals insight whenever the observed subject, be it myself or any other animal, behaves with an appropriate use of means to ends. We call such behavior 'insightful' because in our own case we 'see into' what we are about as we are about it. This means a complication of the pattern of behavior as compared with impulsive or chance combinations of means to ends. The complication involves a differentiation and a clarification of the course of behavior with reference to its beginning, its means, and its end. This differentiation is not prescribed by three things originally indifferent to each other—a hungry ape, a stick, and a banana; it is an achievement that may take place in a course of behavior which began with an impulse of desire for the banana. We do not know precisely when the ape begins to exercise insight in gaining what he wants. Neither do we know when we ourselves began to be intelligent. But we do know the difference between vague groping, with or without chance-success, and the phenomenon: "Eureka, I have found it!"

There must be a difference in these two kinds of behavior. 'Insight' names one of them. While it does not explain it to name it, it does state a problem which my behaviorist, who confines himself to situation and response as quite separate terms, cannot even state.

I have stated it for him in the hope that he will see the desirability of enlarging the scope of his inquiries by adopting as his premise a behavior which to begin with is an integral, self-regulating, process, to be studied in terms of its developmental aspects and properties, and not in terms of a pre-arranged framework of nerves which are excited by an entirely alien set of physical stimuli.

If I have made my points clear, it will be evident that all behavior is integral with reference to the formal course it takes. Its beginning, means, and end are formal aspects of a materially unified situation-response. Only by a differentiation of its aspects does the behavior warrant the attribution of insight which the dictionary calls "thorough knowledge and skill" or "power of acute observation;" because these attributes require distinctions on the part of the organism which clarify its behavior by setting the creature off from its tools and objects. Insight implies neither introspection nor telelogy, the two things against which behaviorism has revolted. But it does imply a difference between intelligent and unintelligent behavior, even though it draws no sharp line of differentiation between them.

With the postulate that behavior is always in some degree integrated, it is easy to find intelligence growing with a natural process of differentiation which provides new means of gaining ends. It is also easy to define the degree of insight by negative comparisons, where we can observe in others, or in ourselves (after the event), possibilities which are not, or were not, utilized. As Koffka says, "we call a behaviour stupid when a performance, all the conditions for which are given, is not carried out."[6] When the conditions are not all given, the differentiations requisite for insight are simply not made. But the resultant behavior, though it lacks insight, is not necessarily stupid. On the contrary, the behavior of lower forms of organisms, and the behavior of our own internal organs of digestion, circulation, respiration, and the like, are essentially 'wise' to the adjustments which they make in and with their respective situations.

Whether or not we choose to call this blind wisdom of behavior an evidence of intelligence is a matter of definition; but I should suppose that intelligence was somehow connected with the fundamental capacity of organisms to embrace situation-responses in meaningful ways.

Insight, however, is a term for noting an intelligent behavior in which the 'why' is registered by an appropriate discrimination of desire, means, and end: that is to say, appropriate in the only way in which any thing is 'realistically' or 'objectively' appropriate. For the use of the term 'insight' does not imply an idealistic philosophy, though it can be made consonant with such a view. Neither does it imply a purposive philosophy by its descriptive employment of such terms as means and ends. But it does imply that behavior is intrinsically meaningful, and that its data are neither the positive existential experiences which Titchener postulated, nor the independently existent parts of a machine.

It was my original intention to discuss a recent article by Mr. George W. Hartmann on *The Concept and Criteria of Insight.*[7] I have refrained from doing so in the body of my paper because Mr. Hartmann's concept and criteria seem to fall outside the context of my discussion. At the risk of an anticlimax, I shall make my comment on Mr. Hartmann's views in the form of an appendix.

Although Mr. Hartmann signs his article from the source and origin of Gestalt-Theory, the University of Berlin, I doubt if his statement of the case for and against insight would meet with the approval of the Berlin Gestalt-psychologists.

Mr. Hartmann's approach to the subject is that of definition and terminology. His "primary task," he tells us, "is to attempt, by means of a comparative and historical analysis, a reduction of the term to the level of a 'clear and simple idea.'"[8]

In the course of his article the author charges me with mixing badly the technical and colloquial uses of the term insight in my book: "Psychology and Education." I make no defense, because he is right. However, his insistence that insight can, or should, be used as a "clear and simple idea" strikes me as being questionable. Insight, as I have tried to show in the earlier part of this paper, states a problem rather than a solution. But the very statement of the problem rules out certain kinds of solution which Mr. Hartmann wishes us to test experimentally.

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[6] Psychologie, in *Lehrbuch der Philosophie*, Max Dessoir, Ed., 1925, 528.
[7] *Op. cit., Psychol. Rev.*, 38, 1931, 242 f.
[8] *Ibid.*, 242.

Mr. Hartmann's paper ends with ten "preliminary questions" which, he says, "must be settled before we can attain a satisfactory theory of insight."

I venture to quote and make reply to his questions without recourse to the "extensive program of research" which, he adds, "will be necessary before even the simplest of these problems can be answered."

Question 1: "Are specific insights the outcomes of prior trial-and-error?" *Answer:* In the context of Gestalt-theory there is no trial-and-error.

Question 2: "Does all learning require some insight, even of a rudimentary sort?" *Answer:* You must first define what you mean by learning.

Question 3: "What is the best single criterion (or group of criteria) of insight?" *Answer:* The observation that the behavior in question discriminates means and ends. I 'feel' my own discriminations, and I infer those of others.

Question 4: "Is insight necessarily accompanied by ideas? If so, what is their character?" *Answer:* In the context of Gestalt-theory ideas have no special place as a class of entities which can be said to accompany insight.

Question 5: "Is insight an active production of the organism, or does it arise spontaneously?" *Answer:* Both. The dilemma is not rigorous.

Question 6: "Is the moment of insight emotionally-toned? What is the nature of this coloring?" *Answer:* These questions imply introspective data which are irrelevant to a bare statement of insight as a special kind of behavior. The organism is undeniably 'moved' when it indicates insight; it may also be 'moved' without insight.

Question 7: "Can insight be identified with association by similarity?" *Answer:* Not in the context of Gestalt-theory.

Question 8: "Is insight a species of the genus intelligence, or vice versa?" *Answer:* It is a species of intelligent behavior.

Question 9: "How is insight related to inference and induction?" *Answer:* As types of behavior inference and induction are species of insight.

Question 10: "What is the connection between closure and insight?" *Answer:* All insight implies closure, but not all closure is effected with insight.

My answers are not all very significant, nor, perhaps, in every case sound, because Mr. Hartmann's questions are not framed as a Gestalt-psychologist would frame them. A functionalist or a positivist—either experiential or mechanistic—would probably answer these questions differently. The point is that such questions cannot properly be asked outside the framework of a definite set of postulates; and when they are asked the framework will suggest most of the answers as a necessary consequence of the postulates.

There is plenty of experimental research to be done before we can know all that insight implies, and whether or not we can give it the strict definition Mr. Hartmann desires, but I confess that my insight does not tell me how these ten questions can be translated into fruitful experiments.

Cornell University R. M. OGDEN

THE METHOD OF GESTALT PSYCHOLOGY

"To understand Gestalt psychology one must understand its procedure, *how* its hypotheses are made, how they are translated into experiments which decide for or against them."[1] Thus speaks Koffka, one of the leading exponents of the

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[1] K. Koffka. Some problems of space perception, *Psychologies of 1930*, 1930, 161-187. Further citations will be to this volume.

Gestalt theory. This statement itself reveals one of the outstanding characteristics of the method of Gestalt psychology. The hypothesis is made first and afterwards experiments are conducted to furnish the data. A sense of fairness demands that it be kept in mind that the evidence may be either for or against the hypothesis. Nevertheless, this method puts the user of it in the position of seeking proof, even when the evidence proves his hypothesis false.

The criticism of Koffka's method is not directed against a search for data to prove or disprove a theory, but rather against his use of an assumption, unsupported by adequate experimentation, as an hypothesis. A mere assumption in harmony with a general theory is not an hypothesis, no matter how convincing may be the arguments in favor of it. Hypotheses are established not by argument and illustration, but by careful experimentation. The truth which advocates of Gestalt will eventually contribute to the science of psychology will have to survive in spite of their procedure. A casual reading of Köhler's discussion of "Some Tasks of Gestalt Psychology" will make this clear to the unprejudiced reader.[1] A better example of the fallacy of this principle is found in Koffka's treatment of "Some Problems of Space Perception."[2]

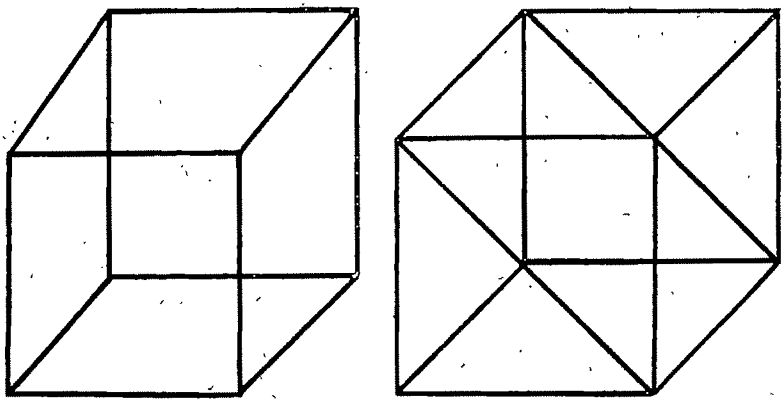

FIG. 1. KOFFKA'S FIGS. 1 AND 4
(a) designed to illustrate a solid, (b) a plane

In his discussion, configuration or Gestalt is accepted by hypothesis. The writer then proceeds to prove the truth of the hypothesis by arguments based upon the visual perception of certain geometrical figures. The two figures on which the main argument depends are the Necker cube, (a) in Fig. 1, and a second figure, designated by Koffka as Fig. 4, (b) in Fig. 1, which is produced by adding two lines to the first figure.

Neither the statement of the hypothesis nor a detailed discussion of the argument is of significance with reference to the point at issue here, namely the method of procedure. Only so much of the argument as is necessary to make clear the fallacy of the method will be given.

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[1] W. Köhler, *idem.*, 143-160.

[2] Koffka, *op. cit.*

Speaking of Fig. 1 (a), Koffka says: "This drawing appears to everyone as a cube, *i.e.* as a tridimensional shape notwithstanding the fact that in reality it possesses only two dimensions."[4] After discussing two other figures, omitted here, he continues: "Finally Figure 4, [our Fig. 1 (b)] which has been produced from Figure 1 by the addition of two lines, will again appear as bidimensional." ... "None of these appearances is absolutely compulsory, but doubtless they are the spontaneous ones."[5] Koffka further argues that it is difficult to see Fig. 1 (b) in three dimensions.

Then follows an explanation and interpretation of these spontaneous perceptions in terms of Gestalt. In attacking the experience hypothesis as an explanation of these alleged spontaneous perceptions, Koffka says: "If the reader admits that Fig. 1 appears spontaneously as three-dimensional and Fig. 4 as two-dimensional, he would have to explain this by pointing out that Fig. 1 as a stimulus has more in common with a stimulus which in the past has aroused the experience of a cube than the second. Can such a statement be validated?"[6] This is followed by more argument in support of the organization hypothesis with more geometrical figures to support this argument. But to leave argument and to seek facts; what data are back of the assertion that "doubtless" Fig. 1 (a) is spontaneously perceived as three-dimensional, and Fig. 1 (b) as two-dimensional?

In order to find out how these figures are spontaneously perceived, the writer, with the aid of one of his students, carried on the investigation described below.[7] The printed pages on which the Figs. 1 and 4 appear in "The Psychologies of 1930" were covered with sheets of white paper. Holes were cut in these sheets so that each figure was plainly visible on a white background, as all printed matter and other geometrical figures were concealed. Beginning with the first grade these figures were shown to white children in every grade up through the senior high school. Then a sampling was made of negro children in the second, third, seventh and eighth grades. The figures were also shown to college students and to members of the college faculty.

Care was taken to show each figure first to half of the *Ss*. As a figure was shown, the experimenter asked the questions, "What is this a picture of? What does this look like?" Records were kept by grade, age, sex, and race; but the results showed that there were no significant differences due to any of these factors. In the lower grades, each figure was usually called a box. Occasionally Fig. 1 (b) was called a kite, but when asked, "What kind of a kite?" the answer was, "A box kite." In the upper grades the figures were generally called cubes, but frequently they were called boxes. One college freshman, more "spontaneous" then others, perhaps, called Fig. 1 (a) a "hexalogan." When questioned further she said that it was a solid. Some called Fig. 1 (b) a diamond or cut stone. Care was taken to ascertain exactly what the terms used to describe the figures meant to those using them. "Diamond," for example, was not scored as tridimensional unless the subject gave unmistakable evidence that a solid was meant.

Table I shows how these figures were "spontaneously" perceived by the different groups.

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[4] *Ibid.*, 163. [5] *Ibid.*, 164 f. [6] *Ibid.*, 166.

[7] Miss Carol Jane Jones conducted the experiment in the public schools of Chickasha, Oklahoma.

TABLE I

"Spontaneous" Visual Perception of Koffka's Figs. 1 and 4

			Perceived as			
			bidimensional		tridimensional	
Group	Grade	No. Tested	Fig. 1	Fig. 4	Fig. 1	Fig. 4
White:						
	1	38	1	3	37	35
	2	48	2	8	46	40
	3	49	0	5	49	44
	4	68	0	1	68	67
	5	73	0	1	73	72
	6	24	0	0	24	24
	7	28	0	2	28	26
	8	18	0	0	18	18
	9	26	0	0	26	26
	10	16	0	2	16	14
	11	16	0	1	16	15
	12	27	0	1	27	26
Negro:						
	2	10	0	0	10	10
	3	29	0	0	29	29
	7 and 8	60	1	8	59	52
College:						
Students		91	4	11	87	80
Faculty		40	1	3	39	37
Total		661	9	46	652	615

TABLE II

Recapitulation of "Spontaneous" Visual Perception of Koffka's Figs. 1 and 4

		Perceived as			
		bidimensional		tridimensional	
Group	No. Tested	Fig. 1	Fig. 4	Fig. 1	Fig. 4
White Children	431	3	24	428	407
Negro Children	99	1	8	98	91
College Group	131	5	14	126	117
Total	661	9	46	652	615

In the light of these results it appears that Koffka is approximately correct when he asserts that Fig. 1 is "spontaneously" perceived as a three-dimensional or solid figure. But the results are very much against his assumption that Fig. 4 is spontaneously perceived as a two-dimensional or plane figure. Only 46 out of 661 so perceived it. More than 13 times as many perceived Fig. 4 as a solid as perceived it as a plane figure. Yet Koffka's argument in support of the Gestalt hypothesis is based, in the article cited, upon the assumption that this figure is perceived as a plane surface.

It has been suggested that possibly the responses here recorded were not spontaneous because of the nature of the questions that were asked to evoke the response. It could be argued that the questions: "What is this a picture of? What does this look like?" are a stimulus which gives the *Ss* a three-dimensional set. These questions were chosen after considerable thought, because they seemed to furnish a stimulus that would evoke an immediate and spontaneous response.

But to find out what effect mind-set may have on the visual perception of these figures a supplementary experiment was carried out with two groups of students in the summer session of the University of Oklahama. In this experiment, Koffka's Figs. 1 and 4 were again used and also two other of his figures, Figs. 8 and 9.

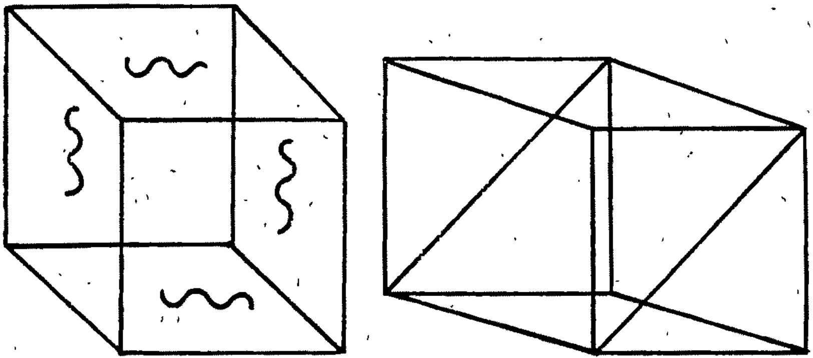

FIG. 2. KOFFKA'S FIGS. 8 AND 9

In the development of his argument in support of the Gestalt hypothesis, Koffka says that Fig. 8 is spontaneously perceived as a cube and Fig. 9 is perceived as a cube more readily than is his Fig. 4.[3]

These four figures were shown to the *Ss* in the following order: 4, 1, 8, 9. This order was used to give the advantage both of mind-set and of first impression to Fig. 4, which is supposed to be perceived as a plane figure. Before showing the figures to an *S* he was told that the purpose of the experiment was to discover if he could see the figures as plane surfaces. Care was taken to avoid any mention of solids or three-dimensional figures. He was told to look for two-dimensional figures. The results of this experiment are given in Table III.

TABLE III

VISUAL PERCEPTION OF KOFFKA'S FIGS. 4, 1, 8, 9 WITH MIND-SET FOR TWO-DIMENSIONAL PERCEPTION

	Perceived as							
No. tested	bidimensional				tridimensional			
	Fig. 4	Fig. 1	Fig. 8	Fig. 9	Fig. 4	Fig. 1	Fig. 8	Fig. 9
57	4	4	3	6	53	53	54	51

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[3] Koffka, *op. cit.*, 167.

Even with a mind-set for two-dimensional perception everyone of the four figures was perceived as three-dimensional by nearly all of the *Ss*. Fig. 4 was perceived as three-dimensional by 93% of the *Ss*; Fig. 1, by 93%; Fig. 8, by 94%; and Fig. 9, by 89%.

Koffka gives no evidence that he has devised any technique to eliminate the influence of past experience and of instruction on the perception of space. The term "spontaneous perception," therefore, is open to serious criticism, as used in his argument. No claim is made that the results recorded in Tables I and II are spontaneous, but only that they are as spontaneous as those assumed by Koffka. Any adequate theory of space perception will take into account the influence of training, both informal and formal, as a determining factor.

The purpose of this discussion is not to attack the Gestalt hypothesis of visual perception or to defend any other theory. But it is written to call attention to the fact that this procedure of Gestalt psychology may lead to conclusions that are not based upon facts. Or does it lead from conclusions? If the Gestalt concept is to make a permanent contribution to the science of psychology, there must be a change of method. Hypotheses must be more than assumptions, and experimentation must take the place of argument and illustration. The Gestalt concept has little to contribute to psychology if its other hypotheses are founded on no better evidence than that which Koffka presents to validate the organization theory of visual perception. Nevertheless, *Gestalttheorie* may be true. There is need for better technique and more experimentation.

Oklahoma College for Women HOWARD TAYLOR

FREE *VS.* MULTIPLE CHOICE ASSOCIATION AS METHODS OF STUDYING SEX DIFFERENCES

For the purposes of differentiating group interests, or "objects more frequently or readily thought about," one might reasonably expect free association to be more illuminative than controlled or multiple choice association.[1] Consider, for instance, a multiple choice exercise in which the subject is presented with a stimulus-word and several, say four, associated words, one of which he is directed to select; namely, the one that goes best with the stimulus-word, or the one of which the stimulus-word first makes him think. If, as frequently happens, some word or object other than those presented comes to mind, the *S* must either refuse to select a word, or select one at random. What is the good of asking the *S*, which of the following words, 'basement,' 'dark,' 'furnace,' 'vegetables,' goes best with 'cellar,' or of which he tends to think, when 'wine' is the word that comes first to his mind and seems to be the most fitting?[2] He mechanically underlines 'furnace,' and marvels at the naïvety of the examiner.

In free association, however, the *Ss* record their associations without let or hindrance, and genuine first associations are, theoretically at least, obtainable. As compared with a limited choice of associations, free associations may claim, (a) to represent the *S* more faithfully, and (b) to produce a greater variety of associative material and be correspondingly more fertile of sex differences. The free method seems, therefore, to be, *a priori*, the better of the two procedures. Yet a comparison of a quadruple choice and a free association exercise recently administered with the object of disclosing sex differences disproved these assumptions. The free association exercise was found (a) to reveal sex differences less clearly than the multiple choice; and (b) to add nothing important to the findings of the multiple choice exercise.

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[1] Part of a research in sex differences directed by Professors L. M. Terman and C. C. Miles of Stanford University. This study was made possible by a grant from the Committee for research in problems of sex, National Research Council.

[2] A free association score for high school *Ss* in a recent experiment was 21 males to 1 female associating 'wine' with 'cellar.'

A comparison of the responses associated with 12 stimulus-words chosen for this purpose at random from those common to a free and a controlled exercise recently administered to the same groups of *S*s suggested an explanation of this result, and exposed the disadvantages of the free association method. The comparison showed:

(*1*) That a majority, 54.5%, of the free responses fell within the same psychological class as the prescribed multiple choice words. By an associate of the same psychological class is meant an associate which, whether of the same meaning or not, clearly belongs to the same circle of ideas.

(*2*) That a change in one or more of the choice words could have materially increased this majority. For the four largest classes of free associates did not always agree with the four choice associates. With the stimulus word 'flesh,' for example, out of 391 free responses 192, approximately 50%, belonged to classes represented by the choice associates 'blood,' 'color,' 'meat,' 'soft.' But 121 free responses were 'skin' or 'limb' or words like them. If 'skin' had been substituted for 'soft' in the choice exercise the 50% would have risen to 90%.

(*3*) That the associates prescribed for choice included some members of a class (about 10%) not represented among the free associates, and yet yielded a significant though seldom a very marked sex difference. That is to say, whereas in free association only the stimulus-word can be used to reveal a particular class of sex difference, in multiple choice association both the stimulus- and the associate-words can be used for that purpose. This is a decisive advantage, because a defect of the free association method is that one cannot tell beforehand how many associates of psychological (sex-differentiating) significance will occur, or whether the yield will be large enough to be significant. Specifically, in free association economic, social, and religious words were far fewer than emotional words, words for parts of the body, or words for activity and adventure; and mechanical, scientific, historical and geographical words figured scarcely at all. This disproportion can be reduced, no doubt, by trial revision of the stimulus-words, but the simplest way to secure an even distribution of a representative variety of associate classes is to distribute the associates in advance as well as the stimulus-words. For example, the inclusion of 'enemy' as one of the four choices against 'face,' or of 'sin' against 'embrace,' induced sex differences over 'adventure' and 'religious' associates in neither case represented among the free associates with these stimulus-words. Sex distinctions can be obtained more adequately and with fewer stimulus-words by the multiple choice method than by the free association.

(*4*) That the free association method yielded certain sex differences precluded from the choice association in the nature of the case—in tendencies to associate synonyms, antonyms, and 'clang' words. Differences in these respects were not considered in our present inquiry, which was confined to meanings or contents; but if these 12 cases are a sample, free associates contain much more of this material than is necessary to establish sex differences in these respects; and the excess is waste. Thus 827 synonym-antonym associates out of a total of 4050 available for all classes of sex difference is disproportionate. It may be added that waste of another class, namely, purely irrelevant associates (words that appear to have nothing whatever to do with the stimulus experience), occurred rarely in the present material, indeed not frequently enough to differentiate the sexes in the tendency to irrelevance itself.

(*5*) That the stimulus-word in the free association method tended to overdetermine the associations; it evoked so many of a particular class of associates (similar or cognate to the stimulus-word itself) that the other classes were reduced to insignificant and therefore non-significant numbers. The stimulus-word 'face,' for example, obtained 194 free associate words for parts of the body, out of 354 words in all. The quadruple choice ('enemy,' 'powder,' 'pretty,' 'watch,') assigned to 'face,' avoided words for parts of the body altogether. 'Danger' drew 264 (136/128)[3] traffic words, 'stop,' 'cop,' 'crossing,' 'curve,' 'flag,' et cetera, and 10 synonyms; which left only 61 for the multiple classes. A considered multiple choice can avoid such disproportions.

(*6*) That beyond the four prescribed classes there were with every stimulus-word some free associates belonging to other sex-distinguishing classes. With 'blush' went 'girls' (24/14) and words for parts of the body (10/16). But these extra responses with a given stimulus-word did not disclose items of sex difference beyond those covered by the control associates with other stimulus-words. Had they done so, this would have been due to the absence of words of these types from the choice of associates presented; consequently the remedy would be obvious. Not an inferiority of the controlled method as such, but a need of making the choices representative, is indicated. For this purpose free association provides one useful approach amongst others to the multiple selection, as suggesting 'promising' associate classes. Once a well-considered, evenly proportioned set of stimulus-words and choices has been developed, a regular use of the controlled association, with an occasional resort to the free, would seem to be the most practical and effective procedure in investigations of sex difference in association.

In seeking an economical treatment of free association material, we were led to the conclusion that the same sex distinctions would emerge from scrutinizing a sufficient number of sharply contrasting cases[4] as from sifting and sorting all the material. But we could not with free associations make sure of the contrasting cases for any particular psychological class being significantly numerous. For example, the contrasting color associates, 29/21, are too few to signify a male preference. But this is precisely the contingency which the multiple choice procedure can forestall. By selecting for its choices the most sex-contrasting words or word classes, and distributing them sufficiently and evenly, the numerical unevenness of the free results can be prevented; and we have instead a method of contrasting cases systematically applied.

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[3] The number before the solidus (/) gives the number of responses by men or boys, and the number after it the number of responses by girls or women. In this instance (136/128) 136 men and 128 women gave traffic words in response to 'danger.'

[4] That is of the reponses recorded by a far higher percentage of one sex than of the other.

Differences in the character of the responses. Let us now consider the effects of the differences between the free and the controlled methods. In the controlled method the experimenter suggests the associations, in the free method they are left to the *S*s. The 'choice'—the presented associates—being limited and prescribed, the *S* concentrates more on comparing the values of the given associates than on giving association free play. The controlled method has no doubt less right to be called *association* than the free method, but it is not, therefore, an inferior instrument for deriving sex distinctions in interest. Put another way, in the free method the *S* reveals (or tends to reveal—he is liable to inhibitions) the word that he actually does associate; in the controlled method the word which, on however short consideration, he *decides* to associate with it as being in some way most relevant to the stimulus-word, without considering what he means by relevancy or by 'going best with it.' In the free method the *S* records any word that comes to mind whether relevant or not, for he is not judging relevancy. For the purpose of the study of sex-differences the controlled method seems to be the more valuable: it excludes mechanical and verbal associations, and it tends to reduce the influence of mere frequency. "These two words" (the stimulus-word and the word chosen) "are in my judgment members of a universe of discourse, or a circle of words, or word-referents, which go together better than do any other pairs" is what the *S* is declaring; and he is to that extent indicating a characteristic universe of thought, or sphere of interest, a topic to which one is emotionally inclined to attend, rather than a mechanical association by frequency or recency. But the free method abounds in mechanical associations—synonyms, antonyms, etc.—which do not correspond to interest in any vital sense.

Moreover, since the experimenter in the controlled method selects the associates, he is able to exclude the trivial ones and to direct attention to the valuable. This is scientific management, an economical use of available material.

In fine, while the free method supplies us contrasting cases in unpredictable proportion and at haphazard, it wastes energy on negligible differences and on trivial forms of association. The controlled method is better able to proportion the associate classes, and to confine itself to relevant cases that are indicative of genuine sex differences. The saving of time and trouble in assembling the raw material is also an additional argument in its favor.

Defense of the controlled method. We now turn to the criticisms against the controlled method. The first criticisms we met were that the *S* tended to record his choice at random, and the experimenter to overlook associative differences outside of the limited repertoire. These criticisms rest upon the asumption that the *S* exercises himself in free association, and that none of the choices given him would have occurred to him. Since these assumptions, as we have shown, are invalid, criticism lapses.

The second criticism seems more pertinent: that inasmuch as the associates are charged beforehand they do not disclose sex differences beyond them; and the more select the selection, that is the less it includes miscellaneous and random words, the more we restrict discovery. But, as has already been pointed out, this did not hold in the present investigation: the controlled method disclosed all the sex differences (except a difference in irrelevant associations) disclosed by the free method. The results of a separate experiment (not reported here)[5] with free associations were confirmatory. Analysis yielded 13 items of noticeable sex difference. It was found that not one of these 13 went undetected by the controlled method, though the material for social, religious, and economic distinctions was, as with the free method also, somewhat meager. For these 13 topics the two methods proved very much on a par, while a fourteenth topic, scientific and mechanical preferences, was fairly adequately represented in the controlled method, but in the free method scarcely at all. By careful selection of associates all the ground opened up by the free method can, it seems, be covered by the the controlled.

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[5] Reported in a paper on "The use of free word association in disclosing sex difference" shortly to be published in this JOURNAL.

In the controlled method we fix a number of routes, and keep to them. With the free method we wander here and there over several. In both methods important directions may escape us: in the controlled because we have left them out of our itinerary; in the free because they do not happen to come in. But the controlled method has the advantage that one can change the association routes as well as the starting points.

Summary. The responses to a quadruple-choice word-association exercise prove more economical, illuminating, and representative of differences between the sexes (and presumably between any groups compared) than free associations because (a) trivial, irrelevant, and random responses, prevalent in free association, can be avoided; and (b) a due representation of distinctive interests, which escape free association, can be assured beforehand.

The fact that *S*'s responses, in the controlled exercise, are relevant rather than mechanical increases rather than decreases its value.

Stanford University H. G. WYATT

"PSYCHOLOGY TODAY:" A QUESTION OF ETHICS

Some years ago a moving picture entitled "Ingagi" occasioned considerable controversy over cinema ethics. In court the producer defended himself on the grounds that all moving pictures contained some "faking"—the house you see is merely a plaster façade, etc. Questions arose. How much of a picture *must* be genuine? What are the *ethics* of cinema production?

To the writer, Henry E. Garrett's essay, "Psychology today: A brief introduction for radio listeners," seems to raise a question of ethics. What psychologists present in technical journals stands as their own and is usually consulted by none but fellow psychologists. This holds to a degree for books as well. Even in popular magazines or newspapers the writings of a scientific man are still in large measure a matter which readers are free to value for themselves. An essay appearing under the authority of a "National Advisory Council on Radio in Education" is in a different category.

That no ultimately accurate résumé of modern psychology is possible does not excuse erroneous statements. An essay depicting "Psychology Today" must be at least up-to-date if for no other than *ethical* reasons. "Ethical," namely, because of the public for whom it is intended and because the name "National . . . etc." predisposes a relatively uncritical, relatively untrained public to accept the essay as gospel. The man into whose hands this booklet is placed is presumably uninformed regarding psychology today. This man cannot evaluate the introductory essay in terms of psychology today, else the entire plan, *which the writer heartily endorses,* would be idle. He cannot be uninfluenced by the words "National" and

"Education" in the Council's title. The question is really quite simple. How accurate must the introductory essay be if it is to play fair in this instance?

A few details. On p. 8 is the drawing of a cat in a puzzle box; we are told, on p. 9, that "the decrease in the time necessary in successive trials is a measure of the cat's ability to learn;" and, (regarding the learning curves on p. 10) that "improvement *nearly always* results from a gradual dropping away of false and useless movements, and from an equally gradual 'stamping in' of the right movements. Animal psychologists have found *little evidence* that animals in such situations 'reason' or 'think out' the correct solutions."[1] If the lay reader were familiar with Adams' work he would not be so dependent upon the statements of this essay.[2] It is because he is probably not that the writer feels a case of this sort at variance with fair play, a violation of trust and hence, in the long run, inimical to the science of psychology. The reader may not have consulted Adams' monograph but the author should have.

It does not suffice that the expressions "nearly always" or "little evidence" should be used. These quantitative terms are at best comprehensible only to those for whom the essay was not intended; at worst they are false for they suggest practical certainty ("nearly always"), which is not true, and practical insignificance ("little evidence"), which is incorrect. To assert that "animal psychologists have found little evidence" means *in this case* "no animal psychologist has found any appreciable evidence." Verbal niceties comprehensible to professional psychologists are wasted on the layman for *he* reads, "Psychology says animals do not reason." The statement is in consequence false despite its restrictive adjectives as printed.

Again, and this time without reserve, the author asserts (p. 13) that "observant or attentive application . . . [is] peculiarly a human accomplishment." Were the author unfamiliar with the literature of animal psychology this statement of a prevalent, if ill founded, popular belief might be excused. But the assumption seems not unwarranted that one writing of "Psychology Today" would have informed himself on these matters. It follows that the author either was not acquainted with the literature or that he has bid for popular applause by affirming prejudices of the untrained.

The case of animal psychology is typical and has been emphasized here as illustrating the point at issue. Fig. I, p. 7 indicates the usual conditioned response chart. That some learning occurs in this way no responsible psychologists deny. In a *technical* journal it is enough that one say, "The way in which fears . . . are *often* learned is illustrated in Figure I."[3] For *this* pamphlet the statement is misleading. The untrained reader sees a picture; *he* reads, "This is the way fears are learned." It is in the nature of things that the author's having qualified with "often" will probably pass unnoticed. In the competition: Figure I with its heavy lines and air of assured finality on the *one* side, the restrictive "often" on the *other*, the black arrows have every advantage. Hence whether he has printed it or not the author has in effect said to his reader, "*This* is the way one learns." (Compare also p. 16.)

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[1] The italics are mine. Discussion of these expressions follows in the text.

[2] Donald K. Adams, Experimental studies of adaptive behavior in cats, *Comp. Psychol. Monog.*, 6, 1929, (no. 27), 1-89.

[3] Italics mine.

We turn to the subject of emotions. If this field were as neatly organized as the discussion suggests, why have psychologists done so little toward a genuine ordering of our lives? Doctors, surgeons and others are integral to civilized life, why not psychologists? The answer is that when surgeons knew no more of human bodies than psychologists of human minds they were not yet more useful than are psychologists today. The study of surgery antedating its present skill is a matter of centuries. A medieval practitioner professing today's knowledge of anatomy would have been, to say the least, somewhat previous in his claims. Psychology does not perform in present society as does the physician because the science itself is not yet adequately developed. It is no disgrace, however, to be young. No one requires of a science that it reach maturity over night. To allege manhood when every act and feature betrays the youth is ridiculous. Psychologists are *constructing* a science: this is the essence of psychology today. Rash and premature claims do not lighten their task.

We read, p. 20, that "the management and control of one's emotional life is so comprehensive a topic that only a few hints can be given here as to how it can be done." There can be no other interpretation here that "it can be done = it is being done = psychology knows how to manage and control." That is what the sentence says to any but those intimately familiar with the details and fully aware that the illustrations of pp. 16-20 are at best merely indications of the direction in which certain psychologists are working.

As regards intelligence the same considerations apply. That much has been done in this field no one denies; that a tone of finality is not justified, perusal of controversial issues will quickly disclose. But those for whom the essay is intended are not at the time they read it in a position to undertake this perusal. There is no other conclusion than that the essay is unfair both to the reader and to psychology.[4]

Frankfurt am Main, Germany — WILLIS D. ELLIS

CONCERNING THE ABSOLUTE METHOD IN PSYCHOPHYSICS

The present experiment is an attempt to determine the validity of a suggestion recently made by Fernberger.[1] When comparing the size of the intervals of uncertainty in a lifted-weight experiment in which a relative series followed an absolute series, it seemed that the intervals of uncertainty for the relative series were larger than one usually obtains.

In order to discover if this effect can be obtained with relative constancy and also if an opposite effect could be demonstrated if the relative series preceded the absolute series, experimental results were obtained from 24 naïve *O*s using the same conditions and instructions employed by Fernberger. The *O*s were divided into 2 groups of 12 each. For Group I the absolute series was given first and the relative series second—an exact repetition of Fernberger's experiment—and for Group II the order of presentation was reversed.

The results for Group I will be found in Table I. In the third column for the relative series it will be observed that there are only three values of the interval of uncertainty (I. of U.) smaller than 8 grm. while 6 values (half of the *O*s in this

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[4] Attention is called to the fact that no reference is made to any other aspect of this booklet than the introduction.

[1] S. W. Fernberger, On absolute and relative judgments in lifted-weight experiments, this JOURNAL, 1931, 43, 560-578. Cf. esp. 573 f.

TABLE I

Showing Results of Group I

(Absolute Followed by Relative Series)

	Relative Series				Absolute Series			
O	h_1	h_2	I. of U.	P.S.E.	h_1	h_2	I. of U.	P.S.E.
1	.087	.082	7.12	97.40	.111	.077	9.28	91.49
2	.066	.061	9.46	92.99	.075	.085	4.84	89.06
3	.077	.074	7.53	96.42	.051	.063	13.13	92.37
4	.079	.082	9.44	95.15	.066	.072	7.46	91.09
5	.120	.131	8.32	94.78	.091	.088	3.97	89.27
6	.080	.088	10.00	96.55	.045	.062	21.65	87.08
7	.076	.114	9.98	98.49	.050	.049	13.28	85.88
8	.070	.086	8.78	95.31	.101	.162	1.28	95.55
9	.075	.079	8.15	93.48	.041	.052	16.48	93.79
10	.049	.121	11.15	96.79	.077	.119	9.84	92.28
11	.068	.076	7.11	96.47	.069	.228	8.66	94.11
12	.065	.079	13.23	97.62	.070	.059	6.52	91.56
Av.	.076	.089	9.19	95.95	.071	.093	9.70	91.13

group) give values greater than 8 grm. These results seem to confirm the observation made by Fernberger that the values of this interval are higher than one usually obtains for a relative series when it is preceded by training in an absolute series.

A consideration of the results for Group II (Table II) in which training was first given in the relative series strengthens this view. For these *O*s the average interval of uncertainty for the relative series is actually higher than for Group I.

TABLE II

Showing Results of Group II

(Relative Followed by Absolute Series)

	Relative Series				Absolute Series			
O	h_1	h_2	I. of U.	P.S.E.	h_1	h_2	I. of U.	P.S.E.
1	.083	.068	5.52	96.09	.093	.105	10.50	94.09
2	.087	.085	6.86	97.91	.093	.103	10.88	93.37
3	.086	.076	11.32	94.26	.079	.100	9.35	95.47
4	.074	.075	3.26	97.45	.052	.054	11.62	91.42
5	.060	.044	15.60	96.35	.074	.077	14.69	93.11
6	.053	.058	13.78	97.30	.077	.098	12.50	96.17
7	.074	.081	9.62	97.10	.071	.077	16.20	93.38
8	.093	.094	2.84	92.51	.087	.104	8.65	93.61
9	.115	.105	7.65	94.85	.092	.083	13.18	87.65
10	.059	.078	9.03	95.78	.096	.100	3.47	89.71
11	.096	.097	8.89	94.67	.092	.108	7.90	97.25
12	.038	.023	19.49	101.15	.109	.097	13.06	91.10
Av.	.076	.074	9.49	96.28	.085	.092	11.00	93.03

This higher average is misleading and is due to two extremely large values. But 5 of the 12 *O*s give values smaller than 8 grm. and the values for 4 of these are smaller than the smallest value for any *O* of Group I.

As a working hypothesis one might believe that this situation is due to the *O*s giving a relatively large number of 'intermediate' judgments in the absolute series and thus developing an attitude which is carried over and results in a relatively large number of 'equal' judgments in the subsequent relative series. If this be true one might expect a smaller number of 'intermediate' judgments in an absolute series if it were preceded by training in a relative series. An examination of the next to the last columns in the two tables would indicate that, if anything, the opposite relation is true; namely, the values of the intervals of uncertainty tend to be smaller in the absolute series if there is no previous training in a relative series. It would seem then that the larger values for the subsequent relative series obtained by Fernberger, as well as in the present experiment, might be accounted for in terms of the difficulty of making the adjustment from one series to the other.

It has seemed worth while to give all of the significant values for all *O*s in both series inasmuch as experience with the absolute series is as yet so limited. We may report that none of the 24 *O*s had any difficulty in formulating judgments in the absolute series. The statistical results confirm Fernberger's former study in several respects: (1) the values of the indexes of precision are of similar magnitude for the absolute and relative series; (2) the intervals of uncertainty tend to be larger for the absolute series; and (3) the point of subjective equality for the absolute series is lower than the central stimulus value, of a magnitude very similar to the effect of the error in the relative series.

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ELISABETH DAVIES
W. W. MARTIN BABB

THE GENETIC PRIMACY OF HEDONIC TERMS

An interest in the genetic primacy of the different hedonic terms led the writer to make a study of the vocabularies of children. All the references which could be found to recorded vocabularies were consulted, in so far as they were available at the Harvard University libraries, and in all 61 vocabularies were tabulated. The group represented a wide range with respect to the intelligence and social position of the children. The particular words looked for in each of the vocabularies were *good, bad, like, love, pleasant,* and *unpleasant.*

Of the individual vocabularies there were only nine whose records were given chronologically for a number of years.[1] Their records are given in the accompanying table which shows the age in years at which the words first appeared.

An analysis of this table shows such little variation between children that we feel justified in making the following generalization. Of the six words studied,

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[1] W. G. Bateman, *J. Edvc. Psychol.*, 5, 1914, 307-320; *ibid.*, 6, 1915, 475-493; T. Beyer, *Educ. Rev.*, 49, 1915, 191-203; *ibid.*, 52, 1916, 478-489; W. Boyd, *Ped. Sem.*, 21, 1914, 95-124; M. C. and H. Gale, *Ped. Sem.*, 9, 1902, 422-435; C. W. Mickens, *Child Study Mo.*, 3, 1898, 203-205; N. M. Nice, *Ped. Sem.*, 22, 1915, 35-64; *ibid.*, 24, 1917, 204-243; *Proc. Okla. Acad. Sci.*, 4, 1924, 147-168; *ibid.*, 6, 1926, 317-333; P. M. Pelsma, *Ped. Sem.*, 17, 1910, 328-369.

good usually appears first, followed by *bad* and *like*, then *love*, and finally *pleasant* and *unpleasant*. While the material is not complete for the rest of the 61 vocabularies, examination of the data as a whole confirms such a generalization. In the supplementary data there were five more vocabularies in which the words *pleasant* or *unpleasant* appeared (in three cases at the age of 3 and one each at 4 and 6), but, by the only objective standard we have, that of extent of vocabulary, all the subjects using pleasant or unpleasant were precocious children. Particularly interesting is the fact that in none of the vocabularies up to the age of 6 years did the words 'pleasant*ness*' and 'unpleasant*ness*' appear.

	Cases								
	A	B	C	D	E	F	G	H	I
Good	1	—	2	2	2	2	—	3	1
Bad	3	2	2	3	3	3	2	2	—
Like	2	3	2	3	2	—	2	3	2
Love	3	4	3	4	2	2	3	3	2
Pleasant	—	—	—	—	—	—	3	—	—
Unpleasant	—	—	—	5	—	—	—	—	—

Since we seem to have discovered genetic differences in primacy of the common words used to cover the hedonic reaction, the question arises of their possible differences in meaning. An experimental investigation of the reasons for the differences in connotation of these words should prove stimulating for the field of affectivity.

Hanover, N. H. EDNA B. HUNT.

A REJOINDER

In a recent number of this JOURNAL,[1] Drs. Ferree and Rand discuss the difficulties they have met in my work on achromatic sensitivity.[2] Their difficulties are of their own making, and are due to an unnecessary insistence on geometrical optics. There is reason for believing that when all the dioptrical influences they suggest have been evaluated (as I have done) the anomalies will remain. My purpose was to show that distance, qua distance, was a factor, and that values determined for one distance were not applicable to another. This I have termed an anomaly from the point of view of geometrical optics.

One cause of confusion is their failure to restrict the term stimulus-distance to my meaning. In the first two pages of their criticism they argue that, as distance has remained constant in their work, its effect has been properly weighted. But this is weighting it only in the obvious and common sense of geometrical optics. It is not at all what I mean by the term, because in their view distance is treated only in so far as it affects size and intensity of retinal image. Later on however, they apparently meet me on common ground in the use of the term, taking distance as a variable which may influence sensitivity despite geometrical compensations.

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[1] C. E. Ferree and G. Rand, Distance of projection of the visual image in relation to its apparent intensity, *op. cit.*, 43, 1931, 678-684.

[2] Ellis Freeman, The achromatic sensitivity of the dark-adapted retina in relation to stimulus-distance, this JOURNAL, 43, 1931, 246-253.

They imply that the anomaly is an artifact of uncontrolled dioptrical conditions (accommodation, pupil, etc.) which may account for inconsistencies. I cannot agree to this. It has seemed to me a matter of elementary precaution to anticipate these possibilities. Indeed I have elaborated this matter in tedious detail in earlier papers.[3] Earlier work shows the futility of dioptrical factors as an explanation of the anomaly, for as soon as one set of conditions is explained by this means, a contradiction arises from another set to which the identical explanation should apply. In one case a wide pupil and thin lens give lower acuity, a narrow pupil and thick lens higher (Aubert-Foerster phenomenon); and in another case the acuities for the same conditions are reversed.[4]

The published data of the work on achromatic sensitivity give internal evidence that pupillary variations cannot account for the anomaly, and that, therefore, an artificial control of the flux is unnecessary. Fig. 2 in my most recent paper[5] shows a drop at the fovea and a rise in the periphery with increased stimulus-distance. If the pupil were responsible, we should be obliged to attribute a drop and a rise in sensitivity to the same cause. Obviously this is contradictory. Drs. Ferree and Rand would have encountered no difficulty in eliminating the pupil as a factor had they not "purposely confined (their) comments . . . to central vision."

It is difficult to agree that "no special precautions have been taken to control fixation" at the center of the field. On the contrary, ample precautions were taken that do not suffer by comparison with the most careful usually employed. Weeks of preliminary training was given, and the subject understood the importance of reporting wavering. Still more important, fixation was physically easy, because each observation began with a stimulus considerably larger than threshold.

The tangent effect also is of no significance here. First it leads to the inconsistencies already described for dioptrical explanations in general. But there are two particular reasons why it is irrelevant. First, as determinations were made to only 20° of eccentricity, the effect would be but slight at best, and too small to account for the massive differences in sensitivity; and, secondly, if it did introduce a difference, the curves of Fig. 2 (mentioned above) ought to be horizontal, lying at different levels perhaps for each angle, but certainly not diagonal. Once a difference appeared, it should, since it is a function of angle alone, remain constant for all stimulus-distances along the same angle. Instead, as distance increases along any selected angle of eccentricity, sensitivity increases.

I must demur against their statement that my work gives acuity and not sensitivity. Logically their notion would prohibit the term sensitivity whenever stimulus-size was varied, and acuity whenever intensity was varied. Perhaps, if one wished, he could formulate sensitivity data in terms of acuity, even in a maximally simple situation as mine—a faintly illuminated disk in a dark room. But this would be inconsistent with what is understood by acuity, which implies the resolution of a detailed complex such as a letter. In my case there is only the differentiation of an illuminated mass from an unilluminated ground. If that were resolution, then any work on sensitivity would have to be called acuity, and it would make no difference if the units were ergs (Drs. Fernee and Rand) or mm. (mine). Perhaps what they mean to object to is the expression of sensitivity in spatial rather than energy terms. I see no objection to such an innovation, particularly when the uncertainty of energy measurements may be thus avoided.

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[3] Freeman, Anomalies of peripheral visual acuity, *J. Exper. Psychol.*, 12, 1929, 324-340; Anomalies of visual acuity in relation to intensity of illumination, this JOURNAL, 42, 1930, 287-294.

[4] Freeman, Untersuchungen über das indirekte Sehen, *Psychol. Forsch.*, 14, 1931, 332-365.

[5] *Op. cit.*, this JOURNAL, 43, 1931, 250.

I am in hearty accord with the suggestion that further work is necessary. That is why I have attempted no explanation of the phenomenon, and far from suggesting finality, I have deliberately chosen the term *anomaly* to emphasize its indeterminate status. While they expressly disclaim the intention to explain the anomaly, it seems that they have hazarded an explanation, though unsatisfactory, in emphasizing the dioptrical factors which they think I have overlooked.

University of Louisville ELLIS FREEMAN

PSYCHOLOGICAL LABORATORIES IN CHINA

Although two short notes concerning psychology in China have been published in this Journal,[1] no report has been made of the establishment and development of the psychological laboratories in China. Psychology, as a separate department of instruction in the universities of China, is not more than ten or fifteen years old; psychology, as science, is much younger; and psychology, as a laboratory science, is even more recent.

Laboratory courses in psychology were first introduced in China at Southeastern University in Nanking (now the National Central University), at the National Peking University in Peking (now Peiping), and at Fuh-Tan University in Shanghai. Fuh-Tan University seems to have been the first to have provided the Department of Psychology with a separate building for its offices and laboratories,[2] but this building is, as I am informed, no longer used exclusively by psychology.

Laboratories have recently been established at the Institute of Psychology of Sun Yat-Sen University in Canton,[3] at Yenching University and at the National Tsing Hua University in Peiping. The Academia Sinica (National Central Academy) of Nanking, which recently added Psychology to its field of activity, acquired a group of buildings in Peiping, October, 1930, that were completely remodelled to meet the needs of a modern psychological laboratory,[4] and active researches are now being conducted in the animal and neurological fields.

The Tsing Hua Laboratory. Although a Department of Education and Psychology had been in existence for some years at the National Tsing Hua University, Psychology did not achieve the distinction of an independent department until the autumn of 1928, when the joint department was divided and Psychology was given 5 rooms for its offices and laboratories. This was only the beginning and entering wedge, for in February of 1931 the Department was moved to new quarters on the first floor of the newly constructed Biological Building which was partly endowed by the Rockefeller Foundation. Great credit is due to Professors Yueh Tang, K. S. Sun, and Ling Yeh for their efforts in behalf of the department. As the history of the development of the psychological laboratories the world over shows, psychology has had great difficulty in acquiring adequate space. It has very frequently had to be content with attic or basement rooms. Indeed, the Tsing Hua laboratory is located in the basement. It is, however, splendidly arranged and equipped and very few American and European laboratories are the equal of it. We feel proud of our laboratory and justified in claiming that it is the first one especially built for psychology in China, as well as being at the present time the most spacious one in China.

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[1] S. K. Chou, Trends in Chinese psychological interest since 1922, *op. cit.*, 38, 1927, 487-488; The present status of psychology in China, *loc. cit.*, 664-666.

[2] Cf. Notes and news, *Psychol. Bull.*, 22, 1925, 140.

[3] *Op. cit.*, 24, 1937, 380.

[4] The National Research Institute of Psychology, *Academia Sinica and Its National Research Institutes*, 1931, 83-87. (The address of the Academia Sinica is 14 Ch'eng-Hsien St., Nanking.)

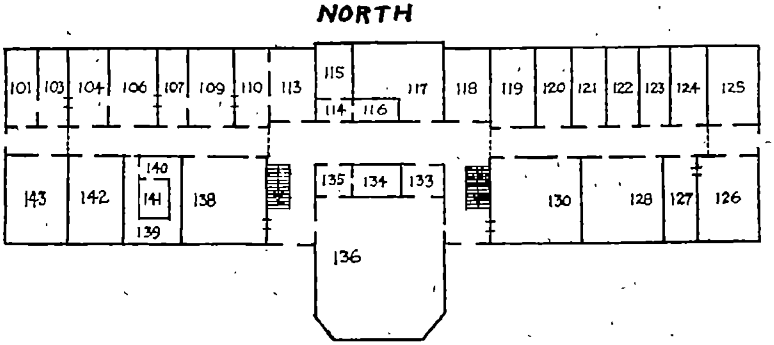

FIG. 1. FLOOR PLAN OF THE PSYCHOLOGICAL LABORATORY OF THE NATIONAL TSING HUA UNIVERSITY

The plan of the first floor of the Tsing Hua Biological Building is shown in Fig. 1. A wide corridor extends throughout the length of the building and the large lecture room (used by all the departments in the University), office, research, laboratory, and seminary rooms, etc., open off of it. Two stairways at the sides of the large lecture room lead up to the second floor of the building. The front entrance of the building is on the north and it opens directly upon the second floor. Four other entrances, one at each side, and two in the rear, open upon the first floor.

Altogether 25 rooms are devoted exclusively to the use of Psychology—about half of the available floor space if the corridor, the large lecture hall (Room 136), the servant (115), boiler (117), and toilet (118) rooms are omitted. Eight rooms (101, 103, 104, 106, 107, 109, 110, and 113) are for offices or individual research; 5 are for the general laboratory (120-124); 2 for lectures (128, 130); 2 for dark rooms (116, 134); and 1 each for the seminary (119), animal laboratory (125), animal quarters (126), storeroom (127), apparatus (138), soundproof (141), library (142), and work shop (143). All the offices, research rooms, laboratories and the workshop have gas, a sink with running cold water, and D.C. electricity. The soundproof room, although not entirely successful in its construction, is perhaps the first attempted in China. The animal quarters is heated by a separate heating system in order that the temperature of the room may be kept constant during the cold winter nights.

The apparatus in the Tsing Hua laboratory invoices at about 30,000 silver dollars, most of which has been purchased within the last few years. Some of the more expensive pieces were obtained from Stoelting and Zimmermann.

The library contains about 500 volumes and the current and back numbers of 30 magazines, of which 7 are complete.

Tsing Hua University SIEGEN K. CHOU

THE TWENTY-SEVENTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The Southern Society for Philosophy and Psychology held its twenty-seventh annual meeting jointly with Section I (Psychology) of the American Association for the Advancement of Science on Tuesday, Wednesday, and Thursday, December 29, 30, 31, 1931, at New Orleans, La. There were five sessions, all held in the auditorium of Hotel Monteleone, the headquarters of the Society, at which thirty-five papers were read.

The Tuesday morning session was devoted largely to learning and forgetting: Rex L. Hoke (Morhead State Teachers College) opened the program with a report on an experimental study of the effects of various factors on the accuracy of tossing steel balls 1-in. in diam. at a target 18 ft. away. The factors were (1) an electric shock automatically applied to *S*'s left hand when an error of a given distance was made, (2) information to *S* of his exact results after each trial, (3) direction of *S*'s attention to any phase of his results or procedure, (4) modifications of intervals between trials, and (5) simple instructions as to how the act should be performed. There were 11 *S*s, each having 60 trials. Punishment yielded fewest large errors, whereas knowledge of results effected the greatest number of bull's eye hits. Intervals of 6 sec. between trials were more effective than 2- or 4-sec. intervals. Direction of attention to the painful consequences of errors increased efficiency as did also putting emphasis on success. Certain aspects of the results indicate the need of using many subjects of different age, sex, etc. in further experimentation.

Richard W. Husband (University of Wisconsin) reported a study of sex differences between a group of 40 college and 20 high school women, on the one hand, and 30 college and 17 high school men, on the other, all of whom were tested on the following five parts of R. H. Seashore's motor skill battery: Serial Discrimeter, Pursuit Rotor, Spool Packing, Crank Drill, and Tapping. There seemed to be slight and uncertain sex differences, except in one of the last two (simple) tests. Comparisons of this sort certainly need very careful control of such attitudes or sets as are affected by different social factors.

Goodrich C. White (Emory University) found in studies on memorizing that the *S*s with poorest initial ability improved most with practice, though there was generally a slight decrease in efficiency in continued practice, a slight advantage in retention of content near the end of the period of continuous memorizing (attributed by the author to 'retroactive inhibition'), and great similarity of learning curves under varying conditions. A striking feature in the curves seemed to indicate a 'final spurt.' This most certainly indicates, however, some sort of unevenness in stimulating conditions.

O. A. Simley (University of Arizona), assuming that the strength of association bonds between a series of geometrical characters and their respective nonsense syllables is measurable in terms of the speed with which they function, reported a study of relations of sub- to supraliminal learning. He had his *Ss* associate 360 meaningless geometrical figures with nonsense syllables in pairs of 9 in each of 40 series. Serial orders were varied to compel only cross (figure-syllable) associations and supraliminal associations were measured by the reaction time between the appearance of the figure in Hull's exposure apparatus and *S*'s verbal response, while subliminal learning was "measured by the number of promptings required to establish the bonds." Learning was (as may be expected in the case of such materials and method) more rapid in the initial stages of the process, and there was a positive correlation, apparently high, between sub- and supraliminal learning both as to quickness of response and as to the slope of the rising curves below and above the threshold. It was hardly obvious from the report that the two different sorts of learning situations were near enough identical or controlled for direct comparison of results.

Fred McKinney (University of Missouri) had had 93 college students learn twelve lines of one poem and 95 learn an equal amount of a similar poem, five minutes being devoted to learning and five to recall. These students were then divided into three groups of about 60 in each; one group learned a second equal amount of poetry, another a list of nonsense syllables, and the third group listened to a Seashore discrimination record. All the students finally recalled what they could of the original twelve lines of poetry. This final retention test was repeated again a week later. No interference ("retroactive inhibition") appeared in the final recall, but only in that a week later. The conclusion that "poetry is not susceptible to retroactive inhibition upon immediate recall" is certainly subject to several conditions which need careful study before such theoretical implications as were inferred in the paper can be safely indicated for a "theory of retroactive inhibition" in general.

John A. McGeoch (University of Missouri), in a discussion of the concept of forgetting, pointed out the need of some systematic attempt to determine the boundaries of "forgetting," which is only the partial obverse of retention. From examinations of phenomena usually classified as forgetting he found only one that stands out as characteristic of all cases—"a loss in the functioning of a learned activity." He concluded also that there is no logical ground for a sharp distinction between this sort of loss or decrement and that resulting from continuous work or from other special conditions.

H. H. Remmers (Purdue University) described experiments which have been in progress for four years on students in beginning courses in Spanish, German, and French, who were grouped according to ability as measured by freshman entrance placement tests and according to certain other factors, and who were graded on test questions according to a standard procedure. No results are available to the reviewer.

The afternoon session, devoted in the main to sensory and perceptual phenomena, began with an informal report by P. F. Finner (Florida State College for Women) on attempts at measuring the differential intensity limen between the several pairs of primary colors and between each color and white. Wave-lengths were kept constant by means of color filters and intensities were controlled by the photo-electric cell. Forty-six *Ss*, apparently college students, were used. Individual differences in brightness thresholds were large, and the correlations between thresholds were highest in the cases of colors near together in the spectrum (*e.g.* R and Y, 0.72; B and G, 0.92) and were low between thresholds of complementary colors (*e.g.* R and G, 0.06; Y and B, 0.08). In the discussion it was developed that exposure times were not controlled; each *S* observed till he saw the difference. This time-of-stimulus factor, it was suggested by H. M. Johnson, was not controlled in Weber's law determinations though the limen is obviously a function of the duration of stimulation. Geldard suggested that correlation differences noted in the paper were due to absorptions of the photo-electric cell which was used practically as a photometer.

W. F. Smith (University of South Carolina) reported an experiment on the relative quickness of visual and auditory perception. Using four different intensities of the sound and also of the light stimuli with either simultaneous or slightly divergent presentations (controlled by means of contact attachments on both ends of the 'hand' on the face of the Hopkins chronoscope, and two movable contact points on the edge of the face), he was able to vary according to predetermined schedules the relative times and the intensities of the two stimuli. One hundred forty-four possible intensity and interval combinations were presented individually to two groups of college students, 40 in each group. Those of one group were to report at every trial which stimulus came first; and those of the other group, whether there was an interval perceived between the stimuli. No doubtful reports were permitted. Results show that by a suitable variation in intensity either the visual or the auditory stimulus, when presented at or near simultaneity, can be made to appear first in time. The uncertainty range can therefore be affected by an increase in the intensity of either the visual or the auditory stimulus. The possibility of an absolute speed-of-perception measurement of visual and auditory stimuli based on the rheobase in chronaxy is suggested.

Lyle H. Lanier (Vanderbilt University) gave a report of progress on his experiment in cutaneous denervation. The general interrelations of the several "modes" of sensation, both immediately following denervation and during regeneration of the nerves, were emphasized. The areas of anaesthesia to warmth, cold, light touch, and pain stimuli at three-week intervals were shown in lantern slides. These were all from only one of the three *Os* (Mr. Lanier) serving in the experiment. Dissociations occurring both immediately after denervation and during regeneration indicate the existence of four separate afferent mechanisms corresponding to cold, warm, pain and contact, and support the quantitative theory of Nafe; but these four forms of sensitivity, however, tend to form two general groups—a thermal and a contact-pain group. The virtual coincidence, in general, of anaesthesia areas for contact and pain and for cold and warm (a much larger but closely corresponding area), suggests interesting neurological problems. Head's theory finds little support in these results. The 'intermediate' zone does not uniformly show the protopathic complex of attributes; the differential order of return, required by his theory, is not found; and "the protopathic aspects of sensation—hypersensitivity and 'reference'—occur on skin responding to light touch, showing apparently no 'inhibition' of the former by the latter."

Max F. Meyer (University of Missouri) called attention to a recent 'refutation' by Galperin of the old theory that the Poggendorff illusion is due to the overestimation of small and the underestimation of large angles. Not denying the effect of such wrong estimations of angles, Meyer pointed out that ten years ago (*Psy-*

chology of the Other-One, 1922, 231-238) he presented a refutation based on a simpler and a more convincing principle. It will be noted, however, that Meyer's explanation is based on double and triple oblique lines (p. 233) and that a single oblique line interrupted by a vertical plane figure, say, shows the phenomenon as effectively as two or three such lines, even though Meyer's explanation does not, it would appear, apply to such a case. The problem treated theoretically, as to the relative effects of different factors (whether innate or acquired) seems to be one that can be solved experimentally by testing different hypotheses on the basis of the judgments, under controlled conditions, of a large number of unsophisticated *Ss* at different ages.

C. C. Ross (University of Kentucky) reported some apparently preliminary experiments on the reliability of students' ratings of different subject matter according to specified characteristics. For example, the ratings of instructors, "according to a certain rating scale," by approximately 200 students ranging from freshmen to and including seniors, and a re-rating "without warning a week later" varied from slightly negative correlations to 1.00, "the central tendency usually being in the 0.60's and 0.70's." He concluded "that many of the current studies employing student ratings are to be taken with the traditional 'grain of salt'." That is, of course, not a new discovery; moreover, Mr. Ross had seemingly overlooked the precautions against memory and other effects of earlier ratings, as well as the fact that both of the ratings of instructors may be influenced in the same way by rather well known 'reputations' current about the campus.

G. C. Brandenberg (Purdue University) reported results of two investigations, two years apart, into the conditions "which might be classed as obstacles to the best quality of instructional work in the class room." He was chairman of a committee organized to discover and remove such obstacles. A questionary was sent to the instructional staff at the time of each investigation. The five most serious obstacles were (1) the large amount of time required for reading students' papers; (2) bad light, heat, ventilation, etc; (3) indifferent attitude of students (evidently only a *result* of other conditions); (4) heavy teaching loads; and (5) unsatisfactory office conditions. A close agreement was found among all the ranks of the instructional staff but departmental heads and assistants, indicating, probably, that "the individual who has the most to do with teaching conditions—does not see teaching problems in the same way as do teaching members of the staff." Considerable improvement in these conditions was found in the second investigation.

Joseph Peterson (George Peabody College) reported data showing a serious deficiency throughout the United States in the training of psychology teachers in state teachers colleges and normal schools, as compared with state colleges for women, standard colleges and universities, and the better privately endowed institutions. From the two reports on the qualifications of institutions and teachers of Psychology in the South, issued in May, 1928 and 1931 by the Committee on the Teaching of Psychology (of which he was chairman during its first five years of work), it was shown that the increase in properly qualified teachers—those holding Ph.D. degrees with major work in psychology—during the three-year period indicated was 54%. In the corresponding period the increase in members and associates of the American Psychological Association was 37% in the other 32 states, and 40% in the entire United States. This rapid improvement in the South he implied was an indication of the effectiveness of the Society's work through its committee. The number of qualified psychology teachers per million population in the year 1930-1931 in each state was found to be as follows: Alabama 3.4, Arkansas 1.1, District of Columbia 14.3, Florida 4.1, Georgia 2.8, Kentucky 4.2, Louisiana 2.4, Maryland 12.2, Mississippi 1.0, Missouri 4.1, North Carolina 4.1, Oklahoma 2.5, South Carolina 1.7, Tennessee 5.9, Texas 2.1, Virginia 5.4, and West Virginia 4.7; all combined, 6.1. There were at the same time 9.5 members and associates of the American Psychological Association living in the other 32 states and 9.1 in the entire United States per million population. Since a considerable number of the associates have not yet earned their Ph.D. degrees in psychology, the difference between the South and the other sections in number of psychologists per million population is not as great as these numbers suggest, and the comparative gain in the number of psychologists is even greater in the South in the triennium indicated than the percentages show.

The Wednesday morning papers were largely theoretical and philosophical. A. S. Edwards (University of Georgia) began the program with a discussion of "The Concept of Normality." He contended that psychological problems are "similar to those in the other sciences, and, as in those sciences, need to be based upon averages." He also raised problems, confronting one in the handling of groups, as to the size "of the normal group, the amount of deviation from the average that can be admitted to the normal group, etc." There seems to be nothing new in the paper.

In the absence of Knight Dunlap (Johns Hopkins University), his paper on emotions as teleological constructs, was read by Buford Johnson. Dunlap points out that while the 'emotions' were definitely and justifiably aligned with the 'instincts' by William James, the text books continue to treat the 'emotions' in the old fashion even though they have abandoned 'instincts,' and that the behaviorist "bases his theory of emotional development on discrete primary 'emotions'." He contends that emotions are clearly artifacts "determined by reference to the situation in which the reactor is placed, and by the interests and purposes of the classifier." "Any of the conventional names may apply to widely different affective conditions, according to the interest of the namer; and the same affective state may have diverse names according to this interest." Reports by individuals, that they feel depressed, elated, etc., are only the application of previous ideas to their experiences. The errors resulting from a rough application of such views are obviously obstructive to research on the emotions. One finds similar difficulties, of course, in any sort of classification or description, or procedure by verbal methods or symbols.

Herbert C. Sanborn (Vanderbilt University) traced briefly the historical developments in aesthetics, and pointed out that there has been a persistent attempt to build an objective science of art. However, this science, according to certain writers, is to exclude everything psychological, their reason being chiefly that psychological aesthetics fails to do justice to the field of art. Sanborn held that disputes of this sort are ultimately metaphysical in nature, and that while investigations should doubtless be carried on by every method available, the question "whether there is any one method which embodies all the others" should be left to metaphysical revision. Specific interpretations of aesthetic experience are valid in so far as they are in harmony with the systems of which they are specifications, while the validity of these in turn would reside in their inclusiveness and coherence, that is, the degree to which they realize absolute system. Concrete illustrations were given. There is certainly ground here for inquiry into what absolute aesthetic systems would mean and how they could be arrived at if not through the experiences of individuals.

Harold N. Lee (Newcomb College, Tulane University), discussing metaphysics and aesthetics, contended that the usual procedure of approaching aesthetics from the viewpoint of the metaphysical doctrine, is exactly the reverse of what an application of the scientific method would indicate. Aesthetics, which deals with one of our fields of experience, "must enunciate its principles independently of any formulated metaphysical doctrine." From the metaphysical point of view, he said, any set of aesthetic principles would be subject to the interpretation indicated by the metaphysical theory of the interpreter. In the discussion E. S. Edwards objected that psychology did or should start with particular experiences, and Sanborn held that science is today beginning from the most sweeping categories. He referred to the work of Percy W. Bridges as an example. Lee replied that the scientific method does not deny analyses and that he said nothing to identify science with induction, except the natural sciences.

Morten ten Hoor (Tulane University), in an examination of introspection, said that "in all philosophical disciplines generally, the concept of introspection has always been accepted as an ultimate." Introspection, however, seems to have had but little analysis by the great philosophers and psychologists of history. The traditional double-self notion is untenable because it implies a dichotomy of experience, something more than immediate experience. The notion of consciousness *per se* turns out on analysis to be an abstraction, a realistic (medieval) universal. But there are only types of awareness distinguished by types of sensation localized within the body, on its surface, or without it (as the redness in a rose). Usually this awareness has been assumed to be within the body (hence introspection), but it is where it is experienced. Any awareness content can, only within certain limits, be verbally reported. What realities underly awareness is a question of inference from immediate experience. Certainly no metaphysical eye or self is given in immediate experience; hence the error of claims making introspection the source of knowledge of the self or of experience. As a criticism of radical behaviorism, it was suggested that "behavior can enter experience only as sensory awareness;" that pure behavior or behavior as such is as much a metaphysical assumption as the ego or self." A layman in philosophy might suggest the need of inquiry into the concept of "experience." Is *experience* identical with *reality*, or what is the relationship here, and what the evidences? Furthermore, are 'feelings' located?

W. H. Walker (Berea College) pointed out that the concept of causality had arisen rather by implication than by any direct observation. "The patent fact is that we do think of the causal relation as a necessary one when we apply it in the physical world." But this assumption is justified in the large only in a general way. "We never have observed the same conditions producing the same identical results." (How could this be possible, considering temporal aspects?) He regarded it as improbable, "that the present repudiation of uniformity in the action of electrons and quanta will be permanently accepted. We cannot think of nature in terms of caprice." As "this is a matter of the constitution of our thought, called into play first by the suggestions of experience," so is also our thinking of ourselves as "creative causes, at least in some of our decisions. This concept of creative personality has precisely the same ground and the same validity as the concept of mechanical causation." This is a rather unique 'logical jump' to make on such grounds! Why did man leave his primitive thought of nature as animistic, and, in modern times, when better means of observing (under experimentally controlled conditions) are possible, find the *vera causa* view untenable even as applied to himself?

Charles A. S. Dwight (Keuka College), closing the program with a paper on "The Shadowland of Science," indicated changes that have occurred under the developments of modern physics: the crude lump-matter theory of the 19th century has gone and the "oppression" of the old coarse materialism has been relieved; physicists now tend toward becoming quasi-metaphysical, and mechanism [the *bête noire* of the theologian] is wanting as an ultimate explanation; and "the principle of physical indeterminism breaks into the old 'closed circuit' idea of nature with something that looks like a mandate for freedom." He therefore sees new possibilities for consciousness and will "as causal factors" and for "faith or immortal hopes." "The spiritual view of the world is, if anything, more easy or likely than before. Science lingers among the shadows, but faith walks among the candlesticks."

Does this premature outburst of hope based on old conceptions of will and freedom not seem to reveal a sinister motivation that is below that of philosophy as a search for truth? Is this the way out for those whose conception of freedom has failed, or may they also not hope and seek for new conceptions?

The afternoon session was largely devoted to child development and measurements. S. A. Courtis (University of Michigan) reported that data from 30,000 school children in America, England, Italy, Scotland, and Switzerland measured as to height, weight, broad jump, eruption of teeth, and tested with two intelligence tests and one educational achievement test, revealed on analysis the influence of three sets of specific factors—starting points, environmental influences, and maxima at maturity—and one general factor—rate of growth. He found in these conclusions the possibility of "a new point of attack upon the analysis and interpretation of psychological measurements, and upon the prediction of growth."

John E. Anderson (University of Minnesota) reported result of an analysis of data obtained in connection with a survey made by the White House Conference on Child Health and Protection, involving a study of the home life of 1626 boys and 1552 girls of ages ranging between one and twelve years. Parents' answers to questions concerning the children's favorites in the home and the conditions under which jealousy was aroused manifested, with respect to the Freudian assumption of tendencies for the boy to be attached to the mother and the girl to the father, no outstanding sex differences in emotional attachment at any age level. About half of the boys and girls had no favorites, and the other half showed a somewhat greater tendency for both boys and girls to favor the mother. The tendency to have favorites decreased with age. The chief cause for jealousy in both sexes was the showing of affection by the mother for another child. Girls showed more jealousy than did boys and there was a reduction in jealous behavior with age. No evidence for the Freudian Oedipus and Electra complexes was found.

Thomas F. Vance (Iowa State College) reported results of observations on food selections by nursery school children, made through a period of one year including the summer term. The order of tasting and finishing the four foods in the main course was recorded. A high correlation of this order between different quarters was found. Meat and eggs ranked higher in general than vegetables, raw were preferred to cooked vegetables, and the latter to puree. Food combinations were not eaten as readily as single elements served separately. The low ebb in appetite occurred in December and January, and in June and July. There was a positive relationship between the frequency of serving the several foods and their ranks as to preference. This, of course, it may be pointed out, cannot be taken as evidence of causal relations, since those preparing the foods have had experience with other children.

Joseph G. Yoshioka (Anthropoid Experimental Station of Yale University) reported results of daily measurements of temperature, pulse, and respiration rates of four young and two infant chimpanzees through a period of three months. The rank as to variability was found to be pulse rate, respiratory rate, temperature; but the ratio of respiration to pulse rate is relatively constant. The P.E. of the ratio, however, varied significantly, indicating a higher efficiency of this heart-lung coördination in some individuals than in others. The P.E. of the ratio was found to correlate positively with degree of variability in general behavior. The constancy of respiratory to pulse rate would doubtless be expected if one considers the sort of stimuli that arouse each of these activities.

Luton Ackerton (Institute of Juvenile Research, Chicago) presented results obtained by him and Minna Emch on the computation by five different methods (those proposed by S. L. Pressey, Merrill, G. Thomson, Woodworth, and also a subjective rank-order method) of the scatter on the same set of 100 Stanford-Binet test records. The ranks by subjective estimates were based on the ratings by four workers experienced in clinical procedures. Inter-correlations ranged from 0.66 to 0.97, and those between different pairs of raters' rankings, from 0.77 to 0.93 (means 0.87 and 0.85, respectively). The mean correlation of the equally weighted composite, subjective rankings with those by the other methods was 0.79, the lowest coefficient being with the Woodworth-Thomson measures. Since these latter were considered the best technique, it is recommended that in clinical practice the subjective method should be discarded and computations made by the Woodworth-Thomson technique.

E. J. Asher (University of Kentucky) reported results of applying the Myers Mental Measure to 363 children in 15 rural mountain schools of southeastern Kentucky, the National Intelligence Tests to 234 of these children and the Stanford-Binet and the Herring-Binet to 56 children. The median IQ were, respectively, 68, 72, and 73; and the IQ by the Myers test decreased with age from 84 at seven to 61 at fifteen. Since there was no significant difference between the verbal and the non-verbal tests and all gave obviously too low IQ, the conclusion was drawn that the tests were not suitable for children with such inferior environment. It should be added that similar results have been found on comparative racial tests; and also that IQ computed from different group tests will give very diverse results so that this method of computing IQ is not to be recommended.

D. A. Worcester (University of Nebraska) gave summaries of three studies of long-time practice effects on later intelligence test scores, one study on high school and two on college students. In the high school group significant increases in scores, due to previous practice, were found. The scores increased directly with the extent of practice, but with a negative acceleration. In the college group it was found on Alpha that a second administration brought significantly higher scores than the first; and, on the Ohio State University Psychological

Test, that experience with any other intelligence test at any time showed a significant effect. Such effects of practice are now well-known to experienced testers.

Noel B. Cuff (Eastern Kentucky State Teachers College) reported preliminary results of his experimentation with the best methods of scoring objective tests. When answers were written on a separate sheet, conveniently arranged for scoring, rather than on the test-sheet in the usual manner, he found it possible with the use of stencils to increase considerably the rate and the accuracy of scoring both true-false and multiple-choice test papers. Stencils containing mimeographed circles around correct answers were used.

The final session, held Thursday morning, was devoted to abnormal behavior and emotional maladjustments. Paul C. Young (Louisiana State University) reported an experiment on resistance to muscular fatigue in hypnosis. Thirteen college students served as hypnotic subjects and six as controls, the former being tested on the hand dynamometer first in the hypnotic and then in the waking state under conditions otherwise as uniform as possible, while the controls were tested both times in the waking state. Results seemed to show "no great increase in the ability to withstand fatigue in hypnosis; but all hypnotic subjects claimed to have felt no fatigue in hypnosis or immediately upon coming out of the hypnotic session during which they had been fatigued almost to exhaustion." Individual variations were apparently not changed under hypnosis. Some criticisms arose in the discussion as to details of the technique which seemed to be inferior to the technique found now in laboratory experiments.

Roy M. Dorcus (Johns Hopkins University) summarized briefly the psychological and neurological implications of "dissociation," and reported experiments comparing the galvanic activity of *Ss* in whom anaesthesia, anacusia, and amnesia had been induced hypnotically with the galvanic activity of *Ss* under normal circumstances. The lack of a difference between these two groups was interpreted to indicate that one of the meanings attributed to "dissociation" is a mere abstraction since there is no evidence of any breaking up of the neural pathways.

Trigent Burrow (New York City), discussing relations of "normal illusions" to "illusions of the insane," held that psychiatry gains little from attention to the patient's symptoms—false ideas, delusions and phobias, elations and depressions, etc.—these being "but reflections of an impairment that is deeper seated within the organism and which is equally characteristic of normal as of pathological individuals. This impairment consists in definite physiological tensions, alterations, and disturbances." He held that "experimentation under conditions of laboratory control" indicates that the psychopath suffers from a conflict between "those feelings and sensations which belong to the organism as a whole and those sensations which belong to the segment of the organism located within the cerebrum, with its secondary acquired ideas and images," a conflict that is social or phyletic in its scope. "Such a condition is perceptible and remediable only through recourse to physiological methods of approach and not through a program that attempts to exchange ideas for ideas and images for images." Unfortunately, the short discussion did not make clearer either the specific nature of the experiments or of the physiological methods referred to in the paper.

John M. Fletcher (Tulane University), in a paper on medical analogies in abnormal psychology, traced briefly some of the early borrowings of psychology from medicine, and pointed out that "in no system of psychology is there such a detailed array of medical analogies as there is in Freudian psychology." This he held to be true of Adler's doctrine of compensation for inferiority derived from data secured in the practice of pediatrics as well as in Freud's diagnosis and treatment of hysteria. "Psychology therefore owes much to medicine," he concluded, but warned that "analogies do not afford proof," and are useful in cases "only by way of suggestion." The clearing up of some of these fallacies in these analogies will probably result in "some of our schools in abnormal psychology" going "the way of faculty psychology and associationism." It is probably charitable to class the Freudian psychoanalysis as 'psychology' even though some of its contributions in motivation (not new, however, with psychoanalysis) have been suggestive for psychological study. Psychology doubtless does owe much to medicine, but the 'therefore' in the context of the quotation above suggests (probably inadvertently) a too narrow source of the contributions of medicine and an unscientific one, in the main.

H. Meltzer (Psychiatric Child Guidance Clinic of St. Louis) reported results of an investigation of college students' adjustments in anger. The anger responses of 93 psychology students—58 women and 35 men—about one-half of whom lived in sororities or fraternities, were studied by a "controlled diary" method. The diaries yielded 393 anger responses for analysis. Data include conditions before the anger was aroused, conditions and sensed impulses in anger, duration of anger, and certain after effects, controls, and social organization of tendencies. Anger impulses were found to be closely related to organization of home, school, and community life, and to frustrations of one's activities; and the study of anger was found to be "a problem in social dynamics." The men reported 39% more anger experiences than the women. Sixty-four percent of the anger experiences reported by women were reactions to persons, nearly three times as many of which were defensive as aggressive. Only 36% of the men's anger reactions were to persons and over four times as many of these were defensive as aggressive. The general problem is suggestive of careful experiments with better technique. Certainly the last items mentioned in the results, for example, need careful study under conditions that are very well controlled.

The final paper of the last session was by J. B. Maller (Teachers College, Columbia University) on the character and background of the young men who were chosen candidates for the Edison scholarships during the years 1929 and 1930. These 39 high school graduates represent all the states and the District of Columbia. They "had been chosen on the basis of rigorous examinations" and were supposed to represent one of the highest ranking selections of high school graduates in the United States. It was found that the Edison group graduated from high school at 15.5 years of age, about 0.5 year younger than the average age of high school graduates through the country; their parents were chiefly of the professional and commercial group, only 5% being children of farmers or unskilled laborers. About 50% of them had scientists in their family, whereas the corresponding percentage for the other high school graduates is 34. The Edison group came from a higher-than-average social background and their interests are predominantly (by selection apparently) in science. They scored "very much higher in science and mathematics and somewhat higher on questions of personality" than the comparison group. They also, incidentally, took more time for sleep and meals (10.6 hours daily to 9.4) than the other group. Their favorite type of book was that which deals with science and invention, while the other group more generally favored books on fiction. The data available to the reviewer are either inadequate or several of these differences may be accounted for on the basis of the fact that the Edison group of boys were selected rather rigidly according to a certain interest and had an environment which encouraged different lines of activity and daily habits.

At the joint dinner of Sections I and Q (Education) held in Kolb's Restaurant Tuesday evening, the vice-presidential addresses by E. G. Boring and by Leonard V. Koos on "The Physiology of Consciousness" (now published in *Science*, January 8, 1932) and "The Procedure of Investigation in the National Survey of Secondary Education," respectively, were given. The subject of the address by the president of the Southern Society, given Wednesday evening by H. M. Johnson, after the banquet at Hotel Bienville, was "Some Follies of Emancipated Psychology." He challenged the validity, according to the criteria of the sciences generally, of "a large body" of doctrine in modern psychology, contending that psychology when emancipated from philosophy "repudiated the rules of reasoning and standards of proof which are prescribed by Aristotelian logic and which govern modern science and common sense practice. For this logic it has substituted a special logic of its own in which equivocation is treated as a valid method instead of a fallacy." Illustrations with definite citations were given of spurious generalizations, of the confusion of assumed data with observational data, of illicit identification of "operationally defined concepts" with formally defined concepts, and of the confusion of resemblances with identities. It is certainly important that psychologists do not sacrifice accuracy and standard scientific caution, in order to get their "contributions" to press either for rewards of advancement in rank or for financial returns. There is serious need today of more careful and frank criticisms and evaluation of published results of research, as well as of positive depreciation of the mere quoting of recognized leaders in the science. It is not the authority quoted but the validity of the data themselves that bears weight. It is a question how this fact can be made weighty when scientific work is mixed with salesmanship and often motivated by personal financial gain.

At the business meeting of the Southern Society for Philosophy and Psychology, following Wednesday morning's program, Anna Forbes Liddell, Florida State College for Women, was elected president for the next year and Herbert C. Sanborn, Vanderbilt University, and J. F. Dashiell, University of North Carolina, were elected members of the Council. The new committees on the Teaching of Philosophy and the Teaching of Psychology did not make formal reports at this meeting and were continued with the following membership: H. C. Sanborn, (Chairman), A. G. A. Balz, Josiah Morse; and P. F. Finner (Chairman), R. M. Dorcus, T. K. Sisk, respectively. It was voted to accept the invitation of American University and to hold the next meeting in Washington, D. C., on April 14 and 15, 1933.

Peabody College JOSEPH PETERSON

THE FIRST INTERNATIONAL NEUROLOGICAL CONGRESS

The First International Neurological Congress took place at Berne, Switzerland, August 31st to September 4th, 1931. There were in attendance 881 members, representing 42 nations. The largest delegation, 161, came from the United States, but France, Germany, Switzerland, and Italy each had a representation of about 100. The President of the Congress was Professor B. Sachs, of New York City. Papers were presented in French, Italian, German, and English, with no translation. There were numerous excursions, interesting exhibits, a number of moving picture presentations, and the usual conference out-of-session chats. The concern of this brief report, however, is to give something of a picture of the present activities of neurologists represented by the 234 papers included in the book of abstracts.

A general summary of the central themes in the papers of the conference may be found at the end of this paper. Although the categories are not mutually exclusive, and the papers often touched on several themes, each has been tabulated only once, and in the light of its major objective. The summary will serve to indicate, however, the general absorption of neurologists in physiological and pathological considerations which is to be expected of a branch of the medical profession.

It is interesting to note that our present understanding of disorders of the brain and nervous system is very largely in the stage of attempts to secure accurate descriptions and sound anatomical and histological knowledge. There were very few hopeful developments in therapy, but presumably the more careful techniques of diagnosis will eventually serve some purpose more constructive than simply the improved accuracy of prediction as to the rate at which the paralysis will advance, and the length of time before an autopsy can be performed.

The contributions of interest to psychologists vary, of course, with the psychologist. Pavlov gave a brief review of the development of 'neurosis' in dogs by strong excitation, collision of stimulus and inhibition, excessive tension of inhibition, and by castration. These were manifested in chronic lowering of excitability or of inhibition, and in chaotic activity. Szondi (Budapest) called attention to the fact that there are no essential symptoms of neurasthenia. Every subjective state mentioned may be matched in some other case by its opposite. He believed it possible, however, to distinguish two types, (1) the apathetic-asthenic, characterized by a flat blood-sugar curve and a hypo-function of the sympathetic system, (2) the excitable-pyknic characterized by a rise in the blood sugar curve and involving a hyper-function of the sympathetic and adrenal.

A number of the papers showed a rather naïve faith in localization of psychological functions. One, indeed, claimed to have established as long ago as 1909 that the center of consciousness was located near the third ventricle, and that all the neuroses, subconscious activities, hypnotic reactions, changes of mood, forms of dementia praecox, catatonia, sleep disturbances, epilepsy, obsessions, compulsions, and character traits must be explained in terms of modifications of this center. Of evidence of localization of motor functions there was no lack, but the modern point of view in psycho-neurology was approached by Krause (Amsterdam) in his report that while catalepsy is destroyed by loss of the motor area of the cortex it can be reëstablished after three to five months, the function being apparently taken over by other cells. He found that the size of the lesion was a very important factor in influencing the extent and permanence of the motor changes. The difficulties in interpreting evidence on the relation of structure to function were made apparent in many of the reports. Thus, two tumors may grow in precisely the same area, but if one develops rapidly and the other slowly, the symptoms of the first may be acute and marked, those of the second may never appear. The effect of the cortical disturbance in bringing about anemia or engorgement of blood supply at other points brings an enormous number of variables into play. The effect upon pressure of the spinal fluid may be felt at points far removed from the original disturbance. Behavior changes are influenced, of course, by the personality make-up. Trauma is used as ground for a neurosis by some persons, while others react quite differently to much the same degree of apparent injury. The break-down in the area of compensation may be more serious than the original disturbance which set up the need for compensation. So the variables may be enumerated. It is small wonder that one reports a case with symptoms corresponding to tumor of the left frontal lobe while autopsy reveals it to the left parietal; another a case with a tumor apparently in the base of the brain but actually with a glioma of the temporal lobe; another a case of apparent frontal tumor but actually with a large hemorrhagic cyst in the left cerebral hemisphere and with internal hydrocephalus; a fourth, a case appearing to be a neoplasma of the left cerebellum while autopsy revealed glioma of the right frontal lobe.

Tests appeared rarely. Weisenburg of Philadelphia reports having found that normals make much the same type of errors as are made by pathological subjects on all the existing tests of aphasia, including Head's, and so has turned to the development of new tests in this area. One neurologist, who may here be nameless although the United States must own him, made the only mention of intelligence tests, and that in connection with an exceptionally naïve report that very dull children learn to talk at a later age than do normals. Minor (Moscow) reported a very simple and hopeful test of handedness as a result of his observations that the spread of veins on the back of the hand gave an anatomical basis for discerning genuine handedness, regardless of training. Oberholzer of Zürich seems to have found the Rohrschach tests of value in distinguishing the organic disturbance due to trauma from the secondary psychoneurotic developments, but in common with most of the reports, the data were not given in a form to permit statistical appraisal. The lack of statistical concern was perhaps most apparent in the paper which discussed sex and race differences on the basis of a total of 18 cases.

Psychotherapy was discussed in only one paper, and that by a Viennese who not unnaturally emphasized the serious limitations of such techniques. Glandular therapy was not often mentioned, and the tenor of the observations was not very hopeful. The obesity, somnolence, and mental retardation commonly connected with a hypopituitary condition was attributed in one very thorough review not to the gland but to pressure on the floor of the third ventricle. Another paper stated that no preparation of the secretion of the anterior-pituitary acidophilic cells supposedly responsible for structural growth, had been found suitable for human use. Another reëmphasized the fact that the function of the pineal is unknown and that the precocious puberty sometimes observed in connection with pineal disorders was not affected by administration of the gland and not reproducible on animals. Of considerable interest to psychologists was the observation (not the first of the sort, I believe) that tired muscle is restored to something like its original efficiency either by sympathetic stimulation or by contact with adrenalin. The action of hormones in the blood stream was clearly demonstrated in one paper which secured electrocardiagraph records from the mother and the foetus, connected with the mother only by blood stream and not by direct nerve communication. When the vagus nerve on the mother animal was stimulated the change in mother's heart beat was almost immediate, and was followed ten or fifteen seconds later by a parallel change in the foetal heart beat.

Other phases of blood chemistry appear of considerable interest in disorders of personality. Timme finds that deprivation in the utilization of calcium (not necessarily in its availability) is associated with compulsive thoughtless, rapid, uncritical, and hence often unsocial behavior. This state is usually accompanied by high blood sugar, high blood pressure, and rapid fatigue. Another neurologist found a low calcium content of blood and spinal fluid in 150 cases of grave mental confusion. Dementia praecox and epileptics showed a normal pH, alkaline reserve, Cl and Ca in both blood and spinal fluid.

Two reports described methods of producing sleep. The first was electrical stimulation of the mid-brain of cats, which produced a drowiness so natural that the cats would not lie down on a damp bed to sleep but waited until a dry cloth was made available. In the other report, a profound sleep lasting three to six hours was produced by injection of a 5% solution of calcium chloride into the third ventricle of cats. Ergotamine had the same effect. Potassium chloride produced a state of agitation.

Students of problems of reading will be interested in some investigations pertaining to eye movement and word recognition. A lesion in the posterior half of the hemisphere, especially one that is supramarginal or in the angular gyrus, or which affects adjacent portions of the parietal, occipital or temporal lobes produced a type of eye-movement which was slow in its pursuit of moving objects, but relatively rapid in its return to and fixation upon the next oncoming object of the series. Lesions of the left supramarginal gyrus were reported by another speaker to be characterized by difficulty in repetition of words, difficulty in oral reading, verbal paraphasia, and the secondary disturbances in arithmetic and writing. A disturbance on the rim of the area striata was apparently responsible for a type of dyslexia in which the subject could grasp one symbol but could not coördinate one with the next and the next in a fashion to make reading possible. Disorders of motor speech, apart from verbal comprehension, were found in connection with tumors of the frontal spheroidal lobe, and with lesions in the Broca area. Of especial theoretical suggestiveness was the observation made during the course of the discussion by one German neurologist that a pathological subject, unable to name an object, does not respond at random. He is apt to give it a name that sounds like the correct one, or to name an object like the original in color, in size, shape, function, or otherwise associated.

The clinical psychologist finds after a week of discussion with the neurologists an increased alertness for organic disorders in his cases. He would always, of course, refer to a competent neurologist for examination any case showing exaggeration or diminution of any reflex, partial paralysis or anaesthesia, hypertonia or rigidity in some muscles, disturbed gait or posture, inadequate coördination, local headaches, restricted visual or other sensory fields, hallucinations involving any sensory channel. Even such symptoms as ready fatigue, aphasias, tendency to unwarranted euphoria or depression, marked alteration in personality, tendency to re-live scenes of earlier childhood, psychic automatism, restlessness, indifferent affectivity, drowsiness, increasing dullness or confusion, vertigo, loss of sphincter control, and memory failures, while by no means usually caused by neurological disorders, were included among the evidences presented for the diagnosis of organic disturbances. The clinical psychologist would probably find, after the neurological diagnosis had been made by the help of the best modern techniques, that, if the patient were likely to survive, the job of training him to some sort of usefulness and enjoyment in life in spite of his inevitable handicaps, must needs be passed back to the psychologist.

In the total number of papers contributed Germany led with 29, Italy and America followed with 28 each, France with 22, Austria with 17, Czechoslovakia with 15, Russia and Sweden with 13 each, Switzerland, England, and Poland with 12 each; Holland with 11, Hungary with 7, Spain and Portugal together with 6, and all others with 9.

As regards the topics treated, the figures are as follows. Under the head of *pathology*, the cause, mechanism, or course of some specific nervous disease or disorder, there were 70 papers, Italy leading with 11, followed by Germany and Russia with 7 each, America, France, Poland, and Holland with 6 each; Czechoslovakia-Roumania-Greece[1] and Switzerland with 4 each, Sweden-Norway-Denmark[2] and Spain-Portugal with 3 each, Austria and England with 2 each, Hungary with 1, and all others with 2. The greatest number of papers under this head dealt with infections, toxins, and lesions, 27 in all, Holland leading with 5, Germany following with 4, Italy, America, and France with 3 each; Czechoslovakia and Poland with 2 each; Hungary, England, Austria and Sweden with 1, and all others with 1. Next in frequency came papers on trauma and post-traumatic neuroses, 16 in all; Italy, France, and Russia with three each; Germany, Switzerland, and Spain-Portugal with 2 each; England with 1. Fourteen papers dealt with atypical and unusual cases; Poland with 4, Italy with 3, Czechoslovakia with 2, and Austria, Russia, Sweden, Poland, Holland, and Spain-Portugal with 1 each. There were 10 papers on tumors, gliomas, and the like, America, Russia, and Switzerland presenting 2 each; Germany, Italy, and Sweden 1 each, and all others 1. Only 3 papers dealt with glandular disorders, one each from Italy, America, and Russia.

Diagnostic technique stood next to pathology in the number of papers dealing with it, 54. There were 23 papers on brain puncture, fluid examination, X-ray examination, ventriculography, encephalography, chronaxy, etc.; 11 from Germany, 3 from America, 2 each from Sweden and Hungary, and 1 each from Italy, France, Austria, Russia, and Holland. Sixteen papers dealt with clinical signs; gait, motor and sensory disturbances, pains, sweat, body-type, etc.: 3 of them from France, 2 each from America, Czechoslovakia, and England, and 1 each from Germany, Italy, Austria, Russia, Sweden, Poland, and Hungary. Five papers dealt with body chemistry: acidity, sugar, albumen, phosphates, Wasserman, Nonne-Appelt, N, Cl, Ca, etc.: 2 of these from Austria and 1 each from America, Czechoslovakia, and Hungary. There were 6 papers dealing with psychological means of diagnosis; tests of intelligence, special abilities and handicaps, perception, handedness, etc.: 2 of them from America and 1 each from Germany, France, Russia, and Switzerland. Four papers, one each from Italy,

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[1] This group hereafter indicated in the text by *Czechoslovakia*.
[2] Hereafter in the text indicated by *Sweden*.

France, Czechoslovakia, and Spain-Portugal, came under the head of miscellaneous combinations of technique.

Under *general anatomical, physiological, and histological topics* 22 papers might be classed, 3 from Austria, 2 each from Germany, Italy, America, Sweden, Switzerland, England, and Poland; 1 each from Roumania, Russia, and Spain-Portugal; and 2 from other countries.

Tone and posture were discussed in 19 papers: 4 American, 3 each from France, Roumania, and England; 2 each from Italy and Holland, and 1 from countries classed as miscellaneous.

Eighteen papers dealt with *categories, classifications, comparisons,* and *outlines:* 4 of them from France, 3 from America, 2 each from Italy, Austria, and Spain-Portugal; 1 each from Germany, Russia, Sweden, England, and Poland.

Seventeen papers dealt with *therapy.* Eight of these had to do with surgery; 2 each from America and France and 1 each from Germany, Italy, England. Four were concerned with radiation, 1 each from Germany, France, Sweden, and England. Two had to do with drugs, from Italy and America respectively. One paper from Sweden dealt with malaria, and 1 from Austria with psychotherapy. Preventive sanitation and hygiene were the subject of 1 paper from Sweden.

Thirteen papers had to do with *localization of function in brain areas:* 2 each were from Germany, Italy, Czechoslovakia, Switzerland, and miscellaneous countries respectively and 1 each from America, Holland, and Spain-Portugal.

Finally, there were 21 papers classed under *miscellaneous,* including medical training, laboratory experiments, embryology, anthropometry, etc.: 5 from Austria, 4 from Italy, 2 each from Germany and Switzerland, 1 each from America, Czechoslovakia, Russia, Sweden, Poland, and Holland, and 2 from countries classed as miscellaneous.

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BOOK REVIEWS

Edited by Joseph Peterson, Peabody College

A B C de la Graphologie, I & II (Documents). By J. Crépieux-Jamin. Paris, Félix Alcan, 1929. I, pp. 357; II, pp. 368.

All readers who desire information concerning graphology at its best should read this lucidly organized book by the veteran graphologist, Crépieux-Jamin, and should study carefully the volume of documents that accompanies it. Even the most skeptical critic of graphology must acknowledge the curious problems projected by such an exhibition of graphic individuality. As an illustration of the inexhaustible variety graphic forms may assume, the author cites the possible variations that may occur in the writing of even one letter. It is possible, he asserts, to accumulate a thousand, ten thousand, or a hundred thousand specimens of the same capital letter without encountering any that are identical with each other.

The first and fundamental need for the student of handwriting in the face of such diversity is descriptive classification. Such classification involves a systematic procedure which slightly resembles systematic work in botany or zoology. It is, of course, keyed much less objectively. The author proceeds from seven genera, which give merely an outline of his program, to the species which are much more basal. Generic characters include speed, pressure, form, direction, dimension, continuity, and order (*l'ordonnance*). These genera are divided into 175 different species, each of which is described and illustrated. The species are differentiated in terms of a general form-quality of movement which one must *learn* to see. Instructions are given by means of which one may re-live a movement. Subordinate to the species are the modes, or *petits* signs of graphology, characteristics which the amateur sees first, and which are misleading. The more narrowly a sign is specialized, says Crépieux-Jamin, the less it is adapted to represent a fundamental trait of character. There are no particular independent signs of the comprehensive graphic pattern. One must begin with the species and not with the details.

Some of the species, described as qualitative, are, moreover, of much more fundamental significance than others, for example *l'écriture inégale* with all its diversity of nuances. Important, too, is the general quality which is discussed under the two headings *l'écriture organisée*, and *inorganisée*. In the latter discussion one approaches the problem of the development of writing from the immature to the mature hand. One may study the organization of an individual handwriting in the experience of the child, or see the reversed process as it occurs in the failure of control as old age sets in and progressively disorganizes the motor expression. There are other dominant categories which cannot be detailed here, except, perhaps, to state that in common with other students of graphic movement Crépieux-Jamin stresses speed and dynamic qualities. It is the habitual rhythm and degree of activity that the graphologist is seeking to penetrate. It should, of course, go without saying that all graphologists of standing insist upon ample documentation in an investigation; and are on the alert to recognize the accidental modifications of writing which occur under such conditions as writing in a cold room, on a moving train, or with different kinds of writing implements, on poor paper, or in a restricted space, and the like. The individuality that persists in spite of surface changes is what is sought by the graphologist.

From the descriptive side, Crépieux-Jamin's presentation is very valuable. But when one turns from description of the various patterns of highly individualized writing to a psychological interpretation of them in terms of personality one finds oneself on slippery ground. The new graphology no longer relies upon the graphic signs of the older graphology. It is interested in the form-quality of the writing taken as a whole, and not in the identification of unrelated details, the synthesis of which is a bewildering matter. A little has been done to test by objective methods the validity of the graphic signs of the older assumptions, but scarcely any attempt has been made to experiment on form-quality of writing although Binet made some approach to the problem. Crépieux-Jamin, while rejecting a purely analytical treatment of handwriting and emphasizing the need for a synthetic grasp of the whole, has little faith in so-called intuition and no faith at all in the amateur graphologist. He calls for prolonged training in the perception of form, and frequently relies upon an esthetic evaluation.

In the absence of any evidence proving or disproving the fundamental assumptions of the new graphologist—namely, that the form-quality which distinguishes the graphic movement will also be found distinguishing the personality taken as a whole—it is possible only to call attention to certain points upon which experimental evidence is actually at hand. Crépieux-Jamin treats reversed writing, or the back-slanted hand, in a fashion much more conservative than that of the older graphologists. He realizes that it may proceed from a number of different causes and cites some interesting statistics as regards the frequency with which it occurs among school children in France. He holds it to be more common among adolescent children than among adults. When it appears in the writing of adults he is inclined to interpret it in accordance with the old-time view as an evidence of emotional repression. Experimental work has, however, shown that a back-slanted hand is, in many cases, evidence of crossed dextrality, that is, opposition between the dominant hand and eye, or of latent sinistrality. It is not impossible that one may find temperamental patterns built upon inhibitions and difficulties due to conflict in kinesthetic impulses. Such, however, remains to be proved. In the meanwhile the explanation of back-slanted writing in terms of latent sinistrality is much more specific than the graphological explanation. I cite this as an illustration of the difference between the graphological and psychological approach to the same problem. In the same way, in the author's discussion of the graphological significance of misspelled words, he fails to reckon with statistical data available in the field of spelling-research.

One would like to see graphology and psychology come into more intimate relationship. The graphologist is, for the most part, ignorant of recent attempts to handle the problems of personality scientifically; the psychologist is blind to the nuances of graphic form. Can ground be found for a common experimental program?

University of Wyoming JUNE E. DOWNEY

Human Nature: A First Book in Psychology. By Max Schoen. New York, Harper & Bros., 1930. Pp. xviii, 504, 3 pl.

Frankly disclaiming all pretensions as a treatise, this book sets out with the avowed aim of establishing an *entente cordiale* with the reader. This objective it would appear fairly to have attained. Fluently written and well printed, with attractive type and ample margins, it prefaces each section with provocative summaries and suggestive questions. The elementary facts of the nervous system are concisely furnished, with accessory cuts of unusual clarity and bold lettering. Habit, instinct, emotion, personality—ever popular themes—receive fairly representative treatment, with copious citations from social, abnormal, and comparative psychology. Accounts of experiments in the psychology of music and questionaries on various traits are inserted—in such detail as to vitiate, one would suppose, the future scientific value of the latter.

These merits are achieved, however, at the expense of serious omissions and foreshortenings of content, jeopardizing the value of the volume as a college text. Though avowedly representing both 'structuralism' and behaviorism, and citing exponents of 'mind' (Russell, C. K. Ogden, Köhler) as frequently as Pavlov or Watson, the author does scant justice to the typical experimental attack of the older, stabler tradition—the use of hypothesis, standardization and ordered variation of conditions. In the fields of feeling, action, and imagination, facile verbal solutions or opinions replace exact analysis of present-day enigmas; and rationalization and defence mechanisms are accorded fuller space than 'thinking' proper. The doctrines of heredity and 'unit traits' are cavalierly treated in an early section, only to be quietly readmitted as basic truths, when needed later to explicate the variants in musical talent and in personality.

The logic of the author's terminology is often difficult to decipher. Wide departures from current usage in the handling of such stock terms as *attribute, inference, accommodation, psycho-physical measurement* occur (pp. 184, 16, 187, 31), with no discoverable gain. Cut to the capacity and tastes of the lay reader rather than the student, the treatment on the whole is of a nature to repel rather than attract the type of recruit sorely needed in the psychological field today—the individual of curious yet critical and constructive bent.

A booklet of clever and suggestive "Learning Exercises," cast in the mould of the familiar 'true and false,' multiple choice, and similar forms, has been devised by the author and his staff to accompany the volume. There are suitable exercises, designed for individual use by the student, for each of the main topics, running in all to 38 pages.

Cornell University — Elsie Murray

Geniale Menschen. By Ernst Kretschmer. Berlin, Julius Springer, 1929. Pp. viii, 254.

In *Geniale Menschen* Kretschmer has given us a scholarly and systematic treatise on genius, written from a scientific point of view. By his repeated emphasis upon objective data and his substitution of scientific method for purely philosophical speculation, the author has made a significant advance in the direction of a scientific psychology of genius.

The problem of the study is "to demonstrate the laws governing genius and its inheritance, and thereby present a plastic true-to-life likeness of creative personalities." The treatment follows the lines suggested in the chapter on "Die Genialen" in *Körperbau und Charakter*, 1921. The scope, however, is much broader, including all the major problems of genius, of which the question of psychosomatic types is only one.

Kretschmer's criterion of genius is two-fold: (1) the possession of certain unique qualities of personality, and (2) a positive reaction of the social group to these qualities. The essence of genius is a specially structured mind, which produces positive vital values, that bear the stamp of an unique and unusual individuality.

The main body of the book is divided into three parts. The first, entitled "Laws" is systematic, dealing with particular problems of genius. The first chapter is devoted to a discussion of the relations between genius and insanity. Kretschmer expressly denies Lombroso's contention that all genius is insane, yet in the end his own position comes very close to Lombroso's. The formula for genius is high intellectual endowment plus the *demonic* element. The *demonic* is defined as that element which finds expression in the strange mental activities, the unusual ideas, and the uncommon sorrows, characteristic of genius. Precisely how *demonic* traits differ from psychopathic traits, Kretschmer never makes clear. On the contrary, he tells us that the demonic and psychopathic elements are very closely related, sometimes inseparable. Some *demonic* traits are identical with psychopathic traits. Nevertheless, there is no necessary 'inner' relation between insanity and genius. The typical genius is insane, but his insanity is not essential to his genius. The argument is not convincing. Whatever might be gained by postulating a *demonic* element seems to be lost in the author's failure to draw any clear distinction between it and the psychopathic element. In the absence of any criterion of the *demonic*, Kretschmer's position approaches Lombroso's in fact if not in words.

Other problems discussed in Part I are abnormalities of the sexual impulse, constitutional types, the inheritance of talent, and the relation between genius and race. One is led to believe that the majority of geniuses have some form of sexual abnormality. In theoretical interpretation of sex functions, Kretschmer leans heavily upon Freud. The author's theory of constitutional types is briefly restated, and the reader is referred to *Körperbau und Charakter* for a more detailed discussion of the subject. Regarding the inheritance of talent, Kretschmer suggests that the high frequency of genius among the offspring of unions of diverse family stocks is due to the fact that such unions produce individuals of complicated psychological structure, inner contrasts and conflicts, restlessness and instability. As for the relation between genius and race, the author proposes a theory based upon the geographical distribution of genius. Genius and the highly developed culture which invariably goes with it occurs most frequently in zones of racial mixture, in which there is a high degree of intermarriage between two diverse races. Within the domains of the Nordic race, the highest cultural centers appear, not where the race is purest, but where it is highly mixed with other gifted races, especially the Alpine. Thus Kretschmer's "objective statistical method" leads to a refutation of the popular theory of Nordic supremacy. Unfortunately, the statistical evidence is not included, so that it is impossible to judge the adequacy of his method of selecting data. Racial types are not identical with constitutional types. There are various constitutional types in every race, with some one type probably predominant. The Nordic race, for example, shows a large proportion of the asthenic type of physique; the Alpine race, a large proportion of the pyknic.

In analyzing the physical and mental traits of the peoples in the zones of Nordic-Alpine mixture, Kretschmer attempts to demonstrate a correlation between ratio of Nordic-Alpine blood, type of temperament, and form of genius. In regions where the Nordic component is predominant, the temperament of the people is relatively 'cooler,' and here we find the center of political talent and techno-scientific development, (England, northern Germany, and North America). In regions of Alpine predominance, on the other hand, we find a 'warmer' temperament—the center of artistic culture, (middle-southern Germany, middle-upper Italy, and France). These generalizations are interesting, but one misses corroborative statistics.

In Part II, certain important trends and types of genius are illustrated by reference to the lives of famous geniuses. Goethe's life is cited as an example of periodic fluctuations in the productivity of genius. The scientific genius is typified by Robert Mayer; the heroic leader of men, by Bismarck, Luther, and Robespierre; the mystically inspired genius, by Hölderlin; the prophet, by Rousseau. The author pauses briefly in his portrait painting to argue that the phenomenon of genius is limited almost exclusively to the masculine sex. A few women, he grants, have done original and creative work, but they were essentially mannish. "The great women were great because they were great men," is his conclusion. Queen Elizabeth of England, Christine of Sweden, and the German poet Annette von Droste-Hülshoff, are presented as evidence.

The portrait collection of Part III has a brief explanatory preface. Philosophers show a strong tendency toward the leptosome (asthenic) type of physique. Athletics and pyknics seldom occur. Among natural scientists, on the other hand, we find pyknics and mixed forms in abundance, especially in the biological sciences. In the more abstract sciences, such as mathematical physics, the leptosomes predominate. Here again one feels the need of statistical evidence in support of the generalizations. The portrait collection itself is excellent, adding considerably to the value of the book. It contains pictures of all those geniuses who have received particular attention in the text and many others. Strangely enough, there are four women among them. But perhaps they all had mannish temperaments! The pictures are grouped according to type of productivity under five headings: (1) philosophers, (2) physicians and biologists, (3) dramatists and pathetics, (4) romanticists and stylists, (5) realists and humorists. There are 80 portraits in all.

Smith College E. FRANCES WELLS

A Primer of Aesthetics: Logical Approaches to a Philosophy of Art. By LOUIS GRUDIN. New York, Covici, Friede, 1930. Pp. xvi, 247.

Beauty: An Interpretation of Art and the Imaginative Life. By HELEN H. PARKHURST. New York, Harcourt, Brace & Co., 1930. Pp. xi, 336, 47 pl.

Of these two volumes, the latter skirts more nearly the psychologist's domain. Essentially an elaboration of a theosophical formula—the duality of the cosmos—it translates all beauty into a juxtaposition of antitheses (movement *vs.* permanence, order *vs.* chaos, unrest *vs.* stability). Art is, as it were, a microcosm reflecting or harmonizing the conflict inherent in nature and human consciousness. There is little explicit reference to either psychological or philosophical theory, and the efforts of the experimentalist are consistently ignored. Considerable space, however, is devoted to enthusiastic and suggestive cataloguing of the sensuous qualities with which the artist works; to eloquent inventory also of the permutations of time and space and movement on the adroit handling of which the charm of sculpture, music and architecture depends. A wide familiarity with the various fields of art and of art criticism is apparent, and the text is enriched with reproductions of classic works of art, and photographs of modern buildings and of natural objects. The treatment of form and of artistic creation is, however, disappointing, the author lapsing too often into a mystical or dithyrambic vein. Few writers in this field, it may be added, command a more flexible and extended glossary of all the nuances of mood and feeling.

Grudin's text, in marked contrast, maintains a deliberate aloofness from the concrete and specific. Inspired, apparently, by the poet-critic Valéry's *Varieté*, the author sets out to forge an idiom of logical abstractions, unique to aesthetics, which shall redeem this field once and for all from the intrusion of the literary critic or the metaphysician. The problem of aesthetics is a problem of symbolism, of meanings in a set contextual field. Psychological aid is explicitly repudiated as irrelevant (this position grows more intelligible as one proceeds and the naïveté of Grudin's psychological concepts becomes apparent). Part I grapples with Russell and Bergson, with verbal logic and space and time and meaning. In Part II (Introduction to a Theory of Aesthetics), at the close, current views receive brief and sometimes trenchant criticism. Definitions of art as illusory, intuitional, utilitarian and moralistic, an escape phenomenon, an aberration, a function of the ego or a specific order of behavior (the latter derived from the limited context of 'the laboratory world of psychology') are all dismissed as tending to "reduce beauty to one or another set of attributes, without suspecting that beauty escapes reduction to any set of attributes whatever . . . but is definable only by means of a conjugation of such reductions through a series of their transformations." One is left with the impression that the initial optimistic promise of the text, 'to establish aesthetics for the first time as a field of authentic and verifiable knowledge,' remains unfulfilled.

Cornell University ELSIE MURRAY

Ensayo de Piscologia de la Adolescencia. By E. A. CHAVEZ. Mexico City, Secretario de Educacion Publica, 1928. Pp. 482.

This volume is written by a man who was intimately connected with the educational affairs of Mexico for about thirty-five years—sub-secretary of Education, rector of the National University, director of the Facultad de Altos Estudios, etc. It is he to whom James Mark Baldwin dedicates his *History of Psychology* as to "the great patriot and scholar." Chavez is also a translator of Titchener's *Text-Book* into Spanish. The present essay is something like a collection of independent sermons which draw on a variety of sources. Like a preaching philosopher, the author does not care about concrete material and experiments, tables, surveys, or case studies. His book is a message of good will "directed to the peoples of the earth, to classes of the nations, to parents and children." The world is in a state of moral anarchy, "and it is the duty of the present day educator to show the dangers and to put the present generation on the single right track."

The titles of the four large parts are as follows: (1) Adolescence and Life, (2) Inference and Corollaries, (3) Cardinal Phenomena of Mental Life, (4) The Highest Structures of Psychic Life. The factual material of the first two parts approaches the typical layout of a textbook such as that by Pechstein and Mc-

Gregor, and the data are almost entirely from non-Mexican sources. Only a few items refer to Uribe y Torosko and to Santamarina, for example, when the prepuberal increase of weight in girls is represented as being about one and one-half year earlier in Mexico than in cooler countries. There is also a section on the recent history of secondary education in Mexico City. The author seems to favor very definitely the separation of sexes in school life without any artificial separation outside of school. The third part of the book reveals the definite leaning of the author towards McDougall's hormic postulates. The formation and dynamics of Freudian complexes is explained as the tendency of unfinished acts toward "closure" in the sense of the Gestaltists. Superior forms of love, the question of sex education, and a criticism of the "so-called philosophical teaching" which unjustly, according to the author, discredits the Catholic Church, are interesting features of the discussion. The drive toward atheism is said to be motivated by the aversion toward restrictions which good morals impose upon people; and the overthrow of religion generally, to be followed by the development of low grade superstitions. Such superstitions are explained psychologically by the author on the principle "that when a more noble and higher function is affected it is replaced by the lower functions which reach a greater development."

It is characteristic both of the esteem which the author is enjoying in his country and of the great tolerance of the Mexican Government today, that Dr. Chavez's essays were published by the Department of Education. There is not a word, however, on the "school of action" which was proclaimed as the official 'school' in Mexico; neither was mention made of the psychological laboratories founded during the administration of President Obregon.

Lewis Institute, Chicago D. BODER

Die Psychologie der produktiven Persönlichkeit. By PAUL PLAUT. Stuttgart, Ferdinand Enke, 1929. Pp. viii, 324.

A methodological study of the principles and methods of the psychology of art involved the author in problems of psychography, and the accumulation of materials which suggested the need for a psychology of productive personality. The author formulates this in detail, although he regards his work as tentative and perhaps as a mere framework for future investigations. Creative personality—the man of genius—has always intrigued the professional psychologist, as well as the layman. Plaut's work combines the best features of previous discussions in this field. He does not rest satisfied with mere concepts of genius, talent or intelligence, but investigates concrete personalities displaying creative abilities. He appeals to the living genius for data. Since art and science are particularly favorable to the appearance of productive activity, the author seeks his material among scientists and artists who answered questionaries concerning their motives and methods of work. Plaut's book, however, is very comprehensive and he prefaces the results of his psychographic studies with a historical survey of genius and intelligence. He shows how the mythological notion of a genius or external spirit connected with man in his exalted moments is partially transformed by Plato into a conception of psychological endowment, in which most subsequent ideas of genius were present in germ. Later Greeks reduced it to a function of reason and no progress was made until the Renaissance. From that time, French, German, and English aestheticians and several of the French encyclopedists elaborated conceptions of genius varying in detail but reducible to the two categories of rationalistic and mystical. The author dismisses the literature on the relation of genius to madness as irrelevant to the psychology of productive personality. He criticises the whole literature on the psychology of genius for its failure to develop anything more than an abstract and dehumanized concept indistinguishable for practical purposes from the mythological notion which was its source.

Intelligence is accorded a much briefer treatment, and the definitions and discussions of German investigators, such as Ziehen, Ebbinghaus, Meumann, Stern, and Lipmann receive most consideration. The influence of temperament and character on intelligence is duly recognized and the author offers a comparative study of intelligence types. He concludes that the psychology of intelligence has erred in the opposite direction to that of the psychology of genius. It suffers from excessive analysis resulting in the dissolution of personality into its elements. This is dehumanization of another kind. Neither the students of genius nor those of intelligence present a psychology of personality. This can only be worked out in terms of what seems to be a configurational psychology.

This brings us to the author's own contribution to the study of productive personality. Modern methods of personality study such as the individual psychology of Adler, the mass psychology of Moede, the type theory of Kretschmer, and the personalistic and allied methods, are stated and criticised, after which the psychology of the productive process in scientists and artists is elaborated in the light of such criticism. The psychology of the scientific personality, as expressed in natural science, philosophy of law, history, and religion, reveals criteria of productivity which are strictly normal, in spite of the fancies of the psychoanalysts. Brief chapters correlate the views of eminent scientists on productive types and processes. Artistic creation is the subject of more extensive treatment. Leading poets, painters, sculptors and architects contribute detailed material on the experience of the artist, and the sources of his mastery of aesthetic form. Sections follow on the typology of artistic creation and the nature of the processes involved. The author finally concludes that productivity emerges from a harmony of personality and its objects. It is a function of the total *Gestalt*. In activity, or will, lies the final formula of productive personality.

The final section of the work presents the views of contemporary artists and scientists on their personal methods of creation. Most of the writers are Germans, but there are contributions by Knut Hamsun and Miguel de Unamuno. Many of the contributions are of great interest and several contributors are frankly skeptical of the value of experimental psychology in this connection. In summarizing a vast mass of data, raising fundamental problems of criticism, and offering the beginnings of new approaches, Plaut has produced a work which will be indispensable to all students of personality.

University of Cincinnati CHARLES M. DISERENS

Wirkungsformen der Übung: Beiträge zur Psychologie der Arbeit. By BENNO KERN. Munster, Helios, 1930. Pp. ix, 508.

This monograph deals with the influence of practice on performance, and is based on the author's experience with aptitude testing during the war, and later with the students in the Westphalian schools. The first part is a theoretical discussion of certain obstacles encountered in performance testing—differences in subjects' mental set, instability of initial performance, fatigue, differences in practice improvement of individuals relative to one another, etc.—and methods of controlling them. A discussion of the pertinent psychological literature, mostly German, accompanies each topic. There is very little that is new and much is omitted.

The second part describes a series of experimental studies on practice effect by the author and his collaborators. The justification for them is hard to discover. The groups of subjects are small and the tests limited in scope. Correlations are worked on less than ten cases and sweeping conclusions drawn therefrom. It is concluded on the basis of ten cases that girls are more accurate than boys. In arranging the successive studies in this series, the author was evidently trying to vary the number of trials at a sitting, frequency of sittings, length of rests between, and age and sex of subjects, in some unsystematic way. The results are presented graphically, demonstrating individual differences in the rapidity of the attainment of a stability of performance level.

The following conclusions are drawn: (1) the ontogenetic law of practice—initial efficiency rises with age, but practice effect decreases with age; (2) the differential law of practice—within any age group low initial efficiency is associated with large practice gain, and vice versa, due to the operation of initial inhibitions; (3) the law of practice consequence—the function is affected both as a whole and in parts; and (4) the limits of improvement—these limits are seldom reached, and an individual's initial level is no criterion of his stable ability. Hence performance tests can at present do little more than roughly to separate good and poor groups, without accurately diagnosing individuals. Certain improvements in procedure are suggested, such as arousing the will to improve, by group rivalry; introducing practice, spread over a long stretch of time, by utilizing alternate forms of tests until a more stable level is reached.

In fairness to the author, we must admit his central criticism, that unique samples of performance are never very reliable measures of ability, and that practice changes relative levels; although as Thorndike and others have shown, it is quite as likely to exaggerate preëxisting differences as to obliterate them. But did any psychologist ever naïvely assume that such samples of performance had any more than a rough classificatory significance? The point is that a test is only useful as a time-saver, and has no advantage over an observation of efficiency on the job itself, if the testing must be carried over several weeks. As to the author's ten years' research program, it was more or less gratuitous, since he has discovered nothing which is not already familiar to students of the practice function, as a result of more systematic experimentation. The five hundred pages could well have been reduced to fifty.

University of Chicago ARTHUR G. BILLS

Differential Manipulation in Children. By JULIA HEIL HEINLEIN. Comparative Psychology Monographs, 7, 1930, (no. 33), 1-121.

This is an investigation of the preferential 'handedness' in various motor activities of pre-school children, in relation to certain training effects. Observations were made under controlled situations as well as in daily activities of the children's own choice. Mixed types of individuals are now well-known, whatever may prove to be the respective rôles of germinal and of training factors in their acts. Downey found by classifying adults and children according to the Rife scheme that these two groups show similar distributions of dextrality types.

The monograph in hand presents results on 24 children of ages 2 to 6 years (16 right-handed, 6 left-handed, 2 ambidextrous) in three different experiments: (1) offering equal opportunity for the use of either hand (marble-board test), (2) imposing inconvenience on the use of the preferred hand (in a form-board test), and (3) requiring rapid, spontaneous choice of the hand to be used (children were to run and pick up some object and bring it as quickly as possible). Different variations in the tests and detailed analyses of the responses are considered, as well as effects of practice on changes in handedness. The adequacy of the tests were judged by the degree of agreement of their results with the reports of handedness by parents. In (1) and (3) above, a pronounced tendency by both right- and left-handed children to use the preferred hand was found; but less consistent results came from (2). In all three experiments considerable variability in degree of bias was noted, convenience being an important factor.

Twenty-one children were also trained on a wooden peg-board to set pegs of different colors (B, O, G) one at a time, as quickly as possible, into holes in parts of the board having the same color. Each child had one practice with the non-preferred hand each day for 5 days, and also a beginning and an end test with the preferred hand. Improvement in time scores was shown on the non-preferred, practiced hand by both the right-handed (17 *Ss*) and, less certainly, the left-handed (4 *Ss*) group. The left-handed group, however, showed greater speed with the right hand on the first day than with the left hand. Moreover, the non-practiced (right) hand of the former group showed an improvement approximately equal to that of the practiced (non-preferred) hand. This may be due in part to transfer, to practice in the tests, to general maturity, or to certain irregular factors. One might expect the non-preferred hand to improve more than the preferred hand, even with equal practice. The left-handed group was too small for any serious consideration of results. This applies also to other experiments on training against dextrality tendencies, which therefore need not be considered here. Motivation in these experiments seems not to have been well controlled, due to several difficulties considered by the author. The results of observations on dextrality in uncontrolled activities agree closely with reports by parents.

The monograph could have been very much clarified and reduced in size by better tabulation and statistical handling of results, and by elimination of many irrelevancies and much confusing repetition. Its contributions, as it is, are not obvious.

J. P.

Common Annoyances: A Psychological Study of Everyday Aversions and Irritations. By Hulsey Cason. Psychological Monographs, 40, 1930, (no. 182), 1-218.

After arranging two lists of 239 and 268 annoyances, collected from 659 subjects, Cason presented them for scoring to 625 and 378 subjects, respectively. From these data he obtained an annoyance score for each stimulus-word, and also an annoyance score for each individual tested.

His method of scoring both the stimulus-word and the individual records is identical. Since in both cases his range of possible scores falls between zero and thirty, some of his comparisons are more significant than they appear at first glance. For methods of comparison, Cason divided his subjects, running in years of age from ten to ninety, into four age-groups for each sex. In no case did a group contain more than one hundred and five subjects, and eleven of his sixteen groups contained fewer than seventy-five cases. Such small groups lessen the validity of his conclusions, particularly in an introspective test of this type.

More than half of the monograph is devoted to a discussion of these annoyances, based to some extent on data collected from some five hundred subjects. These data are not presented for examination, nor can we assume that they came from those subjects who scored in the test.

From his original data, Cason concludes that human behavior constitutes over one half of our annoyances, non-human things and activities, combined with clothes and manner of dress, account for more than a third, while only about one eighth of our annoyances are due to either alterable or persisting physical characteristics of people.

No attempt was made to check the reliability of the test by having it scored a second time. It is unfortunate that the raw scores are not translated into standard deviation units or into percentile tables, and that no attempt is made to prepare diagnostic tests from the material collected.

Alabama State College MARIE HACKL MEANS

Heredity in Man. By R. RUGGLES GATES. New York, The Macmillan Co., 1929. Pp. xiii, 385.

The book is a mine of information concerning researches in heredity in man. In fact, at times it resembles an abstract journal, in that the various researches are not always searchingly criticized nor brought together into any synthesis permitting a final or unified view. In justification the author could no doubt truthfully plead that in many, if not most, cases our present knowledge is too meager to justify any evaluations even approximating finality.

Although the author expresses himself in the first chapter as believing in statistical or biometrical studies as well as in the analysis of pedigrees, only a smattering of studies of the former sort are included, and these few are not always analyzed as critically as might be. Throughout, the tendency is to assume Mendelian inheritance, but the author carefully avoids conclusions as to the exact mechanism of inheritance when not warranted by his facts.

The psychologist will learn little that is new concerning the inheritance of mental differences—only one of the sixteen chapters dealing with this subject. The remaining chapters deal with the inheritance of physical traits such as eye color, blood, abnormalities and diseases, and with racial crossing. The psychologist will, however, find a number of very interesting suggestions and discussions, as, for example, the discussion of the relationship between available knowledge of the general and specific factors in mental ability, on the one hand, and that of the mechanics of inheritance of mental traits on the other. Particularly in the discussion of the inheritance of 'musical ability' does he bring out the handicap under which the geneticists labor in attempting to determine the nature of inheritance of a trait which is not a unitary trait but a composite of an unknown number of factors. "The study of the inheritance of musical ability thus appears to be swamped at present in the welter of components into which it has been analyzed."

University of Illinois HERBERT WOODROW

Über das Wesen der Beanlagung und ihre methodische Erforschung. By THEODOR ZIEHEN. Friedrich Mann's Pädagogisches Magazine, Heft 683. Langensalza, Hermann Beyer & Sons, 1929. Pp. 38.

In this pamphlet Ziehen sets forth a psychological analysis of endowment (*Beanlagung* in the widest sense or *Veranlagung*) distinguished from talent (*Beanlagung* in the narrow sense or *Begabung*). Such an analysis he holds is the proper basis for the construction of tests of potentiality. He then states 15 principles covering the procedures to be followed in the systematic examination of school children and the development of better tests for the determination of vocational and educational aptitude. Finally, he gives examples of such tests in ten fields from Mathematics to Volitional Energy. Under each head several analytical test procedures are described with comments which suggest extensive experience and keen insight on the part of the author. This last part is especially rich with practical suggestions for research in this field, where as Ziehen rightly says "out of more than 150 recommended tests scarcely two dozen are satisfactory."

Points of outstanding psychological interest are the author's insistence on analysis, well summarized in his statement, "a general intelligence (general ability, Spearman) does not exist." In part this may be explained by his rejection of correlation coefficients as adequate means of representing mental relationships. He holds that psychic performances cannot be treated numerically as additive and divisible quantities. In this and in his frequent use of combination as a basic factor in all perceptual and conceptual activities, he seems to lean toward the Gestalt point of view quite in contrast to the emphasis on analysis just mentioned. He favors individual examinations and trusts the psychological training of the examiner more than test standardization, although he urges the importance of age norms.

The monograph is evidently a summary of psychological methods of determining potentiality based on wide clinical experience and close acquaintance with an extensive international literature. Evidence for the statements advanced and statistical data on the tests described are provided only in the bibliography of 67 references, chiefly to German periodicals.

University of Oregon HOWARD R. TAYLOR

Grundfragen der Wahrnehmungslehre. By PAUL FERDINAND LINKE. Second edition. Munich, Ernst Reinhardt, 1929. Pp. xxvi, 430.

This work may be appraised best in comparison with textbooks. In general the latter aim to present the fundamentals of psychology which should be the common basic currency of the science for every student. But this aim is almost without exception rendered futile by the systematic convictions of the writer. Thus textbooks tend to become special expositions of, and arguments for, a particular system like introspective atomism, behavioristic atomism, functionalism, *Gestalt*, etc. The selection of experimental material in each such book becomes prejudiced and inadequate, and logical consistency together with considerations of historical development distort the presentation. This may be a venial offence, inescapable perhaps because of the immature character of psychology. But it is possible that the offence may be more traditional than inescapable; for in textbooks on physics and chemistry and even biology fortuitous impressions and convictions are not so frequently permitted to interfere with the presentation of fundamentals, and a fair uniformity is achieved, at least in the elementary works.

The especial virtue of Linke's work lies in the circumstance that it in no way presumes to be a textbook or a book of fundamentals, but frankly declares itself polemical. Assuming a fundamental groundwork, its primary concern is with the application of Husserl's phenomenology to the problems of perception and particularly to the problems raised by Gestalt. The openly controversial tone of the book is everywhere manifest and constitutes its justification. The work is determined by a freely announced program of ignoring all genetic, and principally physiological, considerations. Throughout emphasis is placed on the significance of experimental data, particularly those published since 1919, the date of the appearance of the first edition. As one might expect from Linke's pioneering studies in movement, a great deal of attention is paid to this subject, with the especial purpose of rebutting Wertheimer. Obviously there is a place in psychology for such a book which, though too frequently unconvincing, warmly and capably presents the arguments for phenomenology.

University of Louisville ELLIS FREEMAN

Das Wesen der Ironie. Eine Strukturanalyse ihrer Erscheinungsformen. By R. JANCKE. Leipzig, Barth, 1929. Pp. vi, 111.

Jancke's study of irony cannot be appreciated without recourse to the methods generally used in phenomenological writings, and especially not without the categories developed in Pfänder's *Psychologie der Gesinnungen.* The author seeks to determine the nature of "irony" by analyzing the various forms of irony employed in social intercourse, and the relations between irony and such phenomena as humor, sarcasm, cynicism, etc. A "lesser" and a "greater" irony are distinguished. The lesser irony is, for example, always a mixture of "manifest" and "covered-up" irony. In case the ironical attitude is permanently assumed by a person, we have the greater irony. The greater irony is a *Lebensgefühl.*

It is not possible here to evaluate critically the phenomenological approach to the study of attitudes, feelings, and emotions in general. The value of Jancke's book is inextricably linked up with the results of such a critique. It may be said at least that, no matter how questionable some of the findings of the phenomenologists are, studies of this kind bring home to us the complexity of emotional states, a complexity which is often overlooked in experimental research for the sake of obtaining "measures of" emotions.

Behavior Research Fund HEINRICH KLÜVER

Essai sur la logique de l'invention dans les sciences. By JAQUES PICARD. Paris, Félix Alcan, 1928. Pp. 288.

The present essay by M. Picard, which is one of a series in the *Bibliotheque de Philosophie Contemporaire,* attempts to show that logic, far from being merely the science of proof, is even more the science of discovery. The author argues that the methods of logic have combined with precise experimental methods in the discovery of new facts and in the progress of science. A wealth of evidence is adduced from the history of astronomy, physics, chemistry, medicine and from mechanical invention to show that scientific method embraces both induction and deduction regarded from the standpoint of discovery rather than from that of proof.

Logicians and philosophers since Bacon have regarded induction as the method peculiarly adapted for scientific discovery, but have treated the deductive method as the method of proof. Picard finds abundant evidence in the history of the sciences for opposing this view. Many discoveries in astronomy, physics, and chemistry have resulted from deductions based on known facts and laws or on assumptions which have been suggested. It was in large part by deduction that Galileo was led to the discovery of the laws of falling bodies, and Maxwell to the discovery of the electromagnetic wave. "A mathematical law is of greater importance than an ordinary discovery," wrote Arago, "for it is itself a source of discovery."

The essay is an elementary and concrete study of the logical method as illustrated in the history and progress of science and invention rather than a formal and abstract treatment of the laws of thinking. A bibliography of seventy-two titles is appended.

University of Montana FRANKLIN O. SMITH

A Point Scale of Performance Tests: Vol. I, Clinical Manual. By GRACE ARTHUR. New York, The Commonwealth Fund, Division of Publications, 1930. Pp. ix, 82.

This manual has been prepared to meet the need for a non-verbal scale of individual tests, comparable, in its field, to the Binet scale for children suffering no school training or language handicaps. The present scale of performance tests seeks to improve upon previous performance tests by adequate standardization against a large sampling of representative public school children. The inference from the discussion (comparing I.Q. of testees with Binet I.Q.) is that this test is the same sort of 'general intelligence' test as the Binet scale, except, perhaps, in cases having special forms of "non-verbal abilities." It should also be noted that in earlier preliminary reports of this scale it was called An Absolute Intelligence Scale.

The manual contains two forms of the performance scale. Form I consists of the following tests: Knox Cube, Seguin Form Board, Pintner Two-Figure Form Board, Knox Casuist Form Board, Pintner Manikin, Knox and Kempf Feature-Profile, Healy Mare and Foal (modified), Healy Picture Completion I, Porteus Maze, and The Kohs Block Design; and Form II, of Knox Cube (reversed), Seguin Form Board, Gwyn Triangle, Paterson Five-Figure Form Board, Glueck Ship Picture Form Board, Healy Picture Completion II, Porteus Mental Maze (number at left of subject), and Kohs Block Design (designs inverted). Form I was standardised on responses by 1,100 children and Form II on responses by 535 of the same children about a year later. "For fifty percent of the cases tested on Form I, the I.Q. falls within five points of the I.Q. obtained on either the Kuhlmann-Binet or the Stanford-Binet scale" (p. 4). The P.E. of measurement between Forms I and II ranges from 4.52 I.Q. points for the 6-year-old children to 8.45 for the 14-year-olds. For the highest age-group (16 years) it is 7.97 points. Careful descriptions of the tests and materials and directions for use are contained in the *Manual.*

While the scale is designed mainly as a supplement to the Binet tests, particularly as to the rating of children deprived of the usual school and language training, it is also said to be serviceable in cases of children requiring special diagnostic tests, and as a means of breaking down timidity and of establishing interest and proper test-relationships for the application of the Binet scale.

The present volume is intended for use by adequately trained clinicians. A second volume is promised which will give information on the details of the scale construction and standardization. It supplies age-norms for the point scores covering chronological ages 5.5 through 15.5 years for Form I, and 6.5 through 16.5 for Form II, with some downward and some upward extrapolations printed in italics.

It is known that the reliability of some of the performance tests used is low. One wonders, in the absence of the promised data of Volume II, whether the selections of specific tests are the most happy in some cases. Criticisms on this score, however, are as yet out of place. The discriminative value of each test is measured by a formula involving P.E. of distributions but there is no correction of these P.E. for unreliability. J. P.

Krankhafte Seelenzustände beim Kinde: Grundlagen für das Verständnis intellektueller und sittlicher Regelwidrigkeiten. By O. Hermann. 3rd. ed. Langensalza, Beyer & Sons, 1930. Pp. xv, 252, 7 pl.

The major portion of this monograph (208 pages) is devoted to an account of various forms of mental impairment resulting from "outer" causes. A second section of 44 pages describes mental abnormalities resulting from "inner" causes, "for the most part of an hereditary nature." The first group includes all forms of mental disorder or defect which have or may be assumed to have a physical basis, such as hydrocephaly, microcephaly, mongolism, cretinism, etc. General mental deficiency of all grades, and special disabilities such as alexia and agraphia, are also included in this group, since Hermann assumes that these are the result of cerebral deficiencies, either general or confined to specific brain-areas. In addition he lists a small group of "mental causes of mental impairment" in which gross bodily accompaniments are lacking.

Under the second head are included lack of self-confidence, oversensitivity, the "psychopathic constitution" etc. Neither the grouping nor the point of view upon which it is based would be accepted by most American psychologists of today.

University of Minnesota Florence L. Goodenough

The Enlargement of Personality: Behavior Patterns and Their Formation. By J. H. Denison. New York, Charles Scribner's Sons, 1930. Pp. xvii, 340.

The author's thesis may be very briefly, and baldly, stated thus: personality is the behavior-pattern of the individual. This pattern is due to the influences of environment playing upon one's capacities. Included in these influences are: the family; childhood gangs; tribal beliefs (taboos, social standards, ideas of caste); and the individual's concepts of himself (standards, ideals, etc.). One's personality may be changed in some cases by auto-suggestion, in other cases by the influence of religion or other strong motivating factors, and in still others by a carefully planned objective involving standards rigidly adhered to. The method of changing depends upon the individual's receptivity to suggestion. Emotions are important in giving intensity to various behavior patterns.

Stated in such fashion, the text presents very little that is new to the professional psychologist. There is a large amount of anecdotal material giving point to the above thesis and making it very readable. The lack of specific citation of sources is not necessarily a drawback to an avowedly popular book such as this is.

While there are many minor details and statements with which most psychologists would disagree, taken all in all the book represents a fairly sound popular treatment of the subject. This opinion is given in spite of a prejudice aroused by the publishers' statement: "Here is explained a successfully tested method for becoming the person you would like to be, for attaining happiness, peace, or whatever your *real* objective has been."

University of Pittsburgh J. H. McFadden

Introduction to the Psychology of the Classroom. By Charles E. Holley. Boston, D. C. Heath & Co., 1930. Pp. xiii, 257.

This book is obviously of interest to those concerned only casually with applications of psychology in the schoolroom, rather than to those interested in the presentation of a system of scientific facts. It is evident that the author is more educationalist, of a practical sort, than psychologist. In the main the facts and suggested principles upon which the educational applications rely are true, and the interest of the book for the psychologist is found in the varying aptness with which the author shows how the principles apply. As to the so-called instincts he is somewhat too vague and perhaps too confident, and he is a little disregardful of the latest findings respecting sex differences. There are numerous statements made in good faith which many real psychologists would hesitate to make. It seems to the reviewer, moreover, that the author, being an educationalist of the practical sort, is disposed to give accepted fact a turn to suit his practical purposes, which may be not only unwise but also unwarranted.

University of Denver Thomas R. Garth

Social Psychology. By J. J. Smith. Boston, Richard G. Badger, 1930. Pp. xxv, 468.

This treatise is characterized by the author as "the psychology of social attraction and repulsion," and aims to present the field of personal and social relationships from the point of view of psychology. It outlines a systematic point of view in which the impulses and feelings, organized into sentiments approximately after the manner of Shand and McDougall, are basic. Desirable sentiments may evolve in genetic growth and in the modification of innate tendencies. An understanding of the organization of sentiments reveals their relation to stability of social groups and relations between individuals and groups.

The author's discussion becomes somewhat incoherent at times, due perhaps to the fact that he makes generous use of quotations and citations. Many pages have from four to six of these. All but five of approximately 250 references are books, and no notice is given to contemporary trends in social psychology as revealed in recent journal literature. Although the author states that "psychology must be based on ascertained fact" his arguments are, for the most part, based on opinions from systematic discourses rather than facts discovered by experiment. The reviewer believes that, to be of value to contemporary psychologists, no treatise on social psychology can neglect the objective viewpoint or experimental method.

University of Minnesota Miles A. Tinker

L'activité psychique chez les animaux: Instinct et intelligence. By L. BRETEGNIER. Paris, Vigot Bros., 1930. Pp. vii, 387.

The reader of this book is taken through a large part of the domain of animal psychology, including its history, its major problems (especially instinctive behavior), and a part of its experimental achievements. The book is richly but carelessly documented with references drawn for the most part from French authors, most of the research done in America being left out of account. Köhler comes in for several pages but other references to German researches are scanty. Some of the older studies of Watson, Thorndike, Morgan, Yerkes and a few other papers by Hunter, Lashley, Jennings, and Loeb are listed in a poorly selected bibliography; but no use has been made of some of these references in the text. Such citations as the "Britisch Journ. of Psych.," R. S. Lashley, J. B. Wahson, and W. M. Weeler, numerous omissions of dates, the meager number of references to studies published after 1915, and dozens of typographical errors do not increase the confidence of a reader in the scholarly quality of the book.

The author is a veterinarian, and has written more in the temper and with the empirical outlook of Romanes than of Washburn or Hempelman. Descriptions of animal behavior from the *Atlantic Monthly* or from *Mercure de France* stand side by side with data from Hunter, Watson, and Thorndike and not a little space is given to Jack London and James Oliver Curwood. The whole account is enriched by the author's experiences from his professional career and by literature from fellow veterinarians.

The author's early statement of program consists of a pointed self-instruction against undue anthropomorphism and of a vote in favor of Lloyd Morgan's canon; but almost with the same sweep of the pen the author argues for a liberal interpretation of animal behavior in terms of human experience; and as soon as the review of the literature gets under way, it is the last injunction rather than the others that becomes dominant.

The first chapter is devoted to a history of animal psychology in which a recital is made of the opinions of ancient and medieval authors. It is followed by a study of the methods that ought to be employed in animal psychology. It is here more than in any other chapter that the author tries to place himself inside of American research; but the attempt is not impressive.

Four chapters, totaling 134 pages, are devoted to tropisms, reflexes, and instinct, one chapter being given to the instinctive behavior of the social insects. The discussion of tropism is hardly more than a review of Loeb and is fair enough in its treatment. Reflexes are considered primarily in their rôle as the foundation of instinct and intelligence. Instincts are described as having a double origin. They represent complex and stabilized reflexes which can be transmitted by heredity, and are descended from acts which were originally reflective and conscious and have become unconscious through repetition. These, too, are said to be heritable. Thus the nature of the descent from intelligence to instinct is indicated; but we are not told how the ascent from instinct to intelligence was accomplished. There is throughout the book the implication that the genetic history of mind is to be read in terms of a progression through tropisms, reflexes, and instincts to intelligence; but this is an assumption that is seriously in need of critical study and possible revision.

The chapter on emotions (26 pages) draws much of its material from Köhler, Romanes, and Darwin, on the one hand, and from general experience, mostly veterinary, on the other. It does more to set a problem and to throw light upon lacunae in research on animals than it does to indicate progress in understanding this part of animal psychology. The chapter on the intelligence of animals begins with a lengthy account of animal tricks and genius drawn from the tales of sportsmen, veterinarians, and trainers, and it ends with the usual remarks about "clever Hans," the Elberfeld horses, and Köhler on chimpanzees. There is, here, as in most of the other parts of the book, nothing more than a recital of facts and observations with hardly a critical remark on what has been reviewed.

The book closes with a short chapter on language, which contributes almost nothing, and a brief résumé of the mental life of the higher animals as compared with man.

The general conclusions do not represent a summary of the significance and general bearing of the researches mentioned but only a diffuse and somewhat philosophical statement of the beliefs and opinions, not to say prejudices, of the author.

University of Illinois COLEMAN R. GRIFFITH

The Layman Looks at Doctors. By W. S. and J. T. PIERCE. New York, Harcourt, Brace & Co., 1929. Pp. 251.

This is a narrative of a series of episodes in the life of an only daughter of an elderly physician, an educated and sensitive young woman who suffers a "nervous collapse" shortly after an apparently happy marriage to a devoted lover and husband. The story of her period of mental aberration is clearly and somewhat dramatically developed from a lay point of view by the writers, husband and wife, whose early married life is thus interrupted by the latter's abruptly appearing illness. In her efforts to regain her mental and social equilibrium, Sally is forced through a chain of unhappy and unfortunate experiences with sanitoria, hospitals, and nursing homes, which are graphically portrayed, and which are terminated, according to the account, only with the treatment by a psychoanalyst to whom her final recovery is gratefully ascribed.

There appears from time to time a slight tendency toward projection mechanisms on the part of the writers, who, as laymen, also do not seem to distinguish clearly between the province of the neurologist and that of the psychiatrist, nor to recognize that psychoanalysis, however useful it may prove, has likewise its definite limitations. It is possible that this carefully detailed account of unfruitful medical contacts may, through hasty generalization by the suggestible reader, prejudice him against seeking necessary medical aid, perhaps much to his own disadvantage. On the other hand it should prove a tremendous stimulus to critical self-analysis of motive and achievement on the part of earnest and sincere physicians.

New York City HAROLD A. PATTERSON

Our New Progress. By J. B. CLARK. New York, G. P. Putnam's Sons, 1930. Pp. 128.

This is not a scientific book. It is a theoretical consideration of what real wealth is, and devotes especial attention to fallacies and dangers connected with charity in its various aspects. Transmission of acquired characters is naïvely assumed and consciousness is regarded as a causal factor. J. P.

The First Year of Life. By CHARLOTTE BÜHLER, and others. Translated by Pearl Greenberg and Rowena Ripin, with preface by translators. New York, John Day, 1930. Pp. x, 281.

Part I appeared originally as Bühler and Hetzer's *Inventar der Verhaltungsweisen der ersten Lebensjahres.* Part II is Hetzer and Wolf's *Babytests* and Hetzer and Koller's *Eine Testreihe fur das zweite Lebensjahr,* with directions for giving the tests, method of scoring, and validity. Part I is a description of the continuous observation over a period of two months of the activities of 69 infants under one year of age, in an institution for children in Vienna. Sixty percent were institution children and 40% were observed in their homes or were brought to the institution by the mother. The observations were made under normal conditions, the setting uncontrolled in every way, the observer noting every change in the child's behavior. The author admits that she and her assistant soon came to the limit of language to represent all movements and vocalization. They then made use of a finer method of notation, but unfortunately do not reveal its nature. The aim was to obtain a composite picture of normal behavior —uninitiated by any artificial stimuli—a picture to serve as a standard for average development. We are told nothing, however, of the amount of handling each child received, how many individuals came in contact with him, and so on. The author regards experiments which utilize artificial stimuli as lacking in scientific value and therefore found it unnecessary to make any attempt to evaluate qualitatively or quantitatively the stimulating conditions surrounding her subjects. In reading the book we must distinguish between what the child does under a so-called normal environment and what he is capable of doing under given stimulus conditions. For example, the author finds that the infant makes no movements of defense until the fourth month. Yet it is well known that under certain conditions the infant as early as two weeks makes definite coördinated movements of defense. An adequate picture of normal development should include not only what a child does in a restricted environment but also what he is capable of in response to the varied stimuli he must more and more be prepared to meet as he develops. Following Part I is an appendix containing a schedule of infant behavior, month by month. The inventory lists the observable behavior of the child at a given age, with amplifying descriptions. The interpretative element is prominent throughout, names such as 'fright,' 'joy,' and 'ill-humor' being given to groups of reactions made in response to stimuli known to the observer. The book is an excellent example of the diary method, valuable to those interested in a description of infant behavior.

Chicago, Illinois IRENE CASE SHERMAN

The American Road to Culture. By GEORGE S. COUNTS. New York, The John Day Co., 1930. Pp. xiii, 194.

Contending that the American road to culture is none other than the American educational ladder, Counts here gives us a running critique of our entire educational system. His treatment closely approximates his objective of looking "at America through the eyes of a foreign observer"; it is admirably unprejudiced, fair, and happily free from any trace of cheap superficiality.

DePauw University STEPHEN M. COREY

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AN EXPERIMENTAL INQUIRY INTO THE EXISTENCE AND NATURE OF 'INSIGHT'

By MARY ELIZABETH BULBROOK, Cornell University

It was inevitable that the recent occupation with 'learning' should presently lead backward toward the consideration of a rapid, effective and 'intuitional' form of operation. 'Backward' because that had long been a favorite conception of learning, but a conception displaced by the simpler and more manageable forms of the reflex, automatic, psychomotor and ideational varieties which set the experiments of a generation ago. At that time the scientific standards coming into psychology placed a high value upon simple and mechanical concepts and procedures. Sensory processes, simple and direct associations, and the blundering advances of 'trial-and-error' were the popular terms in laboratory studies upon learning in man and in other animals. It was only a matter of time when the limits of these simpler categories of description and explanation should be discovered and when 'higher' processes and modes should be again invoked. Moreover, the recent occupation with the organism, in biology and psychology, as against the analytical inspection of parts, details, and elementary functions, has also contributed to this tendency in the study of learning. Organismal factors, *Gestalten*, the primacy of the general pattern, the influence upon development of the total-body, and the general directiveness of life, have all been facts of this same order tending away from the older and simpler mechanisms. It was natural that 'insight,' 'forecast,' 'purpose,' and even 'intuition' should have been rescued from the commoner and less precise employment of them as dictionary terms and turned to psychological account.

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*Accepted for publication May 1, 1931. From the Psychological Laboratory of Cornell University. The research was directed by Professor Bentley.

To psychologists familiar with current and recent discussions, it will be unnecessary to dwell upon the revival in periodical and textbook of the term 'insight' and to point out the variety of meaning and the resulting ambiguity in its use.[1] Some of these meanings are perceptive apprehension, acute observation, understanding, foresight and forethought, rapid learning, an intuitive flash, sudden grasp or illumination, intelligence, sophisticated skill, cognized relations, the felt basis of an attitude, experienced determination, a new perception of a goal, and a new configuration.

Now there are obvious difficulties in approaching by experiment a topic so uncertain and so variously shaded by theory and point of view. All we have tried to do in this study is to submit to experimental conditions various situations which have been commonly alleged to exhibit experiences and performances characterized by 'insight.' Here it has been our hope that the appearance of a common process, function, or accomplishment might help us to an understanding of this elusive characteristic. In anticipation of our results we may provisionally say that no such common and unique operation of the animal organism has emerged from our quest. Nevertheless, the experiments have thrown considerable light upon a class of performances just now under close scrutiny from many sides.

THE EXPERIMENTS

Most laboratory studies of 'insight' have been of the problem-solving and maze-running kinds and have made use of children, other primates, and white rats. In order to add the very important experimental control which is to be derived from the report of the observer himself, we have used human adults, both individuals trained in psychological reporting and individuals untrained. For both sorts we have added the device of technical *instruction* in order to obtain the actual conditions (commonly left—even in experimental settings—to inference or to speculation) under which the organism is thrown into functional commission. It would seem now to be of less importance to *infer* insight from behavior ('insight' taken, e.g., in one of the above senses) than *to obtain a descriptive account of the*

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[1] Only here and there has a critical voice been raised. Examples are G. W. Hartmann, The concept and criteria of insight, *Psychol. Rev.*, 38, 1931, 242-253, and S. C. Fisher, A critique of insight in Köhler's 'Gestalt Psychology,' this JOURNAL, 43, 1931, 131-136.

actual modes of organic performance where insight has already been alleged.

We have taken individual observations and also, in the light of results thus derived, have secured observations from small assembled groups. We consider the problems in order, setting down the individual-results first, then the group-results.

Thirteen problems, presently to be given in a natural sequence, were placed before each of 7 *Os*. Five of these *Os* were trained in psychological observation. The other two were without technical training.[2] For the group-presentations, 110 students were used in congregates of 5-8 from an undergraduate course in general psychology.[3] In order to prevent troublesome influences from successive presentations, the following temporal order was adopted: Problems 1, 5, 6, 7, 3, 12, 2, 4, 10, 9, 8, 11, 13. The groups worked through only certain problems.

Procedure. For the individual *Os*, the formal instructions[4] were placed on a large blank sheet of paper which lay on a table immediately before the *O*. The table was in a corner with blank walls in front and to the right, while on *O*'s left sat the experimenter. Upon the table were usually scattered various objects, some having little or no relation to the problem, others supplying means for various solutions. The arrangement of these objects remained fixed throughout the experiment.

So far as possible, these arrangements were preserved in the group-presentation. Some changes were necessary. For the groups a large rectangular table was equipped with a single set of the same objects in a similar haphazard arrangement. No less than five and no more than eight *Os* at a time were seated on three sides, while the fourth was reserved for *E*. A pencil and mimeographed copies of certain instructions and all questions concerning procedure were placed face downward before each *O*, to be turned over and read at a signal from *E*. Other instructions were orally given. All questions were to be answered 'yes' or 'no' except the last one of each set. Emphasis was placed on the fact that the experiment was not an intelligence test of any kind or a competitive trial of capacity. The grouped *O*s were directed not to handle the experimental objects.

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[2] *Observers.* The trained *Os* were Dr. S. Feldman (*Fe*), Dr. J. G. Jenkins (*Je*), R. B. MacLeod (*Mc*), members of the staff; E. H. Kemp (*Ke*), A. D. Glanville (*Gl*), graduate students in Psychology. The untrained *Os* were H. A. Myers (*My*), and K. M. Simpson (*Si*), graduate students in Philosophy and Physics respectively. C. M. Wiltse (*Wi*), and R. G. Fisher (*Fi*) were occasional observers.

[3] For the group-presentations, 110 observers from the undergraduate classes in Psychology volunteered. This number observed in all problems presented to the groups except problems 5, 6, and 10, which had 63, 50, and 64 observers respectively. Since the experimental period of each group was limited, it was found necessary to alternate problem 5 with problems 6 and 10.

[4] For the three main forms of *instruction* (formal, occasional and self) see O. F. Weber and M. Bentley, The relation of instruction to the psychosomatic functions, *Psychol. Monog.*, 35, 1926 (no. 163), 1-15; M. Bentley, *The Field of Psychology*, 1924, 389-396.

At times during the experiment, however, it was found necessary to assure the grouped *O*s that a solution was possible, and this assurance sometimes evoked a self-instruction that 'I am being compared with others and must make a good showing.' These by-products of socialization—while they must never be assumed—must always be looked for in group-operations. They frequently determine output and function in the solo experiment as well. Only careful report will turn them up and estimate their influence. As antecedents and determinants of function they can never be behavioristically inferred from mere physical proximity and isolation.

Problem 1. This problem turned on the significant use of materials which might suggest some form of 'insight.' A string of beads was placed directly before *O*. It consisted of two small white 'pearl' beads alternating with one larger yellow glass bead, except for the middle portion of the string where five white beads separated the yellow. The bore of the white beads was too small to admit of stringing except by a single thread with a moistened and rolled end. The needles supplied were too large.

(*a*) *Individual Os.* Two sets of instructions were made, the second for presentation in case an *O* failed to obtain the solution under the first.

> *Instruction 1:* Make a single regularly repeated pattern without either unstringing or restringing the beads, and without knotting or breaking the thread. Proceed aloud.
>
> *Instruction 2:* Make a single regularly repeated pattern without either unstringing or restringing the beads, and without knotting or breaking the thread. Use any of the means supplied on the table. Proceed aloud.

Due to the emphasis of the first set of instructions, attention was primarily centered on the experimental object. Each *O* first sought to get the instruction clear and then carefully examine the beads in order to understand fully the conditions and limits of the situation. Twisting the threads together by some means or other and sliding the beads across was one of the most obvious solutions suggested. It never worked, of course; and the *O* soon realized that instructions stood against it. The superfluous beads could not be unstrung.

Four *Os* (*Fe*, *Si*, *My*, and *Gl*) went through a process of elimination to arrive at the solution. *Fe* solved by a consideration of various means of eliminating the irregularity, "I see nothing to do but twist the threads together. If they would hold together, the beads could be slipped over." He picked up the beeswax. "It won't do." He picked up the glue. "No, no. That won't do. It is too sticky." He picked up the needle and sought to use it but found that the white beads would not go over it. From the first the various articles and tools available entered into the situation for this *O* as having possible significance. Now he picked up the saw from among the destructive instruments and laughed at using it. But after he had considered all the ways that the superfluous white beads could be eliminated, he suddenly asked, "Can you break the beads? That is the only way of getting them off that I can see," and he picked up the pliers and broke one of them. "Breaking was a last resort. I had tried everything else. I could think of nothing else until that occurred to me as possible."

For *My*, *Si*, and *Gl* the elimination took place without reference to the objects on the table but from the conditions imposed by the instructions and limitations presented by the object itself. "The beads had to be removed. Since all the natural ways were prohibited, that was the only one left. I tried first to crush them with the fingers but they were too hard. I thought of biting them but that was bad. I looked around and saw the tools and I took the pliers and broke them" (*Gl*). It is obvious here that the instruments played a minor part in the solution. They merely furnished excellent, because stronger, substitutes for hands and teeth. The *O* was set for an instrument to break or crush when he looked at the various materials on the table. "If you cannot move any bead over another bead, you cannot possibly change their relative positions on the string, and they are an uneven number, so they cannot be distributed so as to make a regular pattern. Can the thread be passed through any bead twice? But that would be hard to do. Are you allowed to break the beads?" (*Si*). He then counted the beads and said, "That would do. Why I thought of breaking the beads, I do not know. I just did. It was the only way of doing it." Here again it was only after the correct method of solution was obtained that the instrument for its accomplishment was sought.

My, proceeding step by step, commented clearly and logically, "Two-one, two-one is the pattern. There are five extra white beads. If those five were out, it would be a regular pattern. One might make one by distributing the white beads. How can they be distributed? There are five white beads and twenty-one large ones, therefore they cannot be evenly distributed. The directions say not to unstring or restring, etc.; but those beads cannot be distributed without breaking or knotting the thread. I can try arranging the beads on the paper. Thus far I have been thinking of the pattern on a straight line. I can make a circle or a figure like this, but that won't succeed. Is it permissible to break the beads? The instruction does not say anything about not breaking the beads. I can do that. . . . That was the only possible solution because, even if you could break the thread or did break the string, you could not make a regular pattern utilizing the beads."

No reference in these two cases was made to the instruments lying on the table. They were as completely extraneous to the solution of the problem as if they were not there. But in the actual breaking the *O* invariably surveyed the outlay and seized the pliers for the purpose. That this instrument easily became a destructive crusher in this situation is not surprising, when one considers its manifold uses in constructive building where it is often used to break or bend.

Three *Os* (*Wi*, *Je*, and *Mc*) failed to reach the solution. It never occurred to *Je* and *Wi* that the integrity of the beads could be destroyed in any way. The beads were to be removed or redistributed as beads. They never got beyond this assumption, and this assumption was never formally recognized. That all *Os* were inhibited in some measure—although not so completely as with *Je* and *Wi*—against destruction as a means of solution is obvious from the fact that after a consideration of many possible methods of removing or redistributing the intact beads, the *O* either declared it to be impossible or asked if it was not so. At this point he was invariably assured either that "it was not impossible" or that "the irregularity could be eliminated." This is a clear instance where *the occasion* (occasional instruction) stands directly against the carrying out of a formal instruction.

The most interesting case of inhibited action was that of *Mc* who failed to reach a solution under either instruction. Under the first instruction attention was focussed, as in other cases mentioned, on the beads and the limitations imposed by the conditions of the instruction. After various suggestions in attempting to make a uniform pattern, and after having been told that such a thing was possible, he "does not see how it can be done." Under the second instruction, he picked up the saw, "May I use this?" Then he picked up the thread, "This is the only thing that might do." He took up the objects one by one, remarking each time "That won't do" or "This does not seem to be applicable"; but, except for the saw, he never considered the destructive instruments at all. "I'm still utterly baffled by the situation. I see only one way of making it a pattern and that is barred by the instructions."

That *Mc* noticed the instruments was evidenced by his handling the saw; but that they were recognized only in their usual contexts and not in any unusual relation to a delicate string of beads was apparent from his behavior as well as from the above account. This is the opposite of what occurred in the case of *Ke*. The mere presence of the instrument within visual range caused him, intent on some means of removing the superfluous beads, to give consideration to any possible relation it might have to the problem in hand. After examining the string and trying to run another string through the beads with the needle and thread on the table, and after the assurance that the solution was possible, he leaned back, looked at the table, and said, "I might break the beads and take them out. . . . The beads could not be moved according to the instructions. The large ones could not be slipped over the small ones. I saw a pair of pliers over here and saw the string of beads over here that could not be moved off. I just re-read the instructions and broke them." The instrument for *Ke*, as for *Fe*, entered into the situation from the first; but for *Fe* 'breaking' suggested the instrument while for *Ke* the instrument suggested 'breaking'.

Results. Acting under occasional and self-instructions and anticipatively apprehending the results *O* eliminated one plan after another until the solution either was reached or was blocked by an interfering occasional instruction or by the failure of comment and imaginative apprehension. The elimination of possibilities may take place either with respect to suitability of means or instruments or with respect to procedure permitted within the limitations imposed by experimental object and formal instruction.

For the *O* there is always a plan or method of approach. When his approaches all fail him, he ceases to try and desires to terminate the experimental period. He never continues, trying at random, in expectation that the solution will simply occur. He always applies a definite method and this method is a contribution of his own under self-instruction. He does not wait for sudden appearance of the solution. He does something that definitely brings about, or causes, or creates that solution. There is always an accompaniment of pleased pride in, and approval of, himself with the accomplishment of the solution. In failure there is frustration combined with impatience and disappointment with himself.

Inability to see an ordinary object in an extraordinary context and a predisposition to respect the integrity of property constituted frustrating moments.

(*b*) *Group Os*. The instruction was the same as Instruction 1 above, except for the direction "Proceed aloud" which was omitted. The time allotted was 10 minutes. Then the following questions were answered. (The replies are given in percentages.)

		Yes	*No*
1.	Did you look over the table to see what you could use?	14	86
2.	Did you consider the string of beads only and what the instructions did and did not allow you to do to it?	93	7
3.	Did you solve it?	25	75
4.	Did you rearrange the beads to form a different kind of pattern?	42	58
5.	Did you break the beads (imaginatively)?	25	75
6.	Did you think of breaking the beads first and then look for the proper instrument?	12	88
7.	Did the instrument make you think of breaking the beads?	3	97
8.	Comment further upon your procedure, telling any way you went about eliminating the extra beads that is left out in the questions.		

Since a quarter of the group (Question 5) reached the 'breaking' solution and only 15% of them (Q. 6 and 7) considered the instruments, it is obvious that a tenth of them who thus solved did not regard the instrumental means. But then, they were not actually called to smash as the individual *O*s were. This is an interesting difference in solution, which is also supported by Questions 1 and 2 (concentration on the beads). Answers to the last question indicate that practically all the methods employed toward solving by this large number were also employed by the smaller number of individual

Os. Pattern arrangement, bead counting, and the consequent deduction that redistribution was impossible were means suggested.

In a few cases the solution came immediately. Those who suggested breaking the beads report substantially as follows: "The instruments had nothing to do with it either before or after getting the solution. I took it for granted that the tools on the table were for another experiment." "I thought of breaking them with my teeth or the eversharp in my pocket." "I never thought of the instruments on the table. When I saw the table, it seemed more like a class in vegetable gardening than an experiment"—another good example of apprehension of an object only in its usual context.

Some, after making several tentative and ineffective suggestions, concluded the solution impossible. Most of them, however, failed to get beyond a stage of search or of comment-and-inspection in the time allotted.

These results substantiate those from the individual *Os*. Inhibited solutions are more plentiful, due, perhaps, to the small amount of time; but the initial steps of the solving process—apprehension, inspection-and-comment, search for method, and procedure under self-instruction until that instruction is perceived as inapplicable, re-apprehension and search for another method—are typical here also.

Problem 2. This problem was intended as a parallel to the foregoing. Small white wooden beads were obtained, and some of them were stained black with India ink. They were then strung with the white, the pattern alternating one white with one black, one white with one black, except toward the center where eight whites separated blacks. The solution lay in converting the ink in the bottle on the table to the uses of a dye No *O* questioned how the black beads were colored but accepted them as given.

> *Instruction 1*. Make a single regularly repeated pattern without either unstringing or restringing the beads, without knotting or breaking the thread, and without destroying the beads. Proceed aloud.
>
> *Instruction 2*. Make a single regularly repeated pattern without either unstringing or restringing the beads, without knotting or breaking the thread, and without destroying the beads. Use any of the means supplied on the table. Proceed aloud.

If the *O* failed to solve under the first set of instructions, the second was presented.

The solution involved the use of a medium in unusual contextual relations. In some cases it was obtained more easily than in the preceding bead-experiment. The *O* was memorially instructed from cues gained there. "I got the solution of the present string from a cue from the other beads. As soon as I got away from the rearrangement of the beads on the thread, it was easy to think of doing other things" (*Mc*). "I am looking for possible interpretations of your instructions" (*My*).

Je, *My*, *Gl*, and *Mc*, as in the preceding experiment, set about seeking the solution, not from a consideration of the whole experimental situation, but from the conditions imposed by the instruction and the experimental object itself. Search for a way of attack, accompanied by a careful inspection and comment upon the situation, was characteristic of the procedure. *Si*, for example, asked about the instructions to make sure he had them accurately and picked up the string and examined it. "There is no catch on the string." He examined the string and beads again carefully. "One thing is obvious; if there are more white than black beads, you must either get rid of some white ones or else get some black ones. There are no black ones. Is this string symmetrically arranged with regard to the center ones? No, it isn't." He read over the instructions again. "For all purposes this is a closed circle and no bead can be moved over another nor can they be broken. Is it unsolvable?" When answered negatively he said: "Well, something must be done to the white beads."

Where the solution was reached, the *O* invariably arrived at it through a consideration of painting the beads. "I did not notice the ink on the table, but I named ink instead of dye or paint because I had had more experience with ink than with either of the other two" (*Mc*). "How about some paint or something? Some black ink if there is any available," he picked up the ink bottle, "Or, better still, the black ink from my fountain pen" (*Si*). "I thought of paint first but then I thought of ink because you would be more liable to have it around" (*My*). Here again the method of solution was obtained before the exact medium for its accomplishment. Since the *O* searched for something he could use in following out his solution, ink easily became, under these circumstances, a dye in spite of the penstaff lying alongside the bottle.

As before, *Fe* and *Ke* derived the solution from the whole experimental situation, taking into account every article on the table and its possible relation to the problem in hand. *Ke* ran his eyes rapidly around the table a few times inspecting and commenting upon all the objects on it. "Suppose I run another string through and pull this one out. . . . But I guess that is unstringing and restringing. Oh well, I can paint them black," and he seized the ink bottle to do so. *Fe* followed much the same procedure, glancing all around the table and then reaching for the ink bottle. Here there was a very definite instruction resulting from the solution in the former experiment.

Gl and *Je* failed to solve the problem, the case of *Je* exactly paralleling that of *Mc* in the former experiment under both instructions. *Gl*, however, solved it under the second instruction, "It said, 'use any of the means on the table.' I had tried all the arrangements of the beads before and they would not work. I thought there might be some chemical here that you could put on them to change the color. I looked around and saw the ink bottle and thought, 'Aha, that will do it.' "

Results. Here each *O* followed the same general procedure as in the preceding experiment. If he had limited himself before to a consideration of the experimental object and the conditions of the instructions, he did so in this case; but if he had taken the instrumental approach in the first, he followed it now. Only one exception occurred. The change from the first instruction to the second in the case of *Gl* induced a change in the set toward the problem.

In some cases *Os* took cues from the former problem. Practically all of the inspection of the experimental object and weighing of the instructions were absent here from those who approached the solution by an instrumental elimination. Self-instructed, the *O* looked around the table, immediately questioning the suitability of the various objects. Cues were of aid, too, in the other approach, where the *O* paid more careful attention to possible implications in the instructions, and where he took account of and guarded against any inhibiting moments similar to those in the former problem.

But in other cases helpful cues were not carried over. The *O* was as strongly inhibited here as before by his custom of seeing a thing in a useful context from apprehending it in another and a newer one. In one case (*Je*) the cue from the previous problem gave him a set toward destruction as the means of obtaining a single uniform pattern, a set that was useless in the solution. Even under the new instructions, this set was carried over.

The same elimination of one self-instructed plan after another, the same methodical procedure of search accompanied by inspection-and-comment to the formulation of the self-instruction, and, when no other plan could be made, the refusal to continue planless characterized this problem as definitely as the first.

Problem 3. A cork in a large bottle with a very narrow neck was presented to *O*.

> *Instruction.* Remove the cork from the bottle without destroying either the cork or the bottle.

The situation here differed from the first two in having the solution by instrumental means suggested. A large corkscrew, the usual means of removing a cork from a bottle, lay among the tools, which also included a small flexible wire. While this suggested the use of an instrument, however, it was always that particular instrument. Only one *O* analyzed the situation to the extent of not applying the corkscrew first. *My* sought to gain a thorough comprehension of all the conditions inherent in the experimental object. Inspecting and commenting minutely upon the cork and bottle, suggesting one means after another without reference to the tools, he finally remarked with characteristic acumen, "Judging by the appearance of the cork, I believe mechanical means have been used on this before," and seized the wire, knotted an end which was too small, made a larger knot, caught the cork on it, and pulled it out. *Ke* picked up the wire first but threw it down without trying it, "I must try the corkscrew."

All the other *Os*, after trying the corkscrew, made a loop of either the wire or the thread, caught the cork at the right angle, and with and without the aid of the corkscrew, pulled the cork out. Here as before a means is employed effectively out of its usual context. No *O* failed in this solution for various reasons; first, because corks inside bottles are a common occurrence, and, secondly, the experimental set-up favored the correct solution. When sharp point and corkscrew were not successful, the *O* invariably seized upon the wire.

Results. When the occasion suggested the correct direction in which solution was brought about, there was a tendency on the part of the *O*s to go in that direction. In most cases, however, the more obvious suggestive factor, the corkscrew, inhibited a consideration of less obvious but equally suggestive factors (*e.g.*, those considered by *My*) until failure brought about a further investigation of the experimental situation with another consequent plan of procedure.

The same inspection-and-comment, the same search with an eye to possible ways of procedure, the same methodical self-instructed plan of procedure and elimination through failure or inapplicability characterized this problem as the others.

Problem 4. This problem consisted of a series of seven cards presented in order bearing seven different arrangements of the line-elements of the letter R lying on its side, but the last arrangement comprising the R as it is ordinarily presented except for its sidewise position (Fig. 1). The principle of this series was suggested by problems in the classification of a new species or variety in taxonomic zoology or botany. The aim was to lead the *O* through a number of steps containing very familiar elements in more or less strange relations to each other to these same elements in their familiar relations. Experimental interest was centered on how soon the *O* saw the familiar relation, and, after seeing it, commented upon the whole series.

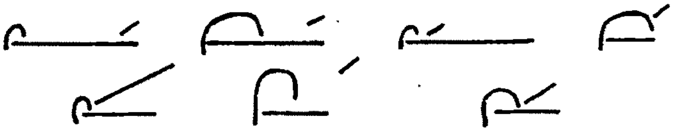

FIG. 1

Instruction. Identify each member of the series presented to you.

The element of search was quite apparent in most of these observations. "I keep wondering, 'What do you want?' " (*Ke*). " I can name the lines. It seems as if it ought to represent something more definite than that" (*Gl*). "Nothing like sea-lions, is it? Is it a joke or anything of that nature? I try to see something there, but I am not sure of it. It is not supposed to be an ideograph, is it? Is it supposed to be something you can see?" (*Si*).

One thing after another was named by the *O*s as a possible identification; although in practically all of their observations there was an element of uncertainty concerning the object with which it was identified. "It looks like the other figures. It might be two or three objects like tombstones, etc., except for faults"

(*Ke*). "It looks like a P on the side with a long vertical part. What the little oblique line is bothers me. It is also a stream of water in a tile" (*Gl*). "It is a railroad track going into a tunnel. I cannot make out the slanting line" (*My*). "It is a mouse that is only partly there. The tail and head are missing" (*Si*).

In every case but one the *O* sought to give a meaning, though often very unsatisfactory, to the figure—to make it a symbol fitting in with known and recognizable phenomena. Only in the excepted case (*Mc*) very little effort was made to name the figure as some familiar object. In this case the object was too uncertain, too unsatisfactory. He comments: "It does not look like anything to me. It looks like a tunnel, a road here, and a soldier." "That is nothing at all." "Nor that. It requires an awful strain to get the soldier and tunnel."

There was a strong tendency on the part of many of the *Os* to analyze the lines before naming the object with which they might be identified. "It is just a curved line, and a straight one, and a short one" (*Ke*). "One is a straight horizontal line, the other a curved one, and the other oblique. . . . It is again like the other one but this time there is a short horizontal line, with the curved and oblique lines above it" (*Gl*). "I cannot make out the slanting line. The horizontal line is longer" (*My*). "It is a poorly drawn semicircle. It is like the first thing except the curve is longer and the little oblique line higher" (*Fe*). In general, these *Os*, who saw the figure as a group of lines first, tended to make the letter identification long before presentation of the last card; although this identification was, for the most part, as uncertain as the object identifications.

Fe, *Gl*, *Si*, and *My* suggested a letter early in the presentation. The first figure for *Fe* is "a sort of P lying on its side with a long stroke," and the second is "like the first except the curve is larger and the little oblique line higher. It suggests an incomplete R." For this second one also *Gl* commented "If I look at it sidewise, it suggests an R with part of it missing." For the fourth, "The curve and the horizontal line suggests an incomplete D," but for the sixth he said, "It looks like an uncompleted R. When I take them (the lines) as water in a tile, the oblique line does not fit. When an R, it does fit." *Si*, in the midst of positing several objects which the lines might represent, said, concerning the third figure, "If I turn it around, I can make an R out of it but I guess I'm supposed to keep it upright." He was self-instructed here to avoid the unusual in position. On the fifth figure *My* commented, "The line is longer, the tunnel is smaller. It looks like the letter R. I turned my head sidewise and looked at it like that. Once before it looked like the letter R rather remotely," and, on the sixth, "It still looks like the letter R."

For *Ke*, *Mc*, and *Je* the letter appeared only on the presentation of the last figure. Failure to suggest an R earlier was ascribed primarily to the set. "It is an R—a letter. The elements were so scattered in the others that that must be why I never got it before. I never thought of a letter" (*Ke*). He picked up the cards and went through them: "It is easy to see them all as Rs now." "The whole series is composed of various distortions of the R turned sidewise. I was set for a picture. I did not expect anything tipped over" (*Mc*), and for *Je* the set was strong enough to cause him to continue the pictorial identification to the last figure, "The first flash is a rabbit, the semicircular thing representing an ear. I just decided this when I got a very brilliant flash with an incipient shrug of the shoulders, 'Why, of course, that is the letter R.' There never was in any of the earlier figures any suggestion of a letter. I have some suspicion that either the card tipped as you put it down, or my head did. You can see that the same motif was carried out throughout" (*Je*).

Care was taken on the part of *E* to prevent tipping the card. This "kinaesthetic pull toward the left," "tipping or turning the head sidewise" was mentioned frequently in the commentary, not only for a letter, but for objects in motion. "There is a kinaesthetic pull in the shoulders toward the left. I have a visual apprehension of the long angular line travelling toward the left" (*Je*).

From the final reports of the various *O*s it was obvious that every member of the series had taken on the characteristic significance of the R symbol. The purpose of every member now was to represent that letter. "We have our R this time. It is a tied image R, too. The R stands out immediately. There was no regular development in the R, but I can see now that that was what it was all along" (*Gl*). "There is the same general character to the whole series. All of them represent an R quite well, although each of them taken individually could have represented any of the other things equally well. But you can see now that it could not have been anything but an R" (*Si*). *My*, who was sure of the letter on the fifth figure, reported, "I think I noticed the R on the third one. There was not a close enough resemblance. I thought I would see a few more before I committed myself."

For the most part, the same general elements were noted from presentation to presentation, but their direction was undetermined. That "one figure is like the preceding one" or that "there is a certain similarity among them all" was reported time and again; but the significance was never fully determined, except in the case of *My*, until the last and most familiar relationship was presented.

Results. The reports from this problem display every stage of advance toward identification from an almost pure perceptive level, accompanied by the vaguest and most indefinite kind of puzzled search, in some cases, to a more concrete, though tentative, symbolization for some familiarly recognized object or objects, in others.

A uniformity among differences was one of the first general conditions noted, but an uncertainty of its significance accompanied the comment of every observer but one until the last member in the series was presented. Then the significance became certain either immediately or secondarily, depending upon the set of the *O*. After once knowing the correct symbol, a recognition of its appropriateness in comparison with any other identification occurred. This revaluation of the whole series in the light of the last member was characteristic of every *O*.

By a process of finding the most appropriate identification-object, an object which fitted in best with the recognized uniformity, two *O*s named the correct letter midway in the series, the later presentations merely serving to confirm the conjecture.

The disposition on the part of the *Os* to verticality, to apprehending objects in their customary positions, was an inhibiting moment in the identification of the lines as a letter. So also was the predisposition to regard the lines as a picture of something.

Those *Os* who inspected the members as lines first tended to name letters as well as objects for identification early in the presentation. *Ke* was the only exception to this.

Problem 5. This problem was intended to demonstrate a solving procedure combining 'trial and error' with 'insight' (= 'foresight and forethought' or 'new configuration'?). A set of papers, each sheet bearing Fig. 2 (but wanting the dotted diagonal at the centre) was presented to *O*, who was supplied with a red pencil for clear retracing of the lines. Gaps at certain junctions were left to discourage retracing. A clear disjunction of the inscribed figure from the surrounding rectangle was hinted at in the drawing.

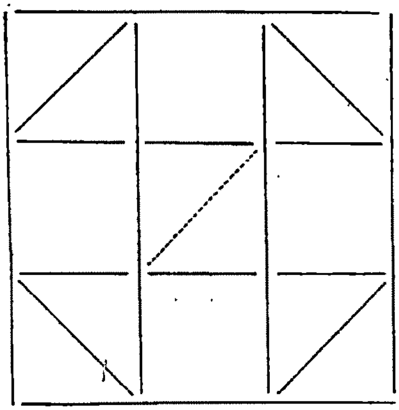

FIG. 2

> *Instruction.* Here are two figures. Trace continuously (without retracing) every line of both. Use just as small a number of copies as you can. You will be penalized for every wrongly traced set. Comment aloud.

(*a*) *Individual Os.* Here as elsewhere conditions were considered and no random stumbling trials were made.

Search for the best way of attacking the figure, and inspection of and comment upon its salient characteristics with reference to the most advisable way of beginning marked the procedure. The *Os* had concerted plans before placing pencil on the figure. "I looked to see how the two could be traced independently. I saw that the square could be gone around simply enough. So could the other figure" (*Ke*). "I did it mostly by means of eye-movements. The solution came practically immediately" (*Mc*).

Some *Os* traced the figure first by moving the eyes over it. Others traced it in the air with the pencil held above the figure. Most of them began the actual tracing on the inside figure; but *Gl* and *Wi* began at points on the outside square. The beginning points were based on occasional instruction *plus* inspection-and-comment. "I saw that I must begin at a point here and I saw that, since the figure is symmetrical, it made no particular difference which of the like points I did begin on" (*Si*). "The outside figure must be begun at a point on the inside one" (*Gl*). "The first glance revealed that a gap would have to be left in the corner of the square which would have to be done at the end. Then the inside figure could be drawn immediately" (*Wi*).

After reading the instructions, one *O* had difficulty in seeing the two figures. "At first the gaps and the two-figure idea bothered me. I saw it first as a square and an octagon with other line-figures" (*Mc*). "I am looking to find the two figures" (*Si*). In general the *O* began by seeking to understand the instructions fully in order not to miss any condition which might be of significance in the given problem.

Results. After a mastery of the instructions and after inspecting and commenting upon the experimental object, the *O* approached the solution by a method of action under self-instruction and perception of its results, that is, of starting from some plausible point and proceeding until balked by an unforeseen condition. Thereupon another point was chosen and the same procedure continued, always, however, with this condition taken into account. This method was followed until the solution was reached.

Owing to the threat of penalization, the figure was concretely traced only after the solution was obtained.

(*b*) *Group Os.* The same figure under the same instruction, but without the direction "Comment aloud," was presented to the group. Five minutes was allotted for actual tracing.

Questions	*Yes*	*No*
1. Did you see the two figures as a square and the figure contained in the square?	92	8
2. Did you begin to trace the figures immediately?	14	86
3. Did the threat of penalization in the instructions keep you from immediately tracing the figure?	44	56
4. Did you look it over before you began to trace it to see first how it should be done?	89	11
5. Did you hold your pencil in the air while you went over it?	56	44
6. Did you begin on a corner of the inside figure?	52	48
7. Did you begin on the outer corner of the inside figure in order to get from it to the square?	49	51
8. Did you begin on the square?	52	48
9. Did you begin on the square at a point where the inside figure touched it so that when you had finished the square, you could get inside?	41	59
10. If you used more than one copy, did you decide to quit this first one because you could get no further and would have to start over again in a different place?	2	
11. When you began to trace, did you do so deliberately?	87	13

12. Were you looking ahead to see where your movements were leading you?	83	17
13. Did you trace it swiftly?	21	79
14. Had you already determined the correct procedure so that you could trace it swiftly?	17	83
15. Comment further upon the whole of your procedure, filling in details omitted by the questions.		

Eighty-two percent succeeded in tracing the figure correctly. Except for the last four questions, where some confusion seems to exist with regard to the method actually pursued, the procedure parallels to a large extent that of the individual *Os*. Many *Os* report in answer to the last request that they were "unable to finish the figure because of the time" or else that they "had not time to consider thoroughly, in order to do it correctly." That most of the tracing (trying out some self-instruction) was done, not on the paper, but above it, is obvious from the answers and from such reports as the following. "I mentally traced it." "After looking it over, its simplicity was evident." "The method of determining the procedure was done in my imagination rather than attempting it on copies."

Action, not a blind headlong sort of action, but from a self-instructed initiation, is characteristic of the procedure. A reason of some kind is reported for every attack. "The square could be traced starting from any point." "I first looked over the whole figure and decided that I should start with the inner figure and then trace the square, which is one continuous line no matter where you start from. I planned how to trace the inside figure and to get from it to the square without retracing. Then I traced it."

The general procedure with individuals and groups alike may be said to have been characterized by a period of search and of inspection-and-comment leading to a self-instruction and action under this instruction, followed by perception of the results.

The figure was repeated on fresh sheets with the dotted diagonal of Fig. 2 now added as an unbroken line. The task was therefore considerably changed.

Instruction. A line has been added. Trace all lines again under the same conditions.

(*a*) *Individual Os.* That the added diagonal was a further complication involving more effort for the solution was immediately recognized by most *Os*.

"This complicates it" (*Si*). "This will take longer" (*My*). "This complicates matters a bit" (*Wi*).

Gl, *My*, and *Si* traced again with the pencil slightly elevated from the sheet, while *Wi* "went over it mentally twice and then drew it."

The reports of no two *Os* were alike and yet a certain uniformity was evident in the procedure leading to the solution. In all cases but one some reason was given for the beginning point and the point of departure from one figure to the other. This one exception reports, "I have decided that I cannot begin where I did before but I will have to begin at the center. The solution seemed to come about naturally" (*Mc*). Most *Os* traced their figures, reporting their procedure afterwards; but one *O* commented while tracing it in air; "If I start on the diagonal, I must take care of the other two lines by crossing. I have to end on one point of the diagonal if I start on the other" (*Si*). Another *O*, who never traced the figure except with his pencil on the sheet, commented: "This has the same figure as the other practically," and, "When I started, I could not swear that the beginning on the middle diagonal would be successful. I knew the square could be traced continuously and that the figure in the previous case could be traced the same way. When I first put the pencil down, I knew that I could get practically all around, but I was doubtful about the corners. When I got to the last corner, I paused and saw that I must do the square and come back to the last line of the inner figure" (*Ke*). "I knew I had to begin on the middle diagonal because I could not reach it without retracing the lines if I began on the outside of the figure. That I would have to leave the inside figure and do the outer one before finishing the inner came because I knew the middle diagonal was substituted for the one I began on in the first sheet" (*My*). "I began at the diagonal because I had just been over the same figure (except for the diagonal), and when the new line was added, the easiest way of getting it in was to begin on it. It was quite obvious that you had to do the square before you finished the middle figure. Otherwise you would never get to the square. If I began on the inside of the middle figure, I would have to end there and there was no way of getting from there to the square" (*Wi*).

The middle diagonal, the new element, commanded primary attention and drew interest away from the outer figure to the inner. Not one *O* attempted to trace the square first, and only one attempted to begin on the inner figure at any point apart from the diagonal. *Gl* held his pencil suspended from the paper, attempting to trace it, "Go from here to here to here, etc.—That won't do.—The square in the middle puzzles me.—I have decided to start at the same point I did on the former sheet." He was supplied with another sheet which he never traced until he had first gone over it in air, "trying to see the whole figure traced before I start. I got stuck because I kept thinking that I ought to arrive at the starting point. The idea of penalization holds one back." Self-instruction is thus an inhibiting factor in the solution as well as an aid.

Results. Apprehension of the object, search, and inspection-and-comment, leading to action under one self-instruction after another were the essential characteristics of the procedure. The instruction of penalization for every wrongly traced sheet caused most of the trials to be carried out either by eye-movement or by tracing in air. In some cases where the solution was not quickly forthcoming, the figures were so traced again and again. As a result, a sureness and swiftness marked the tracing of those who had solved it to their satisfaction beforehand. Their reasons for this or that movement in order to avoid error were already determined.

On the other hand, those who started to trace before having well in hand the proper way of doing it moved the pencil slowly, glancing ahead to study the whole figure before crossing or turning at some critical point. This looking ahead for possible errors and correct ways of procedure simultaneously with tracing is obvious in the report of *Ke* whose tracing time was 30 seconds,—the longest time except for *Gl*, who, though he traced the figure again and again in air, was still uncertain when he began to trace the actual figure.

(*b*) *Groups Os*. The same figure was presented to the groups under the same instructions. The time allotted was 7 minutes.

Questions	*Yes*	*No*
1. Did you consider this task more difficult and complicated than the preceding?	93	7
2. Did you begin to trace this figure immediately?	14	86
3. Did you run your eyes over these figures before tracing to see how they could best be traced?	93	7
4. Did you go over the sheet holding your pencil suspended until you got the successful way of doing it?	72	28
5. Did you begin on the inside figure?	78	22
6. Did you begin on the middle diagonal of the inside figure?	53	47
7. Did you begin on one of the outside points of the inside figure?	19	81
8. Did you begin on the square?	26	74
9. Did you begin on the diagonal in order to get it in?	41	59
10. Did you leave the inner figure before finishing it in order to do the square and then come back to the inner figure?	55	45
11. Supply any information that is not covered by the above questions.		

Fifty-seven percent succeeded in tracing this second pattern. Of the others, only two *Os* traced more than one copy. The others either had not time to do so, or else the end of the period found a few of them still searching for a successful way to proceed on the copy first supplied.

The reports in answer to the last request were scanty in this portion of the problem. They were, for the most part, explanations of why the figure was not correctly traced. Too little time given and too much time spent on trying to find the most advantageous starting point are examples. Comments paralleling those in the preceding figure were given. "I did one hurriedly to see how it would work out and I did it wrong. I saw my error and drew the second one correctly by using the lesson I had learned in my first one." "I slowly traced the figure, not having any predetermined course, and was careful not to pass over any intersections that I could not come back to. When about half completed, I saw the way to finish the problem." "I tried it once and got everything but the middle diagonal. It was merely a repetition of the other figure. So I started with the diagonal and got it out of the way."

The results in the main confirm those preceding. That the majority of the *Os* followed the same procedure as the individuals is evident from the comments and from percentages. Many *Os* never got beyond the earlier stages of apprehension, search for a procedure, and inspection-and-comment upon the object. Others, going further, acted under one self-instruction after another until the desired result was obtained.

Problem 6. This problem, based on the method of multiple choice, was designed to show 'insight' (= comprehension, sudden grasp, cognized relations?). Fourteen rectangular pill-boxes were glued side by side to a long narrow board. In one box a bit of cotton was hidden. The board was removed from *O*'s sight each time the location of the cotton was changed.

(*a*) *Individual Os.*

> *Instruction.* There is a bit of cotton in one of the boxes. Find it with the quickest possible dispatch, giving all your procedure aloud.

This problem afforded a very striking example of one self-instruction replacing another after having been upset by the experimental conditions. At no time, not even to begin with, did the *O* open the boxes with no plan. "The best thing is to open the boxes one by one from left to right" (*Mc*). "There is no way but to go through the thing methodically" (*Fe*). Even in the cases of the unsophisticated *Os*, where the formal instruction was obeyed quite literally at first, a definite system of procedure was followed. One *O* (1) opened all the closed boxes beginning at the left; (2) opened with both hands but began at the opposite end. "If this is going to run on for some time, I am going to begin to look for a system. Now that was in the third from the left. There must be some trick about it, so that if I get the right idea, I can go directly to the correct one" (*Si*). This system, self-instructed, varied from time to time as the occasion favored first one and then another. This is better illustrated in a later stage of the comment. (11th trial) "Last time it was third from that end. This time it is third from this one. That doesn't work every time. Maybe there is no regulation." (12) Opened one but it was not the correct one. Opened another and found it. "I am formulating a theory." (13) "It ought to be in this one by the theory, but it is not, and I hereby discard it." This *O* never worked at random although he recognized the uncertainty of his plan. "I may be on a totally wrong track trying to figure these out from the arrangements but I think it is a sensible way of doing it" (*Si*).

The pattern arrangement was the outstanding method of approach to most *Os*. "It occurs to me that I am getting some kind of pattern-test (like the animals') which I am to learn in the course of the presentation" (*Mc*). "I will begin at the left, examining each box as I go. It is in the third I opened next to three empty boxes" (*Ke*).

Any number of cues were tried—the pattern, marks on the boxes, the total number of boxes from one end, either end, and both ends, the number of closed next to empty boxes from either end, the number of boxes moved farther from either end at successive times according to a scheme, the same box every time, etc. The experimental period was marked with pauses while the *O* sat looking at the boxes, noting their position and arrangement, and striving to memorize them.

One *O* remarked that he "must look out for the danger of making it more complicated than it was." Nevertheless he reported at the end, "I made it complicated at first. I thought of all sorts of possible methods" (*Gl*). "I looked for something exceedingly difficult" (*Mc*). Self-instruction toward the difficult was not apparent in any of the other *O*s. *O* merely took advantage of what the occasion had to offer whether difficult or simple. *My*, who was in general the quickest and keenest to take note of all possible relevant conditions, commented, "It must be some simple system because it does not take you long to fix it" (*My*).

The process of elimination of one self-instructed method after another as the conditions changed was most obvious and most logically organized in the case of this same *O*.

1. "There it is." (After opening the boxes systematically from left to right.)
2. "I do not see any sense to it at all. I cannot find any order to it. The first time it was near the three thumb tacks, next it was third from the end. The more boxes I open, the less credit I get, I suppose."
3. "This time there are no threes to go by at all unless I go by the number of closed boxes." (Opened the third closed box from the left and found it.)
4. (Opened them by hunting for the pattern of three. Opened nearly all of them.) "I know that it has some connection with three. It was in the center of the only three closed boxes together."
5. (Opened the one near the group of three open boxes. Opened another and found it.) "I have the wrong pattern." (Examined the thing carefully and made a mark on one end of the board.)
6. "I marked it because I thought it might be in the same box every time, but it was not." (He counted three from the other end and got the cotton.)
7. (Opened from a cue from last time.) "Why do you have more lids on the boxes?" (Opened again and again and found it.)
8. (Opened a wrong box. Opened the right one. Counted the number of boxes.) "I wondered if you had moved it over one, but you had not. This time it was in the center of the pattern as it was two times ago, but that does not hold for all times."
9. (Looked where he did last time.) "Did you move it over three? Would it be a certain number of covered lids from either end? But figures do not prove it." (Then he found it.) "It is in the third from the end."
10. (Looked at the boxes.) "You do not move it over a certain number of times nor does the number three have anything to do with it, nor is it the center of a pattern, nor is it in the same one every time." (Tried the same box, but from the other end this time. It was there.)
11. (Opened the correct box.) "It is the third closed box over."
12. "I thought that when it was five from one end one time, it would be at the other end next time in the fifth box. I have never found it in an end one. I am trying to make sense of the arrangement. This time it is the sixth closed box from one end, and the third closed from the other."
13. "It must be some simple system because it does not take you long to fix it."
14. "Why is it never in the end ones?" (Counted the boxes.) "Next time I shall count the number of open ones."
15. "It certainly is no pattern."
16. "And it is not the same design this time. Apparently you do not have any system, but it has been in this one at other times." (He had opened the third closed one from the end and got it.)
17. "It is the third closed box from the end."
18. (Opened the third closed one from the right.)
19. (Opened the third closed box from the left.) "They alternate." (*My*).

This protocol is a fine example of the typical procedure in this particular problem. It is rich in illustrations of apprehension of arrangement and place, of search for some plausible ground on which to base a choice, of inspection-and-comment upon the various experimental conditions, of the formulation of the self-instruction dependent upon the occasion (arranged boxes) and action under it, of re-apprehension followed by another self-instruction and action under that, until, all irrelevant plans eliminated, the solving generalization is reached.

Results. The procedure in this problem, as in the others, was marked by a method, a systematic series of interacting self and occasional instructions. A solution was gained through, first, inspection-and-comment upon all conditions possibly relevant to the end sought, and, next, formulation of one self-instruction after another dependent on some occasional instruction until the generalization was reached. Here, again, there was no evidence of any uninstructed and planless activity. Action was always directed, predetermined, relevant to some intention on the part of the *O*.

(*b*) *Group Os.* The scheme for group-presentation consisted in placing the cotton always in the second closed box from the right end. It was necessary to make the scheme simpler than the *alternating plan* used above since the number of *O*s was greater and the presentation-time 10 minutes less.

> *Instruction.* I am going to present to you this series of boxes a number of times in succession. In one of them there is a bit of cotton. On the first presentation, I shall open the box in which the cotton lies. After that you are to write on the paper before you the number of the box in which you think it lies, counting from the right. After you have written down the choice each time, I shall open the correct box.

Questions	*Yes*	*No*
1. When the boxes were first presented to you, did you consider that the best way of finding the cotton was to open all the boxes to see where it was?	52	43
2. Did you try to remember which box it had been in from time to time?	74	25
3. Did you try counting the number of boxes from an end?	80	20
4. Did you try to remember the pattern arrangement from time to time?	60	40
5. Did you try counting the number of closed boxes from either end?	42	58
6. In naming the box in which you thought it might lie, was your reason for the choice based on some location you had noticed before?	84	16
7. Give any other information which you consider of interest or value concerning your solving process.		

The percentages and the reports again parallel those of the individual *O*s. "I tried to base my solution on some probable numerical sequence, as some multiple of two. This seemed to work, but failed in the last instance. Hence my solution was incorrect." "First I tried to find a difference from the others in the box containing the cotton. When this proved futile, I tried to notice whether the number of the box had anything to do with the number of empty boxes. At the end I thought it was the number of empty boxes minus one." "When I saw the first time that the right box was the second closed one from the right, I chose that in every succeeding trial, and, finding I was successful, did not try any other method." "My first idea was that there would be a telltale mark on the lid. I found several such marks, but the placement of the cotton did not correspond."

The procedure was obviously the same as above. Self-instructed, the *O* made one choice after another until the instruction was no longer plausible, whereupon another self-instruction dependent upon occasional instruction was formulated. This continued until either the solution was reached or the end of the presentation-period arrived. The results of this group-presentation confirm those obtained from the individual *Os*.

Problem 7. This problem was chosen to show 'insight' (= flash, new configuration?). Six matches were placed on the paper before *O*.

> *Instruction*. You have presented to you six matches. Construct of them four equilateral triangles without breaking the matches. Comment aloud.

All the *Os* went systematically to work on the presentation of the problem constructing plane figures involving triangles. This practice was interspersed with pauses where the *O* was "trying to visualize some large geometrical figure that can be broken up into four equilateral triangles," or "tried to think of all the figures involving four triangles, (some time since my geometry)." Search here was accompanied by inspection-and-comment, "Obviously each match will have to form the side of more than one triangle." "Each side of each triangle must be the length of one match." "The solution seems as if it might lie in utilizing only the half-matches but I can get only three triangles that way."

All the *Os* assumed, without any formal recognition of the fact, that the four triangles were to lie in a plane. Three of them (*Ke*, *Si*, and *My*) solved it by taking advantage of chance positions in their efforts to lay a plane figure. *Ke* made one triangle. Then he started to lay another match from the apex of it to the base. "As I was laying the match one end down and the other up, I got the idea of not having them in the same plane." *Si*, constructing plane figures, laid out two triangles with a common base so that the figure looked like a diamond with a line across the center. Taking the two matches nearest him, he lifted them, added the other, and constructed the tetrahedron. "I saw all at once that it was a tetrahedron required. Taking two of them up together gave me the notion of getting the third dimension." *My*, also constructing plane figures, started moving the matches around again and then paused with one in his hand. He started laying it down slowly, beginning with one end, "Oh, here it is," and constructed the figure.

The solution followed immediately on the chance occurrence which induced a change in the primary assumption. It undoubtedly was obtained more easily in the cases of *Ke* and *Si*, that of the former because he had taught geometry the year before and that of the latter because he was a physicist and accustomed to deal with mathematical media.

The other *O*s (with one exception) concluded it impossible to construct the required triangles. When no other arrangement could be formed under the self-instruction, the *O* sought to terminate the period. He never continued to move the matches aimlessly. On the second and third presentations he acted under a self-instructed plan; but when the instruction ceased his action ceased.

Mc never realized the impossibility of constructing the figure according to his plan. Even when shown the solution, he never saw it as a three-dimensional figure of planes in various relations to each other, but as a solid. "The idea of a solid never struck me at all. Triangle means to me something in a plane."

Results. The solution here consisted in a recognition of the three-dimensional alternative. Perceptive apprehension of a significant condition was prevented by a predisposition to lay figures in a plane. For some this set was so strong as to inhibit a recognition of the implications attendant upon lifting the matches from their two-dimensional arrangement to place them again in another plane arrangement, while for others a self-instruction that there must be something tricky or catchy about the problem created an attitude of search and uncertainty in the attempts to lay a plane figure satisfying the conditions. For these the re-apprehension of the type of figure required entailed a recognition of the predisposition to lay plane figures with the resulting formulation of another self-instruction.

Action under self-instruction, accompanied by search for a method of solution and inspection-and-comment upon the situation characterized the procedure. For those who solved the problem, the occasion brought about a re-apprehension with a consequent recasting of the instruction.

Problem 8. This problem also was intended to show alleged 'insight' (of the same sort.) The experimental object consisted of a flat steel rectangle with a slot of definite pattern cut in it. The right-left slotted groove was long and had annexed to it above and below 2 shorter T-shaped prolongations. Ten steel buttons, which could be turned or slid in any direction, were permanently fastened in the slot. Each bore a letter of the word 'perplexity,' the letters arranged in haphazard order. Because of the arrangement of the slot, the letter 'p' could not be moved from its position at the right of all the others. The solution lay in turning the puzzle and the letters on the buttons upside down with reference to the obvious top of the puzzle, which was indicated by the imposed name "The perplexing puzzle."

Instruction. Solve this puzzle, commenting aloud.

In every case the *Os* assumed at first that the word was to be spelled according to the words lying above the slot. Under self and occasional instructions, *O* shifted the buttons in every imaginable way. "It is just a matter of moving them around" (*My*).

Search and inspection-and-comment under a misleading occasional instruction characterized the early stages of the solving process. "Where has it got to be when I spell it? Where is the trouble? How do you get the Y over there? The Y is stuck" (*My*). "This takes three, and this takes three, and four have to go there, but they won't" (*Ke*). "You have to get the Y out of there first. You cannot do it. I wonder if any of these are uneven? They are all eccentric; they all roll round" (*Si*). "The Y should go to the end. I see that it is going to be the whole difficulty to get the Y out of there. The P won't let the others pass. There is something fishy here. Is there any possible way of getting them past each other? There are too many letters for this. Now what am I going to do?" (*Gl*).

At times throughout the experimental period the *O*, with an abstracted look on his face, would cease to move the buttons around, or else do so in an aimless, desultory way; but, when asked what he was doing, he reported, "I am trying to see what I can do." "I have got all but one letter in the correct positions, but I do not see how the P can be got in" (*Fe*).

Marking the later stages of the solving process were action under self and occasional instructions and elimination of one plan of action after another, shifting the buttons around until the realization was reached that the letter P could not be moved from its end position. At this point two *Os* declared its solution impossible. "I still have that P down there, and there is no way of getting it out. There must be some way for P to pass some of the others but I cannot see it" (*Ke*). "It is no use forcing the metal; that would not do any good. I cannot get that Y out of there to save me" (*Gl*). One *O* never realized this impossibility because he failed to notice the relative positions of the letters at the beginning of the observational period. "I did not remember whether the P was in that position to begin with or not. I might have got it in that place by moving the buttons around" (*Fe*).

For the others, the impossibility of the removal of the P from the end suggested utilizing that letter as the beginning of the word. "Perplexity is presumably to be spelled in this direction, but I can spell it in the other direction. May I? I can turn it upside down which is not against the directions" (*Mc*). "When I picked it up, I realized that the P was still on the end and there was no way of getting it on the other side. I must be able to spell it with P on that end, then. And it occurred to me that I could turn it upside down, twist the buttons around, and spell it forward" (*Gl*). *My* spelled it backward and turned it bottom-side up. "It was the only way it could be done. I had tried every other way of doing it and was bound to get this way sooner or later" (*My*). "The labelling would indicate that it was to be spelled like it but the P cannot be shifted" (*Si*). He spelled it backwards and then turned the puzzle upside down. "Ho, ho, wait a minute!" and he turned the buttons so that they spelled it forward, and laughed, "I turned it upside down to read it backwards, then I thought of using a mirror, and then I saw the letters could be turned around."

Results. The procedure in this problem, as in others, consisted in apprehension of the object, search for a method by which to spell the word, inspection-and-comment upon the conditions of the situation, action under self and occasional instructions, and reapprehension due to a further realization of the limiting conditions of the experimental object.

The assumption that the word was to be spelled according to the one paralleling the slot was a misleading instruction from the occasion. Failure to arrive at the solution was due either to no change in this assumption, or to an imperfect apprehension of the experimental material, as in the case of *Fe.*

Problem 9. The following word-series were designed to bring out 'insight' of some kind. The approximate sounds of two famous Shakespearean passages—Macbeth's tragic soliloquy from the Fifth Act and the soliloquy from *Hamlet*—were presented in serial form. The Macbeth was given first because it was the less familiar.

Macbeth's Soliloquy

tomb
arrow
and
tumor
row
in
Tom
are
oh
crypt
e'en

this
pet
tee
pays
from
data
day
tooth
he
lass
silly

bull
of
record
dead
thyme
an
awl
hour
yes
todays
have

lie
Ted
fools
the
weight
too
does
tea
death

Hamlet's Soliloquy

two
beer
knot
tube
bee
thought
is
thick
west
shun

weather
'tis
know
blur
inn
them
end
twos
offer
this

sling
sand
air
row
sough
out
ray
just
four
tune

oar
toot
ache
arm
sag
ens
tea
sift
rub
bulls

(*a*) *Individual Os.* One of the above series was placed on a sheet of paper without the title.

Instruction. Read aloud.

The *Os* began by reading the list once. Then they invariably paused, running their eyes up and down the list while commenting. "It is a group of disconnected words. I did not think of the meanings of the words at all. Several of them are rather unusual" (*Mc*). "I noticed that there were some nicknames and a few words together tended to make sense, and some I sought to pronounce in a different way like 'row' " (*Gl*). "I looked over it and thought of some catch in pronunciation. I thought only of each separate word. Then I started reading down fairly rapidly for myself. The 'petty pace' part gave me the first inkling" (*Si*). "I thought it might be like James's 'Pas de lieu Rhône que nous.' Then I saw 'tube' and 'bee' which looked like 'to be.' I suspected what it was then and went to reading it systematically" (*Fe*). "These words, 'tomb, arrow, and tumor,' fell together as I read them I thought the list might be only scattered phrases, therefore I took them separately" (*Ke*). "When I read it, I knew it sounded like something faintly familiar and the first phrase and the last one made sense right off. The rest of it is this" (*Mk*).

The solution was dependent mainly upon the occasional instruction and only in part on self-instruction. Search is evident in the report where the *O*s ask themselves "what it can be for" or wonder "what you want done with this," or an *O* wishes that he "knew what he was to get out of it;" but in any case the *O*s knew that there was something there, not apparent but to be found.

For *Mc*, who failed to recognize the passages after several readings, the formal instruction was changed twice; first to "Read until learned," and then "Read through quickly." Under the first he got slower and slower in his reading. "They are disconnected words for groups of which I have to manufacture connections to help me learn . . . . When I tried to learn it, the slower I went, the more disjointed it became." On reading it quickly he "noticed this last time 'weather 'tis nobler.' I am beginning to get a glimmer that this is a meaningful sentence of some sort."

Gl's complete ignorance of one of the passages in question and his very imperfect acquaintance with the other prevented his getting the meaning. Time and again he struck a leading note: "A few words tended to make sense," and "I wonder if I could make sentences out of it. No, they are too disconnected." Even when the instruction was changed to "Read aloud as quickly as possible" and the report reads, "I could not help noticing the rhythm—there are places in it where a number of words almost make sense," he made no attempt to work it out as had *Ke*, (who was very unfamiliar with the passage first given him), but remained doubtful concerning it. The meaningful phrases suggested by the words were never like those in the original passage, but were like the words of the lists, *e.g.*, 'to be thought,' 'offer this sling.'

Ke, starting from the first phrase which "fell together," methodically worked out the remainder of the passage on paper, repeating phrases aloud and going back to change those already done to fit the meaning. "Two or three of these could be 'tomorrow,' and then there is 'pays from day to day to the last syllable of recorded time.' And 'whole hour yesterday.' He went over it slowly, writing down these phrases. 'Have lighted fools the way to dusty death. Tomorrow and tomorrow and tomorrow,' I see that all right.' He looked at the list awhile. 'Creeps—creeps in this petty pace.' He read on slowly, 'and all our yesterdays.' Then he read from the paper the whole passage correctly.

Results. When one passage was identified, the *O* was instructed by the occasion that the other was similar. Therefore the stage of undefined search was absent in the second procedure. In general, the *O* who recognized the first passage also recognized the other; while for the one who did not recognize the first, the second also was hard of recognition.

The disconnected and strange series of visual words tended to inhibit the recognition of the rhythmical and familiar sequence of verbal sounds.

The procedure was characterized first as perceptive and comprehensive, accompanied by a vague and diffused form of search. Occasional instruction tended to objectify concretely this element and to lead to verbal recognition. Auditory cues from a swift reading aloud led in some cases to identification with the familiar literary passage. Where the passage was not so familiar, the *O* worked out under self-instruction the meaning of the whole in a methodical way.

(*b*) *Group Os.* The series of words simulating Hamlet's soliloquy was arranged on a white cardboard 3 ft. x 2½ ft. The other series was omitted because of the lesser familiarity of the quotation.

> *Instruction.* I have here a chart on which there is a series of words. I shall present them to you and we shall read them aloud in unison three times, increasing the speed with each reading. After the last reading you may immediately turn over your questions and answer them. Here are the words. Let's begin.

Questions	*Yes*	*No*
1. Did you wonder what it was all about when you were asked to read these words?	88	12
2. Did these words remain a group of disjointed words without any connected meaning?	53	47
3. Did you think that they might make sense before you read them?	37	63
4. Did any of the words fall together to make sense as you read them?	77	23
5. Did the whole series make sense only after the second or third reading?	27	73
6. Tell anything about your procedure that these questions have left out.		

From the percentages it is evident that about three-fourths of the *Os* failed to recognize the passage. The causes of this failure may have been either lack of familiarity with the passage or the dominance of the visual over the auditory stimuli.

The reports run parallel to those of the individual *Os*. "The increased speed in reading seemed to make them more meaningful and I left the visual words for the audible." "The unfamiliar word 'sough' was the most definite cue to me that this series was not disconnected." "There seemed enough verbs, nouns, and adjectives to make complete sentences, but there was not time enough." "I thought it was an association-experiment and I tried to memorize the words but only a few of the unusual ones stayed." "Certain words went together like 'thick, west, shun,' and 'toot ache' that could have been 'toothache.' The whole did not make sense."

The dependence of the solution on occasional instruction is quite obvious from these reports where occasional instruction (*e.g.* that it was an association-experiment) often inhibited useful cues. This may be added to those given above as another cause for failure to apprehend the words as familiar and well-known.

Problem 10. This problem was designed to show whether some form of 'insight' was concerned in applying a physical principle to a problematical situation. A deflated toy balloon, a 75-cc. flask, penstaff, pencils, and file were presented to the *O*.

(*a*) *Individuals.*

Instruction. Blow up this balloon and tie it inside the bottle.

Here the simplest and most obvious performance was to place the balloon in the bottle and blow on it. That was the procedure invariably followed, by all *O*s,—even by *Si*, a physicist. Failure led to the question, Why? "Now, why the deuce won't that blow up in the bottle?" (*Si*).

In the majority of cases the *O* quickly recognized the cause as air-pressure and lack of an escape for it. "I must have something to let the air out" (*Ke*). "I just thought of letting the air escape as I put the balloon in" (*Mc*). "All the air is in there and I have to get it out or the balloon won't blow up" (*Gl*). "I saw that it was air-pressure keeping it from blowing up" (*Si*). *My*, the logician, was the only one to make a correct scientific analysis. "To blow it up would have required compressing the air in the bottle but lungs are not strong enough to do that." An excellent example of employing an object as instrument out of its usual context occurred in this case. After finding that the balloon did not expand, he asked, "Can I use anything else?" He looked over the table and said, "I haven't the right machinery. Oh, yes; I am going to show you something now." He quickly unscrewed his pipe-stem from the bowl, inserted it by the side of the balloon and inflated the balloon, the air escaping through the pipe stem. He reported, "Therefore I put a tube in the neck of the bottle and then the air in the bottle escaped through the tube." The pipe-stem was apprehended no longer as a pipe-stem with its specific application but was generalized into a tube.

Fe, who never solved the difficulty, reported that "the problem never announced itself to me as physical. I knew the air had to get out but I never thought of it as a physical problem." To what extent that prevented a solution on his part is doubtful. In the commentary he formulated the question, "How do you get the air out of the bottle?" and he "does not see how the air can escape." Here is a definite recognition of the difficulty but no instrumental solution is suggested.

The other *O*s used the penstaff and pencil indiscriminately. *Ke* picked up the file and inserted it but decided it might break the balloon.

Results. The procedure consisted of action under self-instruction followed first by a period of search for the cause for failure under the first trial and then by a re-apprehension of the experimental conditions with a consequent new self-instruction followed by action. As a general rule, when the cause for failure to inflate the balloon on the first trial was comprehended as due to air-pressure, the solution by instrumental application followed immediately.

Except for one case, *Fe*, formulation of the difficulty led to a consideration of the means of solution. In this case, however, imperfect comprehension of the laws of air-pressure inhibited the apprehension of a means of solution.

(*b*) *Group Os.*

> *Instruction.* If I place this balloon in this bottle and blow, thus, no matter how hard I, or anyone else, blows, the balloon will not go up. Nevertheless, there is a way by which the balloon can be blown up in this bottle under these present conditions. If I gave you the balloon and asked you to blow it up in the bottle, how would you do it?

Questions	*Yes*	*No*
1. Did you find a way of blowing up the balloon in the bottle?	73	27
2. Did you decide that the air in the bottle prevented the balloon from blowing up?	72	28
3. Did you search for some means of allowing the air to escape?	61	39
4. Did you consider any of the objects lying on the table?	69	31
5. Did you think of some absent object as a means?	83	17
6. Did your solution consist in holding some object in the mouth of the bottle parallel to the neck in order to allow the air to escape?	47	53
7. Give any of your attempts at solving which are not covered by these questions.		

Various methods of obtaining the desired result in addition to the one in the sixth question were reported in the answers to the last question. "I thought of pushing the balloon in the bottle, thus allowing the air to escape as the balloon blew up. Of course, this entailed having a tube fastened to the neck of the balloon through which one could blow. I could not find the instrument on the table so considered the problem unsolved." "Wire wrapped round the neck of the balloon (in the neck of the bottle) to keep it smaller than the neck space and allow the air to escape as the balloon got larger." All sorts of other means were offered as impractical as, or more impracticable than, those quoted.

Others reported concerning the sixth question. "The object I should use is a corkscrew with the point away from the balloon because then the balloon could not expand around the object to shut off egress of air." This was another example of an instrument, not only used out of its usual context but assumed to have the generalized character of a tube. "I solved it nearly instantaneously due to my knowledge of physics and air-pressure." "Because of the question in the bead problem, I definitely started by looking over the table." The *O* was self-instructed here from a cue from a preceding experiment.

Results. Great variety in type of solution is evident in the results. Those *O*s who never apprehended the cause of the difficulty naturally failed entirely to reach any solution. Others, self-instructed from past experience, suggested various methods for the most part entirely unsuited to the experimental conditions. Others apprehended the correct cause of the difficulty but spent the allotted time in search for a method of solution; while still others, apprehending the difficulty, had no trouble in immediately apprehending the best and simplest means of solution under the conditions given.

Inhibiting factors were, first, failure to apprehend the cause of the difficulty due to an imperfect acquaintance with the behavior of gases, and, secondly, self-instruction from past instrumental applications combined with failure to apprehend limiting conditions in the instruction and the experimental situation.

Problem 11. This problem was chosen as parallel to the foregoing. The *O* was presented with a bit of wood about 2 in. long, beveled convexly below and so rocking easily from end to end. The top was channeled out longitudinally with a concave bottom. Two small shot were placed in the groove which was then transparently covered. As the piece sat on its rocking bottom, the shot lay together at the centre, but separated by a wire partition. Small pockets were dug out at the outer ends of the groove. The problem was to cause the two shot to go right and left at the same time and so ride up to the two pockets. The solution consisted in twirling the piece suddenly, thus sending the shot centrifugally to the two pockets.

Instruction. Place a ball in each pocket simultaneously. Comment aloud.

All the *Os* began with the self-instruction that it was a matter of manual dexterity or skill. When that principle failed them, some assumed it a mere matter of chance, which was denied by the experimenter. As a result of that self-instruction the *O* tended to cease in his efforts at solution. "I do not see that there is anything to it but manual dexterity. I do not see anything but that it must go in by chance." (*Fe*). "I think it is a mere matter of skill. . . . It will just be chance if it falls in" (*Ke*). "Is it a trick of dexterity or is it a matter of luck?" (*My*).

Every method that might lead toward a solution was tried with the object in every possible position. Only in extreme impatience did the *O* violently and aimlessly shake the object. Every way of holding the box, of sudden striking and jerking, was tried,—a methodical procedure with a definite basis. "The best bet is to get one in a hole and hold it there while rolling the other along the glass top, but I have not succeeded in doing it yet" (*Mc*). "I am trying to keep one ball in the hole while I quietly and diligently get the other in, but I get it nearly there when the other one moves out" (*Gl*). "I have a better idea, I will put one in and then put another one in by striking the bottom sharply" (*My*).

Gl and *My* noticed the structure of the puzzle. *My*, who solved it, said emphatically, early in the experimental period, "One thing is certain, I suppose that wire in the center (partition) has something to do with it. . . . Why is it made in a rolling fashion like that? There are no grooves in it any place, it is just semicircular. That wire must mean something—that one is to be on either side of it." *Gl*, who did not solve it, commented in the interrogative, "Why was it made like this? That wire down there is kind of funny. Why do you suppose it is there?"

That there might be some mechanical principle whereby it could be solved occurred to most *Os*. "One could set up an oscillatory motion so that the balls go up and down in opposite directions, I suppose" (*Mc*). "I do not know any principle of mechanics whereby both could be put in together" (*Gl*). *Fe*, when told that the puzzle was obtained from a physicist, commented interrogatively, "I wonder if the inclined plane has anything to do with it?" *My*, who solved the problem, reported that, "If I had a magnet, I believe I could do it. . . . I wonder if it depends on any scientific law. It looks as if the wire should keep them apart. You have to get them in a place where a single motion would put them both in at once. . . . What kind of motion would drive them in? The only thing left after thinking of all possible forces was centrifugal."

When those who desired to cease trying for the solution were told that the secret of it was centrifugal force, various comments were obtained showing that, though centrifugal force as a name was familiar, not one *O* had a clear notion of the significance of the concept. "I have heard of it but it means nothing to me" (*Fe*). "Centrifugal force—force toward the outside? I never would have thought of spinning it. I thought it was a mere matter of skill" (*Ke*). "Centrifugal force? I know what that is" (*Gl*). He got up and whirled around holding the puzzle in his outstretched hand. "All it ever meant to me was a whirling pail of water" (*Gl*). "When you spin a thing around?" (*Mc*). He took it in hand and tried to go around with it as one whirls round with a bucket of water. Even though these latter two now knew the principle of the solution, they never tried the correct way of spinning the object.

Results. The characteristic procedure here for those who came nearest the solution and for the one who attained it was action under self-instruction accompanied by search for a different and possibly more relevant method of solving, and by inspection-and-comment upon the experimental object.

In three cases the first self-instruction of manual dexterity was never changed—a self-instruction inhibiting the apprehension of relevant conditions which would necessitate a reformulation of the plan of procedure. When *O* found that skill of manipulation did not obtain the desired result, he ceased to attempt the solution. When he had no other feasible plan, he never continued to try aimlessly.

In other cases failure to solve could be attributed to the inhibiting effect of a vague and imperfect knowledge of the controlling physical principle.

Problem 12. This problem was designed to examine an 'insight' which might divine the suitable. Two series of numbers were composed. The pattern of the one first presented was as follows. To all odd numbers additions were made; from all even ones, subtractions. The numbers so added and subtracted ranged from 1 to 14. After 12 first appeared, it then appeared in every 4th place. The pattern for the second was a subtraction-and-addition pattern; thus, $-3+2-1, +6-5+4$. . . .

First Pattern		Second Pattern	
13	13	24	26
14	22	21	25
12	12	23	31
9	1	22	26
13	13	28	30
18	26	23	27
12	—	27	—
5		24	

Instruction. Here is a series of numbers with the last one lacking. Supply the correct number. Comment aloud.

The interesting thing about the first series for the *Os* was its pattern arrangement. All gave 12 as the number, basing their choice on this arrangement. The formal instruction was as meager as possible, leaving wide range for self- and occasional instructions. Not one *O* made any initial assumption that the other numbers followed necessarily from the first according to some ruling principle. All read the series first, inspecting its characteristics and generally seizing on what the occasion had to offer.

Various patterns were described but the one leading to the right number was the most obvious and the most generally found. "Beginning with the third number, every fourth number of the series is 12. Therefore the last one would be 12 to fit in the rule. I saw it was a pattern 13, 12—13, 12 all the way" (*Fe*). "I happened to notice that the numbers occurred in groups of four—that the first number of each group was 13, and the third (?) was 12" (*Ke*). "There are three groups with smaller ones separating them. I would say it was 12, just to keep up that sequence" (*Si*). *Mc* read the series and was struck by the regular rhythmical recurrence of certain numbers. "Without analyzing, one would say that 12 is the number because every fourth time it comes in. Just the fact of the rhythm alone would make one think that 12 was the number" (*Mc*).

My was partially self-instructed in the search for the number, "Do they go in pairs, I wonder?" He tried various schemes of adding and multiplying. After finding himself wrong, he re-read the whole series and announced, "According to the pattern the number should be 12." Occasional instruction, here as in the above cases, led to the correct number.

Gl, under self-instruction, commented, "I went through the series getting the difference between 13 and the other numbers. I found a constant relation between it and the third (?) number from it. I therefore judged it to be 12. I simply took 13 as a standard and worked from it."

Greater variety was shown in the second series. Two *Os* obtained the number by working out the scheme according to which it was composed, one because he had inquired as to the way in which the former group was constructed, "Is that the same business of adding and subtracting?" (*Fe*); the other because systems of additions and subtractions were the simplest and most often used means of forming a series, "I read through the numbers and did not see anything. Then I began by taking the differences. I tried to see if that were the thing here, since most series are made up in that manner" (*Si*). Here the *O* was self-instructed to search for *E*'s method in constructing the series.

The occasion, however, furnished the basis for the other solutions. Self-instruction was less useful. "I have already decided that you are not going to use the same method you used before" (*Ke*). "I suppose you start with a number and the rest follow by rule" (*My*). Such self-instructions were wholly abortive. The pattern gave the correct number except in the case of *Mc*, who, by two pattern schemes, obtained the incorrect number 35, and in the case of *My*, who obtained the correct number but not through working out a pattern. By a process of elimination, counting the numbers and the number of times each occurred, he inferred that the only number in the twenties not occurring at all (*i.e.* 29) was correct.

Results. The progress toward solution consisted in, first, apprehension accompanied by inspection-and-comment upon the series and by a search for any cues likely to be of aid in obtaining the desired number, and, secondly, occasional instruction (in most cases), a self-instruction (in a few), under which the number was supplied.

Strong occasional instruction suppressed the formulation of self-instruction in the majority of cases.

Problem 13. This problem also was designed to explore for 'insight' discerning the suitable. The following stanzas with one word lacking were presented in succession. Care was taken that only one word, and that the author's, could be supplied. The five examples were chosen to bring out differences in literary perception of form and in available vocabulary.

(1) Yea, in the valley of Death I awoke,
Pallid and strange as a vision.
All of my sorrow is vanished as smoke—
These are the valleys..............

(2) Once where the unentered temple stood, at noon
No sun-ray pierced the dim unwindowed aisle,
And all the flooding whiteness of the moon
Could only bathe the outer..............

(3) Let us go hence; the night is now at hand;
The day is overworn, the birds all flown;
And we have reaped the crops the gods have sown;
Despair and death; deep darkness o'er the land
Broods like an owl; we cannot understand
Laughter or tears, for we have only known
Surpassing vanity: vain things alone
Have driven our perverse and aimless............

Let us go hence, somewhither strange and cold,
To Hollow Lands where just men and unjust
Find end of labor, where's rest for the old,
Freedom to all from love and fear and lust.
Twine our torn hands! O pray the earth enfold
Our life-sick hearts and turn them into dust.

(4) The skies, they are not always raining
Nor grey the twelvemonth through;
And I shall meet good days and mirth,
And range the lovely lands of earth
With friends no worse than..............

(5) Well! wind-dispersed and vain the words will be,
Yet, Thyrsis, let me give my grief its hour
In the old haunt, and find our tree-topp'd hill!
Who, if not I, for questing here hath power?
I know the wood which hides the daffodil,
I know the Fyfield tree,
I know what white, what purple fritillaries
The grassy harvest of the river-fields,
Above by Ensham, down by Sanford, yields,
And what sedged brooks are Thames's............

(*a*) *Individual Os.*

Instruction. Here is a verse with one word lacking. Supply the correct word, doing so aloud.

The first verse is composed of four alternately rhyming lines, one of the most common of rhyme schemes, the last rhyme lacking. A word of three syllables, the word 'Elysian,' which is fairly familiar, was to be supplied. All *Os* followed more or less the same procedure in attempting to supply the word. Some trusted to the feel they had for what was required. "One word to rhyme with 'vision.' 'Elision,' 'provision,' 'incision, 'ambition.' All right, 'Elysian.' That is the correct word" (*My*). "It seems as if there should be more than one word, 'valleys of something.' I am trying to find something to rhyme with 'vision' " (*Gl*). *Fi* read it again rhythmically, beating her foot to keep time, and supplying 'dum dee dum' in place of the word.

Other *Os* analyzed in greater detail with regard to metre, rhyme, and part of speech. "It needs three syllables, accent on the second to rhyme with 'vision' . . . I thought, 'Why should not an adjective do?' " (*Fe*). "The name may be some foreign proper name. This notion was strengthened by the fact that it goes with a plural noun with only three syllables to supply. There is a posture that it must be an adjective that is needed as I recall the poetic formula of modifier after noun" (*Je*). "The word has three syllables, the last part of a dactyl, and a trochee. 'Decision' fits the rhyme but makes no sense" (*Mc*).

One *O* (*Ke*) read the word immediately into the blank. Otherwise an attitude of search occurred. The others (whether or not the right word was obtained) invariably substituted other rhyming words which had no connection with the sense in attempting to get the correct rhyming word. 'Derision,' 'division,' 'decision,' 'incision,' 'elision,' 'collision,' 'oblivion,' 'provision,' 'ambition,' were variously employed. *Je* was prevented from supplying the correct word obviously because he continually assumed the word to end in 'ision,' identical with the ending of the word 'vision.'

Those to whom the word 'Elysian' occurred were convinced that it was correct if familiar to them. To *Si* the word occurred immediately, " 'Elysian valleys,' something like that is used in classical poetry. I am not sure of the word and I cannot spell it. It begins with a capital E, but I am not sure of the rest. I guess that would be right. . . . Yes, wasn't it the Elysian fields that Greek heroes went to after death?" To *Mc* and *Gl* the word was none too familiar. *Mc* reported, "I knew the word. 'Elysium' was the only thing that meant anything here, however. The adjectives from it simply did not emerge;" and *Gl* reported, "I knew it but I never thought of it. I never use it ordinarily, anyway."

The second verse is also a 4-line alternately rhyming verse with the last word lacking. This last word, 'peristyle,' is semi-technical and ordinarily absent from the vocabulary of common usage.

The search for the word ran parallel to that of the other verse. Words like 'domicile,' 'campanile,' 'mile,' etc., were supplied. Only in one case was this absent. *Ke* read 'peristyle' into it immediately. That the metre was harder here was indicated by the large number of one-syllable words supplied. *My* and *Gl* never realized the fact that more than one syllable was required. "It might be any noun that rhymes with 'aisle'—'style,' 'file,' 'defile,' 'mile,' 'Nile,' etc. The rhythm is so irregular that it might be any syllabled word. It ought to be 'pile,' a one-syllable word. A two-syllable word would be awful" (*My*).

Without formally stating it, *Mc* assumed that the word was at least of more than one syllable. He read 'domicile' into it. "Words like 'pile' spring up but they are too short." *Si* methodically attempted to get the metre, "I do not know whether it would be one syllable or three. I think it could get away with one. I will try 'domicile,' which is not the word for the metre. Three syllables sound better."

Mc, *Gl*, and *Si* tried to obtain the word from the meaning as well as from the rhyme. "It looks as though it would refer to the vestibule or something like that. Is this a technical word connected with a church? I am now visualizing a cathedral with various parts that might be illuminated by the sun. This term might apply to the roof or walls, or to the general outer part of the church" (*Mc*). "It would not be good poetry but you could say the 'outer tile' if it were made of tile" (*Gl*). "What is outer that is bathed by moonlight? So many words have three syllables but that is all. Minaret is not right, of course" (*Si*).

Only one *O* (*Mc*) had no acquaintance with the word. *Gl* and *Si* had heard of it but had no knowledge of its meaning. *Ke* had heard the word until it was familiar enough to read directly into the verse, but he was not clear as to its meaning. "What does 'peristyle' mean? It fits according to the rhyme and metre but I do not know what it means."

The third verse, the Italian sonnet, was selected for its definite and unchangeable rhyme-scheme and metre. Anyone knowing the sonnet form would have no trouble in analyzing the rhyme-ending and the exact number of syllables required. As a matter of fact, no one of the *Os* analyzed the metre to determine the number of syllables lacking. In reading the verse there seemed to be a feel for the correctly syllabled word, probably due to the familiarity, formally recognized or otherwise, of all *Os*, with the sonnet form.

Two *Os*, *Mc* and *My*, inferred the rhyme from the form. "Oh it is 'band.' I have written too many sonnets not to get that, a b b a, a b b a,c d, c d, c d", (*My*). *Mc* supplied 'band' and said, "It is the sonnet form and I knew it would have one of the 'and' endings," but added that the rhythm or metre puzzled him at first. "The rhythm does not seem to be any good. This tended to upset the rhyme-scheme."

The others without recognizing the sonnet form gave some reason for their choice of a rhyme. "I knew it had to rhyme with 'understand' because before it had been 'hand,' 'flown' 'sown,' 'land,' and 'understand,' 'known,' 'alone;' must be 'band.' It naturally falls into first and last, second and third rhymes. I divided the thing into quatrains, and 'band' just came like that" (*Ke*).

Ke and *My* supplied the proper word from the requirements of the rhyme and metre. The sense was not considered except in so far as it did not contradict the word supplied. The others obtained the word through a consideration of the meaning. "I looked then at the sense and 'band' came" (*Mc*). "Oh, 'us,' that makes it plural. 'Have driven our perverse and aimless band'" (*Gl*). "'Band' is the most reasonable word. 'Land' and 'hand' do not fit so well. A land is not driven generally. The 'us' is not just an editorial 'us' because it says 'our life' and 'sick hearts'" (*Si*).

The fourth verse was chosen for its irregularity of rhyme-scheme. All *Os* read the word 'you' into it, more from a sense of the demands of the verse as a whole than from any one variable, such as the meaning, the determination of the metre, or the feel for the rhyme. This was obvious in the comment of *Ke*, "I do not know why I rhymed it with 'through.' 'You' just came like that and it just happened to rhyme with 'through.' I did not definitely try to rhyme it with anything." "It is the sort of poem addressed to someone, 'friends no worse than some person' and it has to rhyme with 'through'" (*Si*). "It would not look well rhyming with 'earth;' besides, this is the usual way it rhymes. It means something to rhyme it with 'through' and it did not mean anything when I sought for a rhyme for 'raining'" (*My*).

The rhyme played a major part in the search of *Mc* for the correct word. "When I came to the third line I noticed 'mirth' and thought of 'birth' but when I met 'earth' I knew it would not do. It was a closed couplet. Also it would not rhyme with 'raining.' 'You' came and fitted in the sense." Here a very cogent reason was given for not having a consecutive three-line rhyme, but no reason at all was set forth for choosing a rhyme for the second line rather than the first. An aesthetic element, a feeling for what is most pleasing, seemed to enter here; for instance, "I do not know why I think it has to rhyme with 'through,' but it sounds better and brings the poem to a close" (*Si*).

In the last verse the meaning played the leading rôle in obtaining the absent word. "The sense decided me in favor of the 'fritillaries' rhyme" (*Ke*). "The brooks suggested Thames's 'tributaries'" (*Gl*). "I tried to do it by the sound but could not do so. Then I took the sense and sought for the relation of brooks to the Thames" (*Si*). "It filled up the sense of the sentence and rhymed with the unusual word 'fritillaries'" (*My*).

Although the meaning was the surest means of obtaining the correct word, the rhyme was given a fair amount of attention. There was very little uniformity among *Os* on this score, although all of them supplied the correct word. Two of them attempted to rhyme the word otherwise than with 'fritillaries.' "I did not know when I first read it whether the word to supply would rhyme with 'trees' or with 'fritillaries.' . . . I knew it rhymed with that word because no one would use a word like that unless he were using it for a rhyme" (*My*).

Mc was the only one to differ markedly from the other *Os*. Whereas they laid the major emphasis on meaning and only minor emphasis on the rhyme, with none whatsoever on metre, he gave prime importance to the consideration of rhyme-scheme, secondarily considering the metre, and, as a last resort, the meaning. "As I read it, it flashed through that it might be a sonnet. Then the line ending with 'tree' showed it not to be that. 'Tree' rhymed with 'be.' All the other lines are complete couplets and 'fritillaries' stood out alone without a partner." Then he read the line ending in 'fritillaries,' rhythmically beating his fingers on the table. " 'Fritillaries,' two trochees." He read the last line also moving his fingers to the rhythm, "Two trochees there, too. 'Tributaries.' I was not sure of the exact word required. What could the Thames have that I might know? 'Capillaries' came but it was not the right one. Then 'tributaries' came in with a click, not logically. I was too much concerned about the structure of the word and too little about the meaning."

Results. Apprehension of the material was followed by a period of search which varied from a general diffused type, where the feeling for what was suitable to the verse as a whole was considered, to a more concretely objectified search as requirements relative to rhyme-scheme, metre, meaning, and part of speech were analyzed and commented upon. Inspection-and-comment was a function much used.

Lack of vocabulary or a faulty apprehension of the literary form were inhibiting factors.

(*b*) *Group Os.* Copies of the first and fourth examples were presented to members of the groups.

Instruction. Here is a verse with one word lacking. Supply the correct word.

Questions (*1st selection*)	*Yes*	*No*
1. Are you familiar with the word 'Elysian'?	47	53
2. Did you read the verse as if you were scanning it?	69	31
3. Did you count the number of syllables required?	45	55
4. Did you try to rhyme the word with 'vision'?	96	4
5. Did you try to rhyme it with any other word?	15	85
6. Did the meaning suggest the word?	35	65
7. Did you supply the word and then test it by the meaning?	24	76
8. Did you name words ending in the 'ision' sound which fitted the rhyme but not the meaning in your search for the proper word?	80	20
9. Were you sure of the word which you supplied?	29	
10. Was this certainty due to the fitness or suitability of the word?	28	
11. Make other comments on how you went about supplying the word.		

Twenty-nine percent of the *Os* supplied the correct word, 38% left it blank, while 43% supplied various words with a more or less correct rhyme but, for the most part, lacking in meaning. These words were identical to a great extent with the words named over in the search for the proper word by the individual

Os; for example 'decision,' 'oblivion,' 'elision.' On the other hand, some of the words supplied were imperfect as to rhyme, but the meaning suited the context. Such were 'Stygian,' 'Elysium,' 'eternal,' 'arisen.' The *O*, when he did supply a word, based it on his apprehension of what was required and his inspection of the conditions. It is interesting to note that, whereas 72% of them supplied a word, only the 29% who gave 'Elysian' reported a certainty concerning the word supplied.

The reports from the last question parallel reports from the individual *Os*. "Meaning, rhyme, and number of syllables left no doubt in my mind that 'Elysian' was the correct word." "I could think only of 'eternal,' but I knew it would not fit. I tried to get a rhyme with 'vision.' " "I finally tried 'Stygian' from the river Styx. I did not like it, but could think of no other." "I took all the letters of the alphabet and tried putting them in front of 'ision'." "I knew it needed an adjective, but I could not think of any that fitted. I did not know the word anyway."

Questions (4th selection)	*Yes*	*No*
1. Did you read 'you' into the blank immediately?	78	22
2. Did you try to rhyme the word with any other line than the one ending in 'through'?	26	74
3. In trying to get the word, did you come over other words with the dipthhong 'oo' sound?	25	75
4. Did you determine the number of syllables required?	47	53
5. Did the meaning suggest the word?	74	26
6. Did you supply the word and test it by the meaning?	17	83
7. Were you sure of the word you supplied?	71	29
8. Was this sureness due to the suitability of the word as regards rhyme and meaning?	72	28
9. Report anything about your solving process that these questions have omitted.		

Ninety-one percent of the *Os* supplied the word 'you' in the blank; 2% supplied other words obviously unsuited to anything except, perhaps, the rhyme; and 7% left it blank.

It is evident from the reports that the *Os* were self-instructed from cues gleaned from the first verse and the questions concerning it (occasion). "My first step was to look for the rhyme-scheme. There were three possibilities, though only two (rhyme with first or second line) were likely. Then I read the verse for the meaning and 'you' came at once. It was unnecessary to count the syllables. The metre was too obvious." "The solution was very simple. After reading the verse, the word 'you' followed immediately to complete the meaning. Then I tested it for metre and found that it fitted."

The majority read the word directly into the blank. "There was no conscious effort to think about what word was necessary. There was no recognition of the rhyme. It merely was the immediate cognizance that 'you' would rhyme and fit without any other qualification." "I tried to supply a word that coincided with the spirit of the lines." "A decided sense of satisfaction prevailed throughout the process because of the felt fitness of the word immediately supplied at the first reading."

The uncertainty attendant upon some who supplied the correct word was explained as follows: "The word 'you' did not seem to fit my idea of the sense

I would like the verse to have, so I was not sure of it." " 'No worse than you' sounds quite sarcastic. The word was evidently 'you.' However, I could not see the meaning when used with 'worse'."

The procedure here consisted in an apprehension of the meaning of the verses accompanied by search for the word most suitable to the rhyme and meaning or to the stanza as a whole. Individual variations occurred in the bases given for supplying the word Some, sought for a word to fit the meaning first, others to fit the. rhyme while still others supplied it from both rhyme and meaning, or from a sense of fitness to the whole. Lack of an apprehension of the poetic form was an inhibiting factor. To some the occasion was a poetic construction; to others, a non-aesthetic verbal exercise. A wide range of occasional instructions appeared.

INTERPRETATIONS AND CONCLUSIONS

Throughout the course of the experiments we have constantly looked for 'insight' under its various alleged forms. In the first three problems, which encouraged 'insightful' discernment and recognition of available materials (beads and cork-in-bottle), we turned up various forms of the apprehensive functions operating to recognize tools and instruments in an unusual setting. There occurred two approaches toward solution, one involving the perceived applicability of the available instruments to the occasion offered, and the other the applicability of an instrument after reaching an imaginative solution of the problem set by the formal instruction. The first approach involved search together with perceptive apprehension, both extended by inspection-and-comment. All these psychological functions were induced and guided by occasional and self-imposed instructions as well as by a rehearsal of the problem as formally proposed. In those instances (both individual *O*s and group *O*s) where search led to an imaginative apprehension which devised promising instrumental means of solution, the actual application of these means was either itself imaginational (as in the non-executing groups) or actional (as when the individual, having once conjured up means, turned to the table and demonstrated his solution). In both cases, the protocols instanced under Problems 1, 2 and 3 will reveal the very large part usually played by verbal comment in *O*'s progress toward the solution. Just what takes the place of this comment where children, apes, and rats are wanting in linguistic resources is too often covered up by the ambiguous term 'insight.'

These first three problems also reveal some special procedures which are likewise often disposed of in the same way. We may cite the *O* who, after handling the beads of Problem 1, laid them down, leaned back, thus bringing the pliers within his range of vision, and apprehended them as means of solution. If there was 'insight' here, it was obviously an extension of the apprehensive functions *plus* inspection-and-comment. We may approach more closely the actual organic resources of solution in such a case when we observe that the enrichment of the apprehension was obviously due to the preceding search and comment, under appropriate instructions of all our three kinds, formal, self, and occasional. Having determined these essential conditions, the addition of an alleged act of 'insight' would seem to be superfluous.

In Problem 4 were the mutilated and displaced Rs, with a gradual approach from nonsense-forms to the standard letter. The formal instruction to identify each form as it came tended toward configuration, taken in the non-technical sense. The initial perception together with this instruction set up a puzzled search which led first to a family likeness among the forms as they came along, secondly to the perception of figures, animals, and the like, and finally (usually at the seventh and last member) to the symbol R. Thereupon, in retrospect, all the members had become (imperfect) Rs. The successive appearances (which were at times 'sudden') might be called 'insights'; but, more descriptively, perceptive changes under formal and occasional instructions and a puzzling search, the search gradually becoming informed and directed as the series progressed. One moment in the occasion (the prone letter) tended to block understanding (the symbol R) until the letter was practically complete in linear form. The occasional flashing out of the R would be called by some an insight; but with as little justification as the earlier perceptions.

Problem 5 called for the tracing of the rectangle and inscribed figure, with and without a central diagonal (see Fig. 2). *E* had meant to invent a problem in which trial-and-error should be supplemented by something which might be called 'insight.' As a matter of fact, the combination of occasional and formal instruction led chiefly to inspection-and-comment instead of to a headlong tracing of the lines with the red pencil. Our *O*s proved to be neither Thorndikean cats nor *Köhlersche Affen.* Upon comprehending the instructions and perceiving the figure, they set about, under inspection-and-comment, to solve the problem. Imaginational apprehension played its part and so did search and various simple forms of action. At times a failure to solve led to a new direction of search and to a new plan. As the tracing (actual or imagined lines) proceeded, the occasion was modified, and in turn modified—by its new instructions to the organism—further procedures. This may be called trial-and-error; but it is certainly not the blundering and blind 'mechanics' of the older maze-experiments. The addition of the diagonal made the task more difficult and called out more verbal comment and self-instruction; but the general procedure was the same both with individual *O*s and in the groups. The added line decidedly changed the occasion which faced the organism and made new demands upon resources and functional expedients. Where there is an occasional 'aha!' in the protocols, it may be set down among many comments upon the course and terminus of a search. It should be noted that the 'aha!' and the 'flash' (dear to the champions of *closure*) were no more in evidence at the final solution than at various steps along the way. Here that form of the behaviorist's fallacy which interprets performance out of the experimenter's own thoughts and fancies is obviously to be discouraged.

In Problem 6 were the pill-boxes and the hidden cotton. The intent of the arrangement was to induce a situation where many possibilities offered. This intent was realized. *O* devised a plan when the boxes were presented and the formal instruction read. Upon the formulation of the plan ("I shall begin from the left," "I'll try even numbers," "I must find hidden markings," and the like), the search changed from an indefinite thrust "to solve" to "this is my way of hunting." Failure to find led to comment, to imaginative forecast, and to a new plan. As the cotton was found and replaced, the specification of method made memorial use of past success. The occasion changed, the self-instruction changed, inspection-and-comment grew; but the search went on. It is a good instance in little of scientific observation and the gradual elaboration and correction of a theory. Our commentaries throw much light upon this complex functional procedure. While many functional resources of the organism were called forth by the problem, the most notable and outstanding was comprehension, in the sense of understanding or grasping-the-significance-of. Surrogative means doubtless exist in various animals for problem-solving where plural choices offer without perceptive cues, but our commentaries make it abundantly evident that comprehension was the central resource in all our human *O*s.

The three-dimensional construction with the matches was called for in Problem 7 and the spelling of "perplexity" in Problem 8. Both exercises are of the 'puzzling' kind. Both involve a certain spatial arrangement where something bars the way to solution. In the first it is the lay-out on the plane surface: in the second, it is the ordinary left-to-right set-up of letters in a word. Under this handicap to success, search and inspection-and-comment go on. The outcome is a manual construction. Where *O* succeeded (there were many failures), some chance change in the situation led to the match-solution, and the discovery that the initial "P" (in "perplexity") could not be moved from the right end of the slot led to the word-solution. Free trials were abundant in these puzzles; but *free trials under comment,* which tended to advance the problem toward solution. This is rather 'trial-and-advance' than 'trial-and-error.' There is nothing new here to suggest a unique operation of 'insight.' It is informing to observe how easily wont, habit, or the accustomed prevents those functional accomplishments which would lead on to invention and discovery. It appears that man's functional resources are much greater than his actual accomplishments.

In Problem 9 again (the burlesqued soliloquies) the organism was misled so far as verbal understanding was concerned. Understanding there was, from the outset, but of individual verbal symbols which did not integrate into a linguistic and aesthetic whole. If the ultimate emergence of such a whole, in a strange context, demands 'insight,' then our *O*s showed that they possessed that gift. Flashes and illuminations were absent, however; and our theories did not lead us toward 'closures.' What little clear perception there was was chiefly buried under a constantly shifted comprehension which first integrated small groups of words, then larger, and finally (in many cases), the entire Shakespearean text. Underlying these progressive comprehensions were self-instructions constantly warring with the occasional instructions and fed by a search which changed its direction when inspection-and-comment brought the hint 'this is a significant passage.' It is a fine case of two conflicting comprehensive functions, one sustained by visual receptors and the other by auditory. The visual is at first dominant; but the auditory gradually gains the ascendency by virtue of formal and self-instructions which promote and direct search. The partial successes and the failures (in *E*'s sense!) are quite as instructive as the reports of those *O*s who read in the soliloquies. Here the groups were very informing. They reveal the organism working away under the lead of the various instructions and using one device after another. Especially do they show the effectiveness of the function *search*, a describable operation distantly related to the mechanician's 'drive.'

Problem 10 presented the flask-balloon, and Problem 11 the centrifuged shot in the rocker. Both suggested the initial comment 'puzzle to work!' which set off a fairly simple action-train (inserting the balloon, tipping and rocking the shot). Failure to carry out the formal instruction usually led to long inspection-and-comment with interspersed actions. The special resource demanded by the occasion and the formal instruction was a formulated principle or 'natural law' (air pressure, 'centrifugal force') and the comment 'This is a case of . . .' This is commonly called 'the applying of physical knowledge.' 'Knowledge' in this situation involves the *inclusion* in comprehension of a generalized rule and the *comment* that the rule applies here. The mere grasp of the principle is not enough. Understanding must fit it to the occasion. Occasional and formal instruction are not sufficient for a solution. Our commentaries and our group-questionaries show in a very striking way the functional modes involved in the solution, wherever the solution actually took place. These problems are probably more difficult for our *O*s than any preceding, in the sense that more functional devices and resources of the organism, combined in more subtle combinations, were called out. We might ignore the whole procedure and simply label the means 'insight'; but that would be to give up the central psychological problem. It is obvious that our problems of this sort only touch the surface of these important human procedures, which call loudly for searching experimental study.

In our last two problems, Problem 12 (discovering and completing a number-pattern) and Problem 13 (completing a poetic stanza), the presented occasion *contains within itself* the means for completion (*i.e.* omitted final number, omitted final word). It contains it, however, only when the occasion is *for the solving observer* a number-pattern (first problem) or a poetic structure (second problem). Here appreciation of pattern or form takes the place of outside physical principle and application in the two problems just now reviewed (10 and 11). Our commentaries and the replies to the group-questions throw a good deal of light upon the means (occasional and self-instructions, search, comment, understanding and imaginational apprehension) which supply—and also fail to supply—the required solution. Search (for a pattern) and occasional instruction leading to comment were the chief means used in Problem 10. It is obvious that the discovery of pattern in the first series helped to define search in the second series and so led toward the goal.

In Problem 13 the temper and training of *O* were important. Without an appreciation of metrical form and of poetic phantasy, the *O*s usually found it impossible to secure the correct final word. Here we come upon the important distinction between occasion-for-the-observer and the 'stimulus' as set up or assumed by the experimenter. It is only the former, that-which-is-present-for-the-organism, that is of primary significance to the psychologist. Where the *O* poetically understood, the formal instruction set a definite search and led (through the intervention of self and occasional instruction, inspection-and-comment, and imaginational apprehension) to the missing word. A limited vocabulary sometimes defeated the search. Our group-results are of especial importance in this problem because they have helped us to discover by inquisition the relative uses of rhyme, metre, rhythm, vocabulary, poetic appreciation, and other factors, in attempting to solve the problem. Less than one-in-three of the group-members found the missing word 'Elysian' under the restrictions imposed. The fourth selection profited (91% of the group *O*s supplied 'you') from the earlier solutions, and it was also simpler and less dependent upon the appreciative abilities of the individual. Again, this experimental fragment only touches the surface of a large problem, the discovery of the psychological means resorted to by the adult and literate organism in selecting and verifying 'the suitable' in a poetic sense.

We began by listing various ways and contexts in which the term 'insight' has been recently used. Were we to review our own commentaries and the answers to our group-inquiries, we could easily find instances of the following sorts to which we might apply the word without very greatly extending it beyond its present uses. The instances are: (1) perceptive apprehension extended on the side of use and means, (2) perceptive apprehension accompanied by imaginative revaluation, (3) anticipative imagination, (4) comprehension of a solving generalization, (5) sudden drop in learning-time, (6) re-apprehension of experimental material under occasional instruction,

(7) comprehension of a general principle and its applicability in a specific instance, (8) apprehension of a total pattern-arrangement or of particular relevant features in the pattern, and (9) comprehension of a constructive scheme, of the rules or canons of an art, or of the natural relation between presented objects.

The fact is that the further experimental description and functional analysis go in the directions taken, the looser, the more ambiguous, and the less satisfactory the term 'insight' becomes. Its recent revival in psychology has been natural if not inevitable—as we saw in the beginning of our study—; but there would seem to be no longer any sanction for its varied and uncritical connotations. Under arbitrary definition it may serve for some time as a label for certain behaviors in rat-running, primate-manipulation, and school-accomplishment. As a cause and explanation of these behaviors, insight will probably be less and less appealed to in factual and experimental contexts.

Our own procedure in the problematical situation used has been (1) to present a definite and solvable problem, (2) to provide a brief, unambiguous, and constant formal instruction, (3) to employ trained and untrained observers, individually and in groups, (4) to determine by report and interrogatory[5] a descriptive account of the psychological functions brought into play, together with the instructions (formal, occasional and self) and other antecedents which threw the functions into commission.

The functions most frequently and effectively found in our solutions have been search, the three forms of apprehension (perceptive, memorial and imaginational), inspection-and-comment, and comprehension. In the examination of our descriptive material we have found no characteristic process, operation, form of conditioning or mode of discovery, which we could with propriety distinguish as 'insight.'

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[5] The groups were always formed and interrogated after a given problem had been studied through the elaborate reports of trained individual observers. Only in this order could the groups be sensibly and usefully questioned, and only so could the individual and the grouped results be reasonably compared. We found this form of group-observation extremely informing.

FREE WORD ASSOCIATION AND SEX DIFFERENCE

By H. G. WYATT, Stanford University

A familiar disadvantage of the free word association exercise for disclosing personality traits is the time and trouble involved in manipulating the data. The question to be considered here is whether a free word association exercise of the ordinary kind, in which the stimulus-word is exposed and the subject records the first associate that comes to mind, can yield equally valuable and significant results from an abridged as from an elaborate procedure. But opportunity will also be taken to summarize the sex differences in 'interest' which the association exercises used in this inquiry disclosed.

METHOD AND PROCEDURE

Subjects and materials. To attempt an answer to the main question the contents of two parallel free association exercises, each containing 110 stimulus-words, were scrutinized. The first exercise (Exercise A) was given to 452 *Ss*, 75 of each sex being obtained from the seventh grade, 87 from high school, 26 from college, and 35 from business. The second exercise (Exercise B) was given to other groups similarly derived and to 510 *Ss*, in numbers respectively 80, 87, 53 and 35 of each sex.

The two lists of stimulus-words are given below in the order in which they were given to the *Ss*.

EXERCISE A

Chair
Green
Wind
Carrot
Letter
True
Cellar
Body
Despise
Ill
Face
Spoon
Twilight
Bashful
Gun
Cheek
Taste
Escape
Proper

Sixteen
Nasty
Social
Bare
Knight
Sneak
Strong
Memory
Mate
Fight
Birth
Hunt
Earn
Hug
Stage
Policeman
Leg
Angel
Fun

Mother
Play
Toad
Thin
Paint
Lover
Darkness
Art
Electricity
Save
Kill
Youth
Kiss
Soap
Slender
Desire
Rooster
Cry
Skin

EXERCISE B

Table
Blue
Anger
Breast
Rain
Turnip
Promise
Gentle
Book
Pure
Fame
Boss
Kitchen
Flesh
Tomboy
Give
Admire
Pink
Faithful

Wedding
Evil
Artist
Necklace
Color
Rodeo
Safety
Religion
Handsome
Battle
Maiden
Virtue
Want
Embrace
Wash
Shoulder
Closet
Ankle
Wish

Skirt
Manners
Filthy
Society
Propose
Ring
Naked
Castle
Build
Mouse
Cheat
Athlete
Love
Foot
Feeling
Fellow
Handkerchief
Bold
War

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Marriage
Diamond
Pink
Worship
Lovely
Honor
Longing
Throat
Secret
Sacrifice
Male
Party
Coarse
Brave
Flower
Rule
Family
Limb
Vain
Bargain
Curl
Fear
Mouth
Dress
Curse
Hips
Success
Tender
Ambition
Mistress
Sissy
Gift
Immoral
Machine
Shame
Figure
Leader
Ideal
Submit
Lily
Charm
Clothing
Rude
Engagement
Bells
Make
Passion
Arm
Garter
Dare
Beloved
Thrill
Automobile
Hair
Flirt
Blush
Form
Moon
Modest
Child
Bull
Hunt
Fresh
Snake
Stout
Fashion
Danger
Powder
Sweetheart
Decent
Duty
Female
Bell
Weep
Picnic
Delicate
Waist
Hero
Fair
Garden
Physical
Serve
Power
Home
Rose
Magic
Hand
Thirteen
Baby
Darling
Scream
Pain
Money
Squeeze
Clothes
Actor
Protect
Knee
Devil
Enjoy
Jealous
Shop
Dimple
Afraid
Lips

The *S*s were directed to write down 'the first word' the word shown 'makes you think of,' and the stimulus-words were exposed at 10-sec. interval.

Procedure. Since the main object of the study was to determine the most convenient and economical method of handling the data, the method of extracting sex differences was purposely varied with different groups of *S*s. First, the associations of the business group of *S*s taking Exercise A were subjected to a somewhat meticulous analysis; and then the responses of the other groups in that series (the grade, high school, and college groups) were examined afterwards, but on a slightly different plan. Then the associations of Exercise B were separately analyzed for the different groups. We shall refer to those as Analysis A′, Analysis A, and Analysis B, respectively.

Analysis A′. The responses of the 70 *S*s of the business group were treated as follows. The 110 stimulus-words were divided into five groups of which Group I contained words referring to the body and its parts, *e.g.* body, face, cheek, throat, limb, leg, curl, mouth, hips, figure, skin, arm; Group II, to sex, *e.g.* bare, beloved, birth, desire, hug, immoral, kiss, lover, longing, marriage, male, mate, mistress, passion, spoon, thrill, vain; Group III, to plants and animals, other than man, *e.g.* carrot, flower, lily, worm, rooster, toad;. Group IV, to emotionally exciting words (not already classified above) *e.g.* bashful, cry, curse, fear, ill, kill, secret, shame, flight, escape, darkness; and Group V contained the remaining words, classified as miscellaneous.

It should be noted that the basis of classification was psychological, not logical—for instance, the reason for including 'spoon,' 'immoral,' and 'desire' with sex words was that the words seem provocative of thought about sex, not that their dictionary meanings are primarily sexual. Similarly the word 'ill' refers primarily or commonly to a bodily condition, but was included as emotional because illness commonly influences markedly one's emotional condition, and the thought of illness is likely therefore to have marked emotional associations. Of course a precise psychological classification is impossible—no one can be sure of striking the average mental effect of any word, and the effect varies with the individual affected. On the other hand one may easily exaggerate this difficulty —the reader will probably agree that the word groups above do constitute sufficiently distinct psychological classes to justify one in attributing any marked differences between them in the kind or distribution of associates aroused to the class character.

For the purpose of this analysis single-frequency associates, *i.e.* words associated by one *S* only of the sex in question with the particular stimulus-word, were first set aside; and then the rest of the associates, *i.e.* the multiple-frequency associates, were arranged, for each in turn of the 5 groups of stimulus-words, in order of decreasing frequency of response. Thus a frequency or popularity order of associates was obtained for each sex with every stimulus-group.

The single-frequency words were also ranked for each sex in order of decreasing frequency. An associate single to a given stimulus-word with one sex might of course often occur with that sex in response to other stimulus-words, and be multiple with the other sex.

A small minority of the responses, those in which the sexes most contrasted, were studied in their association-context, that is, in relation both to the stimulus-words to which they were responses, and to 'competing' associates with the same stimulus.

In this way sex differences were elicited from the 5 different classes of stimulus-words; and, with each of these 5, for 3 different classes of responses: multiple-frequency, single-frequency, and sex-contrasting responses. Intercomparisons of the results would, it was hoped, suggest whether and under what conditions a proportion of the responses—the single-frequency, or even all but the sex-contrasting responses—might be safely ignored in deriving significant sex differences, thus preserving efficiency while saving labor.

Analysis A. For the next analysis the records, under each of 110 stimulus-words, of the words associated and the numbers of male (M) and female (F) *Ss* recording each associate were arranged in separate columns for the grade, high school, and college *Ss*.

The stimulus-words were divided into 5 groups as for the previous analysis. Every group was then searched successively for associates of 13 psychological classes, which had been suggested from evidence obtained elsewhere:[1] viz. words referring to (1) body parts; (2) dress; (3) personal adornment; (4) persons or people; (5) aesthetic attributes or appraisal; (6) colors; (7) activity and adventure; (8) outdoor phenomena; (9) social activity; (10) religious observance or literature; (11) highly emotional states or exciting objects; (12) economic activities; and (13) foods, culinary operations and occupations.

In determining the class of an associate it was necessary to have in mind a fairly, but not too, rigid definition of the class concerned, but there were usually doubtful cases—more so with a class like 'social' and 'emotional' than for a concrete class like 'dress' or 'colors.' Is 'complexion' psychologically a part of the body, or 'evil' a religious word, or is 'fish' to be considered a food? The same word may vary its class with the context (especially with the stimulus-word) and with the individual *S*, and the examiner may also vary on occasion in the strictness of his censorship. Examples of each class are given in Table I, to which may be added the following comments on some less self-explanatory classes.

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[1] From an investigation of sex differences conducted by Professors L. M. Terman and C. C. Miles about to be published.

Two classes crossed but did not coincide; namely, the 'activity and adventure' and the 'outdoor' classes, *e.g.* 'gate post' and 'tree' are confined to the latter, 'murder, and 'fire' to the former. The 'activity and adventure' class included names of heroes and adventurous types (robber, knight, King Richard), verbs indicating physical activity or prowess (jump, hike), names of articles used adventurously (gun, aeroplane, but not automobile), dangerous conditions, daring occupations, and travel (rodeo, ship, but not hotel).

The 'social' class comprised words implying fellowship or voluntary social mixing (friend, good fun, society, but church is marginal).

The class 'religious' comprised words characteristic of sacred literature (Abraham, angels) words of religious as distinct from ethical connotation (evil, sin, holy, but not virtue, vice, or good), words for religious institutions (God, devil, Easter), and words of religious ceremony or practice (altar, hymns).

In the 'emotional' class fall words which signify emotions, or states or objects which commonly excite emotions or feelings of pleasure or unpleasure, as distinct from mere physical pain, (lonely, dearest, desire, but not wish as it is too general and weak).

Among the 'food' nouns several words (vegetable, rabbit, turkey, eggs) proved marginal cases where outdoor or animal life interests might seem to come in.

The numbers of each associate for each sex for each educational stage (grade, high school, college) were collected, and the results were tabulated separately for multiple-associates and contrasting associates, single-associates as already defined being omitted from this analysis.

It was thus possible to examine with the larger group, as with the 70 adults in the previous analysis, the relevance of different classes of stimulus-words to different classes of sex difference, and in this case in addition the relation of age or educational stage to sex differences; and to check how far with differently sized groups one might safely disregard lower multiple frequencies.

Analysis B. With the third group of 510 *S*s (255 of each sex) the stimulus-words of Exercise B were used. The procedure was slightly amplified. As regards the classification of responses 'personal' associates were divided into 3 subclasses, for names of females, males, and persons of either sex indifferently (neutral); while to the 5 classes of stimulus-words was added a sixth, 'names of persons'; and the miscellaneous or unclassified residue (55 stimulus-words) were subdivided according as they were nouns, verbs, or adjectives. There were thus 8 instead of 5 classes of stimulus-words: 5 psychological and 3 grammatical. It should be noted as regards sex-contrasting responses that in the other analyses a response was considered sex-contrasting when the response frequencies of the two sexes differed by 3 or more. In the present analysis this basis was retained for the two smaller groups of 52 and 70 *S*s respectively, but for the grade group of 160 a sex divergence score of 4 upwards, and for the high school group of 174 of 5 upwards was required to constitute sex contrast.

RESULTS

Sex trends disclosed. Before proceeding to our main inquiry we summarize the sex differences in association trends gathered from the results of the three analyses. In Table I, which follows, Exercise A was given to 35 business adults of each sex (Analysis A′), and to 191 academic *Ss* of each sex (Analysis A), and Exercise B to 255 *Ss*, business and academic, of each sex (Analysis B).

TABLE I

SHOWING SEX PREFERENCE OF DIFFERENT GROUPS FOR VARIOUS CLASSES OF ASSOCIATE WORDS

Associate class	Analysis	7th Grade	High School	College	Business*	Examples
(1) Body parts	A	N	F	M	FF	neck, limb, nerve, hand, teeth, hair, face, body, flesh.
	B	F	F	F	M	
(2) Dress	A	F	N	M	FF	garter, corset, hose, suit, hat, stocking, shoe, coat.
	B	N(M)	N	N	N	
(3) Personal adornment	A	N	N	N	FF	rouge, marcel, jewelry, diamond, ring, locket, gems.
	B	N	N(F)	N	N(F)	
(4) Persons or people	A	M	M	F	N	soldier, actor, hero, God, hunter, silly kid, lady, artist, him.
	B	M	M	N	N(M)	
(a) female	B	MM	M	M	M	girl, woman, sister, Mary.
(b) male	B	M	M	F	N	boy, man, father, husband.
(c) neuter	B	N	M	N(F)	F	people, children, baby, friend, teacher.
(5) Aesthetic	A	FF	F	N	N	pretty, beauty, graceful, piano, picture, music, art, painting.
	B	F	F	N(F)	F	
(6) Color	A	FF	F	N(M)	N(F)	red, white, golden, rosy, color, black.
	B	FF	FF	F	N(M)	
(7) Activity and adventure	A	MM	MM	N	M	dive, war, kill, hunt, travel, rodeo, robbers, knight.
	B	N	M	M	M	
(8) Out-door	A	M	M	M	M	auto, snake, snow, deer, grass, coach, war, ride, woods.
	B	M	M	N(M)	M	
(9) Social	A	N	N(F)	N(F)	N(M)	friend, companion, party, picnic, dances, play, fun.
	B	N	N(M)	N	N	
(10) Religious	A	N	N(M)	N	N	Jesus, God, church, devil, saint, holy, Bible, righteous.
	B	N	N	N	N(M)	
(11) Emotional	A	FF	FF	M	N	ecstasy, good time, admire, happiness, fear, anger, longing.
	B	N(F)	F	N(F)	N(M)	
(12) Economic	A	M	M	N	N	money, cheap, work, counter, wages, spend, dollars, earn, sale.
	B	N	N	N	N	
(13) Foods	A	M	MM	F	N	eats, coffee, soup, chicken, ice cream, sugar.
	B	M	MM	N	N	

*Analysis A′ for this group.

The letters 'M' and 'MM' or 'F' and 'FF' signify distinctive, and markedly distinctive, male or female tendencies. Parentheses signify that the predominance is probably non-significant, and 'N' that there is no distinctive sex tendency.

The differences in sex preferences for different classes of associate words are summarized in Table II. This summary shows that though

TABLE II

Summarising Sex Preference of the Eight Groups for Different Classes of Associate Words

	Associate Class	Sex Trends				
		FF	F	MM	M	N
(1)	Body parts	1	4	0	2	1
(2)	Dress	1	1	0	1	5
(3)	Personal adornment	1	0	0	0	7
(4)	Persons or people	0	1	0	4	3
(a)	females	0	0	1	3	0
(b)	males	0	1	0	2	3
(5)	Aesthetic	1	4	0	0	3
(6)	Color	3	2	0	0	3
(7)	Activity and adventure	0	0	2	4	2
(8)	Outdoor	0	0	0	7	1
(9)	Social	0	0	0	0	8
(10)	Religious	0	0	0	0	8
(11)	Emotional	2	1	0	1	4
(12)	Economic	0	0	0	2	6
(13)	Foods	0	1	2	2	3

there is no class of associate or response words scoring the same positive sex trend in every group of *S*s for both the parallel exercises, that is, scoring eight like positive signs, in classes 3, 5, 6, 7, 8, 9, 10, 12, and in 4a, the tendency is either neutral or distinctive of one sex only, of the female sex in classes 5 and 6, less emphatically in 3; of the male sex in classes 7, 8, and 4a and less emphatically in class 12 in which the 'economics' trend is too slight to be conclusive or decisive, neutral indices predominating here, as also in classes 3, 9, and 10.

The table shows the six remaining classes 1, 2, 4, 4b, 11, and 13, ambiguous—sex trends varying according to the group or exercise. But the evidence of the very small Analysis A college group (26 *S*s of each sex) conflicted with that from the college group twice its size in 7 out of 13 cases, and was in other respects singular. For example it scored the only M for 'dress' associations, the only F for 'personal' and 'food 'associations, and the only clear M for 'emotional' word associations. It seems fair to conclude that the group through small size or otherwise was unrepresentative; and we may take the bulk of the evidence here as indicating a female preference for dress, and parts of the body, and emotional associates.

Personal associates score M in 4 groups, F in 1, N in 3. But in a subsequent subdivision (in Analysis B) into words for males, females, and of either sex indifferently, it appeared that associating females is a distinctive male tendency in every group, there being no uniform male preference for the other two classes of persons; nor have females a general preference for names of males.

Moreover, in connection with Analysis A' the personal associates with 26 sex stimulus-words, if further subdivided, show as follows: number of males recording feminine nouns, 73, masculine nouns, 24; number of females recording feminine nouns 22, masculine nouns 42.

This means a very marked male preference for feminine nouns and a less marked female preference for masculine nouns; and suggests that thinking of the 'opposite' sex is stronger in males than in females, and that not only at the school stage. It also illustrates the risk of accepting sex agreements or differences for large classes of objects in general without further analysis. An aggregate male score for personal names may conceal a special tendency to think about girls or women as objects of distinctive sex attraction.[1]

The same need of deeper analysis, if we are to get at the effective sex motives, is illustrated by the 'food' word associates. These score M or MM in 4 groups, F in 1, N in 3. But, as our discussion of sex differences in relation to stage will presently suggest, this need not indicate any particularly general distinctive male interest in food.

Distinctive female associate preferences appear most marked for aesthetic and color words, less marked for words for parts of the body, for articles of personal adornment, and emotional words; males distinctively associate outdoor words and words of activity and adventure, names of females, and perhaps (but see below) words for foods. Over economic, social, and religious words there is no very noticeable sex distinction, nor, in curious contrast with other evidence, for words for articles of dress.

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[1] Further segregation showed that aunts, grandmothers, and sisters were not associated more often by males than brothers, uncles, and grandfathers by females, or by males either.

Associates in terms of politics, history, geography, science, and mechanical or manual occupations, occurred too rarely to warrant classification or generalization.

Correspondence in results from Exercises A and B. Table I shows that out of 13 classes of associates or thought-referents for which the sex scores in the two parallel series A and B are tabulated, for each of 4 groups of *Ss*, yielding 52 cases for comparison, in three cases the sex trends of the two series are opposed, in 36 the same, and in 13 definite in one series and neutral in the other. In the 26 cases where the two larger groups are concerned (7th grade with 78 and high school with 87 *Ss* of each sex) there are no opposite, and there are 22 similar, trends and 4 partially discrepant (neutral versus definite).

In the 26 smaller group cases (college groups with 26 and 53 *Ss* and a business group with 35 *Ss* of each sex) there are three opposite and 14 similar trends and 9 partially discrepant. Of these smaller groups the 26/26[3] college group, and perhaps the 35/35 business group in series 2, seem to be least representative, if consonance with a majority trend of the groups in its series be any criterion. The smaller college group trend contrasts with the majority trend in its series in 6 cases, but the larger 53/53 college group in none. Decrease of reliability with decrease in size of group is what we should expect. The evidence of college group B is thus probably the more trustworthy for adults.

Sex differences in relation to stage and age. The doubtful trustworthiness of the smaller groups of older *Ss* makes stagewise comparisons where they are concerned precarious. Taking the scores as they stand the extent of correspondence between stages is summarized in Tables III and IV.

Age and stage preferences disclosed. Summarily we may say that the two school groups concur in their sex trends on the one hand, and the two adult groups on the other, more closely than do adult with school *Ss* (Table IV); that sex differences in religious and social associates are negligible throughout (Table I); there is a rather general distinctive male interest in female persons but not in personal associates as a whole; and there is a rather general female preference for aesthetic associates (Table I).

As regards particular agreements and disagreements the following points may be made: (1) A female tendency towards aesthetic expression seems to characterize the youngest stage, and towards emotional associates and color words the school stage, in general (Table II). (2) A male preference for male person associates, which distinguishes the grade from the college groups, and a more marked male preference at the grade, but especially at the high or adolescent stage, for words of activity and adventure (Table II) is perhaps attributable to the youthful male's heroic and 'he-man' predilections.

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[3] The first number is of male *Ss*, the second of female.

TABLE III

SHOWING AGREEMENT AND DISAGREEMENT BETWEEN GROUPS IN SEX TREND

Group		7th Grade	High School	College
High School	Concurrence	25		
	Opposition	0		
	One N, the other M or F	4		
College	Concurrence	13	13	
	Opposition	5	5	
	One N, the other M or F	11	11	
Business	Concurrence	13	14	16
	Opposition	1	4	3
	One N, the other M or F	15	11	10

The difference among the various groups, in both exercises, is shown in Table IV.

TABLE IV

SHOWING ASSOCIATE-WORD CLASSES IN WHICH GROUPS DISAGREE IN BOTH EXERCISES

Groups	Associative classes differing
Two School Groups	No cases
Grade and College	Persons, Aesthetic, Activity, Foods
High School and College	Persons, Aesthetic
Grade and Business	Persons, Body Parts
High School and Business	Persons, Colors, Emotional, Foods
College and Business	No cases

It is noticeable that the business groups score more markedly male than the college (the more intellectually interested?) groups in these respects (Table II). (3) The business group females prefer adornment and dress associates more distinctively than the college or school females. (4) A strong male preference appears for 'food' words at the school, especially the high school, stage, in contrast with the adult female or neutral tendency. The fact, however, that 'wine' scores 21 M to 1 F in association with 'cellar' in the high school group, and that many food words are also words for outdoor plants and animals suggests that further analysis of a wider range of content is required before we can infer a distinctive male interest in foods as such. (5) Sex divergences appear more pronounced at the school stage than later. Out of 58 indices 21 are N or neutral for the school groups and 35 for the adult groups, while in 10 cases the distinctive tendency is markedly pronounced in the school scores, and in only 4 cases in the adult groups. Average sex divergence scores based on the associations considered in our analysis of exercise A were approximately 4.2, 3.4 and 2.8 for grade, high school and college groups respectively. We will now come to the main object of this study.

Longer and shorter procedures compared. (*1*) *Analysis A'*. Table V below indicates numerically for the business group the extent of correspondence in scores and signs of 4 different collections of associates, namely, single, multiple, total, and contrasting associates.

Contrasting cases cover all associates in which the difference between the sexes in score is 3 or more.

According to this table, the multiple associates show the same sex predominant as the total associates in every one of 13 classes of associates, but the proportion of predominance differs a little in some cases. In other words, disregard of the single associates would not have altered the sex character in any case, but would, (though probably within chance limits) have reduced the differences in some cases. On the other hand, the single associates scores do actually show a different sex predominant (however slightly) from that indicated by the total scores in the case of personal, aesthetic, color, social, emotional, economic and food associates.

Comparing the multiple scores with those of contrasting cases we find the same sex predominant in 11 out of 13 cases, and, in 1 of the remaining 2 (personal) 'contrasting' cases show a slight male against a 'multiple' slight female predominance (111/100 to 265/278) yielding an N sign in both cases, and in the other (colors) there is a 'contrasting' N (13/13) against a 'multiple' F 35/59. But in this case the 'contrasting' numbers are too small to signify. In other words, where the contrasting case numbers are at all considerable, and in most other cases, there is agreement or near agreement in the sex character; but we cannot rely on this agreement obtaining where the score for an associate is low.

The 'contrasting' cases tend to accentuate sex divergences—in 7 cases they have double signs against 3 for the 'multiple' associates,

TABLE V

Analysis A′: Correspondence of Higher with Lower Frequency Results

	Associate class	Score and sex trend for			
		Multiple associates	Single associates	Total associates	Contrasting cases
(1)	Body Parts*	66/142 = FF	36/46 = FF	102/188 = FF	25/45 = FF
(2)	Dress	28/56 = FF	21/40 = FF	49/96 = FF	12/21 = FF†
(3)	Adornment	24/58 = FF	13/29 = FF†	37/87 = FF	23/51 = FF
(4)	Personal	265/278 = N(F)	190/189 = N	416/437 = N(F)	111/100 = N(M)
(5)	Aesthetic	55/74 = N(F)	52/47 = N(M)	105/110 = N(F)	7/32 = FF†
(6)	Colors	36/59 = F	19/16 = N(M)†	55/75 = N(F)	13/13 = N†
(7)	Act. & Adven.	83/51 = M	53/53 = N	136/104 = M	35/11 = MM†
(8)	Outdoor	196/167 = M	73/68 = N(M)	269/235 = M	74/59 = M
(9)	Social	46/36 = M	29/31 = N(F)	75/67 = N(M)	23/21 = N†
(10)	Religious	66/58 = N(M)	28/20 = M†	90/80 = N(M)	11/4 = M†
(11)	Emotional	156/176 = N(F)	117/112 = N(M)	277/288 = N(F)	38/66 = FF
(12)	Economic	64/80 = N(F)	34/28 = N(M)	98/108 = N(F)	15/24 = F†
(13)	Food	84/63 = N(M)	26/63 = F	110/103 = N(M)	52/36 = MM

*66 words for parts of the body were associated more than once by males, and 142 by females, and 36 once by males, and 46 once by females; and 25 words for parts of the body were associated by males on more than three occasions, and 45 similarly by females.

†Too few to signify.

ANALYSIS A: STATEMENT OF SEX TRENDS IN SCORE WITH CORRESPONDING INDICES FOR 'MULTIPLE' AND 'CONTRASTING' ASSOCIATES IN 3 ACADEMIC GROUPS

Associate Class	7th Grade Associates		High School Associates	
	Multiple	Contrasting	Multiple	Contrasting
(1) Body Parts	N 425/424	N 234/229	F 559/602	F 363/397
(2) Dress	N(F) 127/138	F 98/127	N(M) 175/160	N 133/137
(3) Adornment	N 82/89	N 14/14	N 109/122	N 87/80
(4) Personal	N(M) 797/736	N(M) 492/444	M 1135/967	M 702/586
(5) Aesthetic	FF 108/180	F 71/122	F 133/162	F 77/96
(6) Colors	FF 108/163	FF 29/76	F 138/156	F 78/106
(7) Act. & Adv.	MM 377/216	MM 219/110	MM 401/298	MM 287/156
(8) Outdoors	N(M) 342/305	N(M) 212/201	N(M) 422/388	N 314/319
(9) Social	N 83/90	N 64/64	F 93/128	F 54/71
(10) Religious	N(F) 106/110	N 74/75	N(M) 126/112	N(M) 54/45
(11) Emotional	F 475/675	F 330/490	F 490/633	F 314/513
(12) Economic	N(M) 209/184	M 162/127	N(M) 202/185	N 147/140
(13) Food	N(M) 201/191	N(M) 130/124	M 231/190	M 200/166

Associate Class	College Associates		Total	
	Multiple	Contrasting	Multiple	Contrasting
(1) Body Parts	N(M) 111/92	MM 64/34	N 1095/1118	N 661/660
(2) Dress	N 29/31	N 17/13	N(F) 313/337	N(F) 237/278
(3) Adornment	N 33/39	N 3/6	N 227/240	N 104/100
(4) Personal	F 241/295	F 122/146	M(N) 2172/2008	M(N) 1316/1176
(5) Aesthetic	N 43/42	N 22/21	F 284/384	F 170/240
(6) Colors	N 32/30	M 20/14	F 278/349	F 127/186
(7) Act. & Adv.	N 84/85	N 41/42	MM 872/599	MM 547/308
(8) Outdoors	M 94/78	M 30/25	M 858/770	N(M) 556/548
(9) Social	F 20/26	M 14/10	F 196/244	N(F) 132/145
(10) Religious	N(M) 42/36	N 28/29	N 274/260	N 156/152
(11) Emotional	N(M) 179/165	N(M) 59/51	F(FF) 1296/1658	F(FF) 697/1075
(12) Economic	N 49/45	N 32/37	M 460/414	M 341/304
(13) Food	F 53/65	F 28/34	M 485/446	M 358/324

the single associates, and the total figures; and only 3 neutrals, against 6, 8, and 8 respectively. The contrasting cases tend to bring into prominence the same sex divergences which the multiple and the total figures manifest. We may also add that a scrutiny of the contrasting cases in their entirety, that is, in relation to stimulus word and fellow associates, is more illuminative of the mental traits behind the sex differences than a study of the statistical differences alone.

TABLE VII

NUMBER OF 'CONTRASTING' AND 'MULTIPLE' ASSOCIATE-WORDS

Associate class	Multiple		Contrasting	
	M	F	M	F
(1) Body Parts	1095	1118	661	600
(2) Dress	313	337	237	278
(3) Adornment	227	240	104	100
(4) Personal	2172	2008	1316	1176
(5) Aesthetic	284	384	170	240
(6) Colors	278	349	127	186
(7) Act. & Adv.	872	599	547	308
(8) Outdoors	858	770	556	548
(9) Social	196	244	132	145
(10) Religious	274	260	156	152
(11) Emotional	1296	1658	697	1075
(12) Economic	460	414	341	304
(13) Foods	485	446	358	324
Total each sex	8900	9027	5203	6556
Total both sexes	17,927		10,659	

We may conclude that for this particular exercise at any rate, and presumably for exercises with similar ratios of stimulus words to subjects, we can safely neglect single associates in our analysis, thus saving labor in the preliminary tabulation. In this exercise single associates accounted, on a rough calculation, for some 36% of the total.

(2) *Analysis A*. We now come to Analysis A, in which single associates were disregarded and 'multiple' compared with 'contrasting' associates for 3 differently sized and aged groups. Table VI is relevant.

Taking all the groups and cases we find that out of 52 comparisons in 46 the multiple and contrasting cases yield the same signs, that is agree in sex difference or in absence of sex difference. With the two larger groups of *Ss* (grade and high school) in all 26 cases there is a close or (in two) fairly close agreement; and either the same sex predominates or the departures from equality are too small to signify.

In the smallest, namely the 26/26 college group are these three cases of difference in signs:

	Multiple	Contrasting
Body parts	111/92 N(M)	64/34 MM
Colors	32/30 N	20/14 M
Social	20/26 F	14/10 M

Here we have either a sex distinction the same but (in one case only) more pronounced, or non-significantly small numbers.

In fine, where the numbers of *S*s, of stimulus-words, and of the associates in any given case, are adequate, a scrutiny of cases of sex divergence in score of 3 or more yields the same sex distinctions as a more comprehensive survey. Specifically, with *S*s exceeding about 70/70, and stimulus-words exceeding about 100, the briefer procedure would appear to suffice. But where the associates of any class fall below 50 in all, we may do well to supplement from more comprehensive data. No doubt exact statistical treatment would produce a more precise criterion of adequacy for different ratios of *S*s to stimulus-words, always presuming that other cicumstances are also sufficiently similar. Table VII suggests the saving of time and trouble that would be thus effected by reducing the number of responses to be considered.

(*3*) *Analysis B.* In Analysis B we come to a second set of stimulus-words (Exercise B) analogous to that for Analyses A′ and A. In this case we compare the evidence from the total scores of the associates considered with that for the scores of contrasting cases for 4 differently sized and aged groups. But whereas in Analysis A by 'contrasting' was uniformly meant a sex divergence in score of 3 or more, in the present analysis 3 is retained for the 2 smaller groups of *S*s (the 53/53 college and the 35/35 business); but the figure is raised to 4 or more for the grade school *S*s (80/80), and to 5 or more for the high school *S*s (87/87), on the assumption that the more *S*s the higher the scores for any associate and therefore the greater the proportion of wider score divergences, so that the safety limit for contrasting cases can be correspondingly raised. Table VIII summarizes the data.

TABLE VIII

Analysis B: Statement of Sex Trends in Scores with Corresponding Indices Attached for All, and for Contrasting, Associates in 3 Academic Groups of Subjects and 1 Business Group

	7th Grade (80/80) +4 up, sex divergence in score		High School (87/87) +5 up, sex divergence in score		College (53/53) +3 up, sex divergence in score	
Associate class	All cases	Contrasting cases	All cases	Contrasting cases	All cases	Contrasting cases
(1) Body Parts	F 487/508	F 246/319	F 542/590	F 332/365	F 254/286	F 168/175
(2) Dress	N(M) 233/218	N(M) 164/140	N 267/268	N 183/161	N 164/167	N 91/97
(3) Adornment	N 91/89	N 55/67	N(F) 83/111	N(F) 42/64	N 82/88	N 29/36
(4) Persons	M 919/757	M 466/372	M 1048/849	M 574/468	N 561/559	N(F) 274/290
(a) female	MM 353/240	MM 212/134	M 442/332	M 336/242	M 297/245	M 163/119
(b) male	M 325/271	M 142/119	M 303/257	N 100/96	F 138/178	F 53/96
(c) neuter	N 243/246	N 112/119	M 304/250	M 168/123	N(F) 125/136	N(F) 58/84
(5) Aesthetic	F 199/276	F 93/156	F 266/325	F 122/164	N(F) 150/171	N(F) 78/90
(6) Colors	FF 194/251	FF 93/146	FF 211/318	FF 151/238	FF 191/251	FF 95/146
(7) Act. & Adv.	N 314/313	N 137/146	N 337/276	M 154/113	M 181/149	N(M) 84/68
(8) Outdoors	M 470/422	M 247/219	M 479/437	N(M) 211/202	N(M) 292/284	N(M) 161/154
(9) Social	N 123/118	N 60/60	N(M) 133/118	N(M) 34/29	N 56/52	N(M) 27/19
(10) Religious	N 88/83	N 0/0	N 121/114	N 30/35	N 61/72	N 45/48
(11) Emotional	N(F) 577/657	F 197/262	F 617/738	F 208/312	N(F) 405/446	N(F) 246/261
(12) Economic	N 140/141	N(F) 64/79	N 165/162	N(F) 68/82	N 89/70	N(M) 48/32
(13) Foods	M 221/161	M 106/66	MM 214/142	MM 100/40	N 119/117	N(F) 34/40

TABLE VIII (Continued)

Analysis B: Statement of Sex Trends in Scores with Corresponding Indices Attached for All, and for Contrasting, Associates in 3 Academic Groups of Subjects and 1 Business Group

	Business (35/35) +3 up sex divergence in score		Total (255/255)	
Associate class	All cases	Contrasting cases	All cases	Contrasting cases
(1) Body Parts	M 155/98	MM 83/42	N 1438/1472	N(F) 829/901
(2) Dress	N 100/97	N 65/59	N 764/750	N(M) 503/457
(3) Adornment	N(F) 37/50	N(F) 19/28	N(F) 293/338	N(F) 145/195
(4) Persons	N(M) 348/322	N(M) 123/101	N(M) 2876/2487	N(M) 1456/1233
(a) female	M 165/122	MM 63/28	M 1257/939	M 764/523
(b) male	N 109/105	N(M) 37/29	N(M) 872/811	N 332/240
(c) neuter	F 74/95	FF 22/43	N(M) 746/727	N 360/369
(5) Aesthetic	F 77/112	N 32/33	F 592/894	F 325/443
(6) Colors	N(M) 118/97	M 29/21	F 714/917	F 368/548
(7) Act. & Adv.	M 92/72	M 19/10	N(M) 924/810	N(M) 394/337
(8) Outdoors	M 166/135	M 66/43	N(M) 1407/1278	N(M) 685/618
(9) Social	N 43/42	MM 14/3	N(M) 355/330	N(M) 135/111
(10) Religious	M 52/38	MM 18/4	N(M) 322/307	N(M) 93/87
(11) Emotional	N(M) 220/192	N(M) 89/80	N(F) 1819/2033	F 740/921
(12) Economic	N 64/58	M 32/23	N(M) 458/431	N 212/216
(13) Foods	N 73/79	N(F) 22/36	M 627/499	M 262/191

Taking all groups and cases we find that out of 64 comparisons in 56 the total and contrasting cases yield the same signs, that is the same noticeable sex divergence or absence of sex divergence in tendency to associate or think of, the class of word or object in question. In the remaining 8 cases an N sign is attached to one of the 2, the total and the contrasting, sets of scores and a definite sign to the other; but in all 8 cases the total and contrasting associates differ not in the sex character of tendency evidenced but just enough in degree to admit our distinguishing one as N and the other as of a definite sex character. Moreover, in 4 of these cases of partial discrepancy the scores for the contrasting associates were precariously low. Wherever the score is adequate the same sex trend is indicated, but occasionally less markedly by one set of associates than by the other.

The results of our analysis suggest strongly that, for an exercise in which the stimulus-words and the *Ss* both exceed 100, for all practical purposes every associate for any given stimulus-word in which the sex scores differ by less than 3 can be safely disregarded; and that where the *Ss* number 160 or more we may raise that limit to 4 and even 5. This applies to the 13 psychological categories or directions of interest which a scrutiny of the associates, and other evidence not considered here, suggest as most likely to differentiate the sexes in the present experiment. But the proviso should be added that where the associates of any category receive less than a certain minimum score (in ordinary cases 50 would seem to serve) a reduction in the contrasting limit would be on the safe side.

Relation of class of stimulus-word to class of associate. That the exercise under survey fails to elicit in adequate numbers certain presumptively important classes of associates has already been noticed: stimulus-words of the kind to elicit these were absent from the series. That the stimulus-word, as one would expect, influences the associations, and stimulus-words of one class predispose to corresponding classes of associates (corresponding is not necessarily similar to the stimulus-word), and what some of these 'correspondent' classes are, our present analyses indicate. Comparison of responses in relation to stimulus-words shows that words for activity and adventure are elicited most from emotional word stimuli, not from sex words or from words for the parts of the body; words for personal adornment from sex not from plant or animal word stimuli; outdoor words from plant and animal stimuli not from emotional; personal names from sex and personal stimuli; and from sex stimuli feminine noun associates result far more often than masculine or neutral nouns; color associates are fairly evenly distributed, but are evoked most especially from plant and animal stimuli; social words issue from emotional and personal stimuli, not from sex, body, or plant and animal—and so on. That these predominances are so pronounced indicates one of the difficulties with exercises of this type: the associations are so far determined by the stimulus-words that little is left to separate the sexes; so that with any given class of stimulus-words community of associations between the sexes far exceeds diversity. Divergence scores are an inconsiderable percentage of the total gross scores. The most marked general sex divergence, for example, in Analysis A is in emotional associates, which total 1296 for males and 1658 for females; that is, a divergence in amount of 13% only on the total. As one works through the gross figures likeness is far more impressive than unlikeness, even with stimulus-words already, as in the series under scrutiny, selected out of others for their sex-differentiating capacity.

CONCLUSIONS

Having now considered the free-associates of 4 different groups with 2 series of stimulus-words and studied for each the sex differences in frequency of response along 12 to 15 lines of interest, we conclude from analysis of one group's responses that single associates (words associated once with a given stimulus-word) may be safely disregarded without significantly affecting the results; and, from two analyses of the responses of 4 groups with two sets of stimuli, that we may limit our consideration to 'contrasting' cases and obtain, with a margin for chance deviations, the same sex distinctions as where we consider either all, or all but the single, associates. This conclusion holds provided (a) the 'contrast' spans are adjusted to numbers of *Ss* and stimulus-words; and (b) where a topic of interest yields fewer associates, recourse is had to multiple or total responses.

A word is required on both these conditions. Regarding (a), the spans suitable for sample cases have already been suggested (*supra* 467). If the scores and, what is also important, the number of instances which contribute to those scores, are to be enough to signify, we need an adequate number both of stimulus-words and of *Ss*. The more the *Ss* or the more the stimulus-words the more numerous the instances expectable of spans of any given width;

hence to insure a significantly high aggregate score and a sufficient number of instances we may raise or lower the span of contrast correspondingly. It remains for the statistician to attain precision. As regards (b) a difficulty with free associations is that we can not tell beforehand the fertility of associates along any line of interest, though a repeated experiment would, no doubt, *coeteris paribus*, give statistical probabilities; so that we cannot exclude the necessity on occasion of supplementing our contrasting cases with a study of multiples. Moreover, as contrasting cases are a fraction of the whole, it sometimes happens that a single stimulus-word may yield a single associate with a high score and a wide sex divergence, (*e.g.* the word 'wine' as one of the food associates), and misleadingly bulge the aggregate sex scores. Hence not just a sufficiently high aggregate score, but a sufficient number of cases, and an eye open for unevennesses of this kind, are necessary precautions in the use of the contrasting case method. It cannot be applied, nor indeed can any method, quite mechanically.

Moreover, it appears that (1) as the sex differences disclosed depend upon the class of stimulus-word, some may easily escape representation (*e.g.* associates in terms of social or physical sciences, mechanical or manual occupations escaped the exercise under examination); and (2) the sexes agree far more than they differ in their responses. In fine a wider range of stimulus-words with a shortened analysis, confined to the most sex-contrasting responses, is more informative than a full analysis with a narrower range, and is the more generally practicable. In using free word associations, therefore, fully representative stimulus-words with examination of the most divergent associates is, it appears, the economical program.

SYMMETRY IN VISUAL RECALL

By F. Theodore Perkins, University of Kansas

Most of the experiments on memory, even the classical ones of Ebbinghaus,[1] are chiefly concerned, not with memory, but with learning. Even the *Aussage* experiments initiated by Stern,[2] which minimize the aspect of learning, were not entirely free from the complications of the learning problem. Few studies have been solely concerned with the problem of memory. The list is short.

Hempstead,[3] in 1901, noted in a tachistoscopic experiment that the changes in memory seemed to be "guided by the principles of symmetry and similiarity."[4] Kuhlmann,[5] in 1906, found that the reproductions of meaningless visual forms, which were made from 2–90 days after presentation, tend toward standardization: slight differences are omitted and parts are standardized, equated, and symmetrically arranged. Wulf,[6] in 1922, found that successive reproductions of a visual figure change continuously and gradually toward greater symmetry, and suggested a theory based upon laws of balance and stability of energy patterns. Lastly, Gibson[7] repeated Wulf's experiments in modified form, and concluded, contrary to Wulf, that the changes toward symmetry were to be explained by habit.

In the methods of both Wulf and Gibson there are certain objections to be raised in order to satisfy the requirements for a critical investigation of the memory process. First, the material should be presented only once, a stipulation that is violated by Gibson and to some extent by Wulf in his use of the 'part-stimulus.' Secondly, the stimulus-figures should be simple, in order that one brief presentation will suffice. Thirdly, there should only be a few stimulus-figures, in order that the average observer will be able to reproduce all of them after the first presentation. Fourthly, the material should be reproduced entirely from memory at various time-intervals to make possible a study of the trend, direction, and extent of the changes with succeeding increments of time, thus giving a temporal picture of the memory process.

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[1] Hermann Ebbinghaus, *Über das Gedächtnis*, 1885, 1-114.

[2] Wm. Stern, *Beiträge zur Psychologie der Aussage*, Leipsic, 1903-6.

[3] L. Hempstead, The perception of visual form, this JOURNAL, 12, 1901, 185-192.

[4] *Ibid.*, 192.

[5] Fred Kuhlmann, On the analysis of the memory consciousness: A study of mental imagery and memory of meaningless visual forms, *Psychol. Rev.*, 13, 1906, 316-348.

[6] Friedrich Wulf, Beiträge zur Psychologie der Gestalt: VI. Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-373.

[7] J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

PROBLEM

The present experiment was conducted for the purpose of ascertaining the nature of the changes that occur in the recall of visually perceived forms.[8] The problem, although the same as that of Wulf and Gibson, differed in the material and in the method used. The experiment was divided into two parts designated as A and B, on the basis of the two sets of materials used.

METHOD

As the method was the same for both parts, it will be described only once. Two sets of 5 simple, nonsymmetrical, geometrical figures were drawn with black ink on cards 14 x 11 in. The first set, called Cards a (see Fig. 1), was used with the A group of *Os*, while the second set, Cards b (see Fig. 1), was used with the B group. In all there were 150 *Os*, 98 of whom made up Group A, and 52, Group B. The *Os* were all naïve with respect to the problem as well as to psychology in general. At no time were the results of the experiment disclosed to them, or did they know in what way the research was related to the problem of memory. They were all students in beginning psychology laboratory and the experiments were performed during the regular class period. The series of figures was presented to groups of about 20 *Os* at one time. The only instructions given were: "I want you to look at this group of drawings." The cards containing the figures were then exposed, one at a time, for 5 sec., until the entire series of 5 drawings had been presented. This was the only time the figures were seen. Twenty seconds after the series had been presented the *Os* were given blank paper and the instructions: "Reproduce the group of drawings that you just saw as accurately as you can." No instructions were given regarding the size of the reproductions with respect to the original. After 2 days the *Os* were told to reproduce the figures entirely from memory, with the same instructions as before, and again at 1, 6, 7, 14, and 19 day intervals. This meant that since the stimulus-figures were pre-

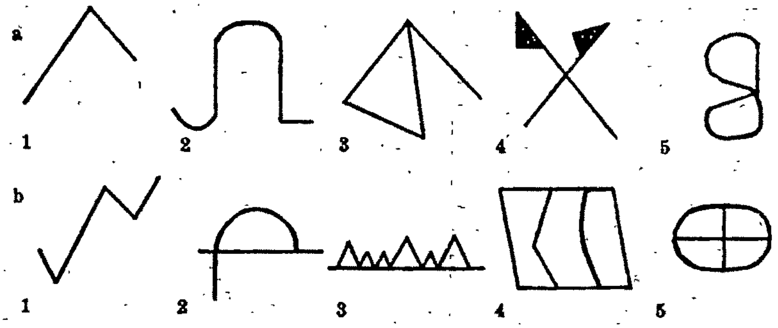

FIG. 1. STIMULUS FIGURES

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[8] The author wishes to express his appreciation to Dr. R. H. Wheeler for his suggestions concerning both the experimental technique and the writing of the results.

sented only once at the beginning, a series of reproductions was obtained that would indicate the undisturbed trend of the change with successive increments of time, with the last reproduction 49 days from the original presentation of the figure. Introspective reports were not taken.

Results

There were 3,559 reproductions of the 10 stimulus-figures. In all but a very few of these there was some change from the original that could be classed as greater than an error due to an inability in freehand drawing. As would be expected in an experiment of this kind, the changes seemed on first inspection to be as diverse and complicated as the personalities of the individuals making the drawings. Inasmuch as the present problem was not concerned with a catagorical classification of the changes, which after all would be dependent on the material used and therefore only applicable to that experiment, no such attempt was made. Rather, the task was limited to the discovery of some general trend that would apply to all the memorial material and thus lead to a broader understanding of the problem of memory.

(*1*) *Changes toward symmetry.* It became evident from detailed examination of the data that all changes were in the direction of some balanced or symmetrical pattern, which was in part determined by the figure and partly by the perception of that figure. For this reason tabulation of the figures was made on the basis of the changes toward symmetry. There were a few reproductions early in the experiment that showed no change in any direction. The changes toward symmetry were recorded in tabular form for every one of the 10 figures. The individual changes for a particular figure fell into general classes. These classes for the individual figures seemed, again, at first, to be as diverse as the figures themselves, but on closer examination they all appeared as specific cases of 9 general groups of changes, which applied in most cases to all of the figures. These general classes are not proposed as categories, but represent trends of change that occur in the recall of the type of material illustrated in this experiment. They are intended not as explanations of the change, but as examples of the types of changes that appear. [illegible] in mind we may proceed to a consideration of the changes [illegible] try.

[illegible] A change in which corresponding parts of a figure, that [illegible] riginal stimulus-figure, are made so in subsequent repro- [illegible] change took place in two ways in the present experiment, first, in an equalization of lines in a figure, and secondly, in an equalisation of angles. Practically all of the classes of changes mentioned in this section occurred in all of the figures, except where two changes were contradictory. In many cases several types of change appeared in the same reproduction of a given figure. Fig. 2, No. 1 gives examples of this type.

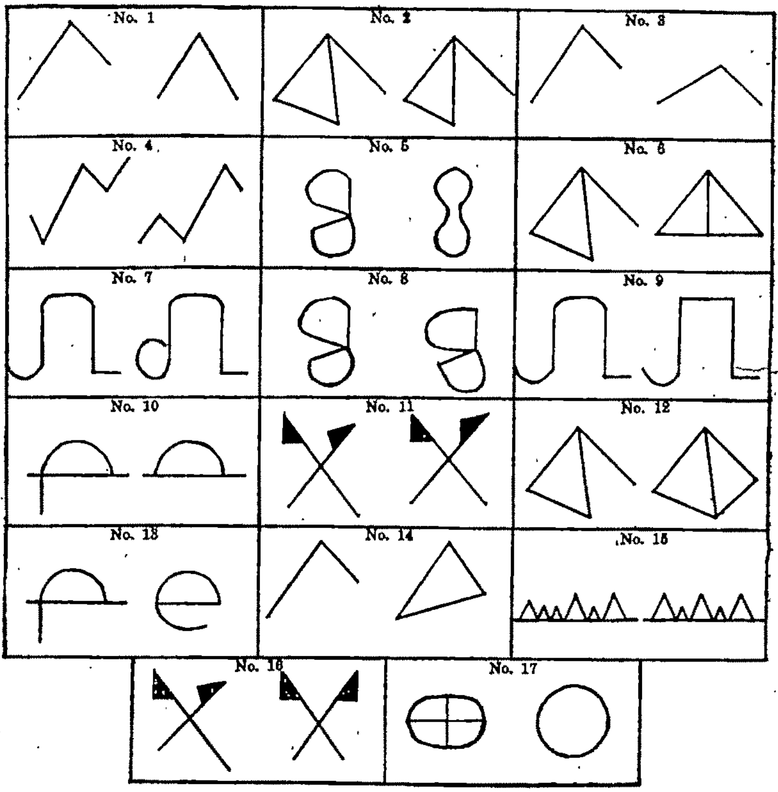

FIG. 2. TYPICAL CHANGES IN REPRODUCTIONS OF THE STIMULUS-FIGURES
The first drawing in every case represents the original figure, the second the reproduction of the figure illustrating various types of change.

(*b*) *Orientation*. Orientation may be a change that involves the whole fig [illegible] or only a part of it. It may take any one of three forms. First, there [illegible] verticalising of the figure or a part of the figure that was either ho [illegible] angle in the original stimulus-figure (Fig. 2, No. 2). Secondly [illegible] form of making the figure or a part of it horizontal when [illegible] (Fig. 2, No. 3). Thirdly, the orientation change may b [illegible] inversion with reference to the stimulus-figure (Fig. 2 [illegible] (*c*) *Standard figures.* This change is one that may take the form of a trend toward the symmetry of certain standard objects (glasses, dumb-bell, dollar sign). In this case the change is determined by the nature of the figure (Fig. 2, No. 5) and not by habit. The second type of change is that toward standard geometrical figures or parts of figures that are symmetrical (Fig. 2, No. 6). There are three special cases under this type of change that deserve special mention. The first of these is a change toward circles or parts of circles (Fig. 2, No. 7); the second is toward ovals or parts of ovals (Fig. 2, No. 8); and the third is a tendency toward squaring (Fig. 2, No. 9).

(*d*) *Simplification.* A change toward simplification of a given figure may assume two forms: first, the omission of parts or lines destructive to the symmetry of the figure—*i.e.* of parts that stand alone in the stimulus-figure, upsetting its balance (Fig. 2, No. 10); secondly, the decrease or shortening in successive reproductions of characteristics that upset the balance of the figure (Fig. 2, No. 11).

(*e*) *Complication.* A change toward complication is of necessity the opposite of simplification, and like simplification, may take place in two ways. First, there may be additions to the figure (Fig. 2, No. 12); and secondly, some characteristic of the figure may be increased, with subsequent reproductions, toward a more symmetrical pattern (Fig. 2, No. 13).

(*f*) *Completion.* A change toward completion of the original stimulus-figure may take place either in the addition of some new part of the figure or as an enlargement of some portion of the figure in making a closed and symmetrical form out of the original (Fig. 2, No. 14). The factor of completion depends of necessity on the nature of the figure involved.

(*g*) *Proportional relations.* Proportional relation of parts is a change which involves altering the position of some member, having no relation other than spatial to some corresponding part, so that the former is proportional to the latter (Fig. 2, No. 15).

(*h*) *Bilateral symmetry.* A change to bilateral symmetry occurs when the two sides of the stimulus-figure are nonsymmetrical in some detail and that detail is changed to correspond to the opposite side (Fig. 2, No. 16). It may be not only a change which involves the differing member, but may involve a change toward the differing member, or a change of both sides to a new form.

(*i*) *Whole symmetry.* Whole symmetry involves a change in the original stimulus-figure to a figure that, taken as a unit with all its members, is recognized as entirely symmetrical. It may occur in a change of a few details in the figure or it may be a change, involving the whole figure, toward an entirely new form (Fig. 2, No. 17).

(*2*) *Average change per drawing.* In tabulating the changes in the reproductions of the individual figures the necessary data were obtained to make possible a computation of the average change per drawing for successive reproductions of the 10 stimulus-figures. In doing this it was found necessary to compute the average time-intervals between the various reproductions of the different figures. This was necessary for the reason that some of the *O*s did not make all of the reproductions that were called for, which meant that the time-intervals between the first and the second drawings would not be the same in one case as in another. For this reason the average time-intervals between reproductions are used in the data given. Fig. 3 presents graphically the percentage of change toward symmetry per drawing for each successive reproduction of the 10 figures. Both the A and B groups are included in this graph. The average cumulative time is given on the abscissae and the percentage of change on the ordinates.

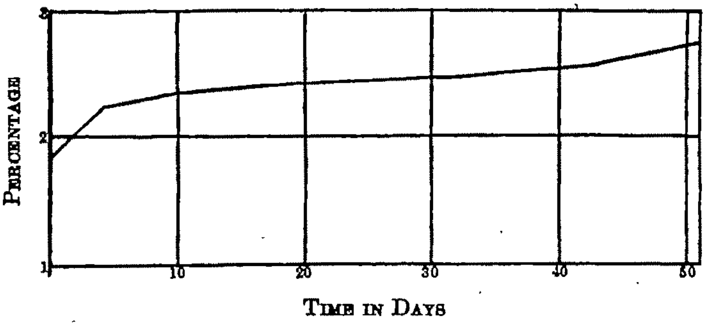

FIG. 3. PERCENTAGE OF CHANGE TOWARD SYMMETRY

It will be noted on examination of this curve that the greatest number of changes in the figures occurs on the first reproduction, that is, the greatest number of changes, in comparison to the increase with successive reproduction, appears in the reproduction that immediately follows the presentation of the stimulus-figures. The curve also shows that the increase of changes per drawing is proportionately greater in the first part of the experiment, and, although the changes per reproduction still increase as time goes on, the increase becomes less with each successive reproduction. There is, however, some indication that the increase in the number of changes becomes greater again toward the last of the experiment, after about 50 days (average cumulative time) from the presentation. These facts in all probability, mean with respect to the problem of memory, that at the first of the experiment there were a great many minor or smaller changes that came on the first or second reproduction, and thus account for the large increase at the first of the series. Whereas, as the experiment progressed, the changes that occurred were of greater significance, that is, involved more of the figure, and therefore of necessity could not be correspondingly as great in number. These later changes were also, as we shall point out later on, more progressive in their development, which meant that the later changes were only gradual modifications of the large number at the outset. This would give meaning to the rapid increase in number of changes at the first of the experiment and the more gradual increase later on.

(3) *Figures with no change.* In tabulating the results of the various reproductions of the stimulus-figures not only the figures with changes toward symmetry were recorded, but in addition, the figures with no changes. Fig. 4 (dotted line) shows the graphic representation of these results. The average cumulative time is represented on the abscissae, and the percentage of the total number of reproductions for a given day that show no changes is represented on the ordinates. It will be noted that even at the outset there is a very small number of figures with no change and that this initial percentage decreases very rapidly on the second reproduction. The percentage of the figures with no change on the succeeding reproductions is very small—1% or less. It is quite evident from this graph that there are relatively few figures with no change throughout the whole experiment and that these are confined for the most part to the first reproduction.

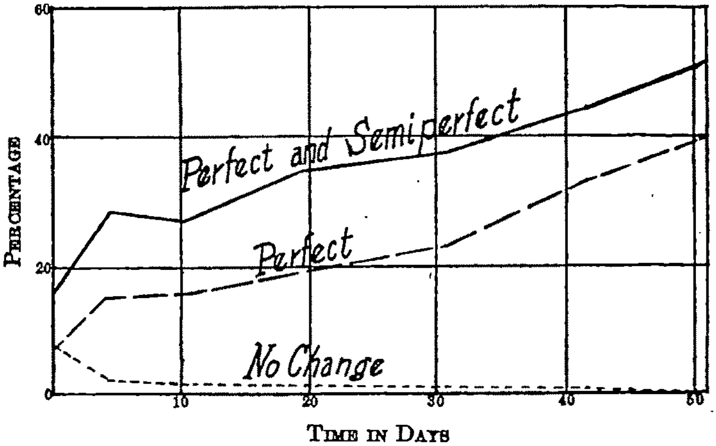

FIG. 4. PERCENTAGE OF REPRODUCTIONS IN WHICH 'NO CHANGE' WAS MADE, AND IN WHICH THE CHANGE WAS TO PERFECT SYMMETRY, AND TO PERFECT AND SEMIPERFECT COMBINED

(4) *Symmetrical figures.* An independent grouping of the drawings was made in which the semiperfect drawings and the perfectly symmetrical drawings were recorded for each day of the series.

Fig. 4 (dashed line) shows the percentage of perfect figures out of the total number for any given time-interval. It will be noted that the percentage of perfect drawings increases gradually with the time-interval after the original presentation of the stimulus-figure. On the seventh reproduction, or 50 days (average cumulative time) from the original presentation, perfectly symmetrical figures occurred in approximately 40% of the drawings made at the time. Along with this curve is another (continuous line) that represents the addition of the perfect and the semiperfect figures. This curve follows that of the perfect figures very closely, except as would be expected, it is higher than the 'perfect' curve. On the seventh recall the percentage is 50% of the total number on that day. This means, then, with respect to the present experiment, that each successive reproduction of a nonsymmetrical figure tends toward a symmetrical pattern, which involves the whole figure. That is, the tendency for the whole figure to change in an explicit fashion toward symmetry is more marked as time goes on.

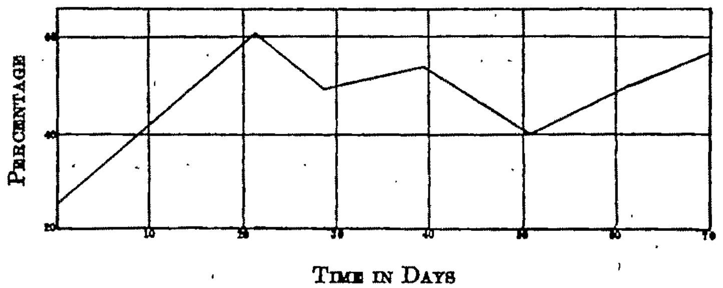

FIG. 5. PERCENTAGE OF FINAL REPRODUCTIONS APPROACHING PERFECT SYMMETRY

(*5*) *Last symmetry*. In recording the perfect and the semi-perfect figures, a record was also kept of the figure's location in *O*'s series of reproductions. That is, if that particular perfect figure was the last in the series as far as that particular *O* was concerned, it was recorded. Here was a means by which to determine the percentage of the total number of last drawings that was symmetrical. Fig. 5 shows the results of this analysis of the data. The average time-intervals between these symmetrical drawings are given on the abscissae and the percentage of the total last drawings that were symmetrical is given on the ordinates. The curve ascends very rapidly from the first to the second reproduction. It will be noted, however, that the greatest average time-interval occurs between the first and the second reproduction. The reason for this is that in this case the *O*s did not draw the figures again, after the first reproduction, until relatively near the end of the series. This would mean that the time-interval was longer. This high point on the second day is in fact the high point for the rest of the curve. The remainder of the curve descends, with time, until toward the end of the series when it again mounts to very nearly the point reached on the second reproduction. Interpreted, this means that in the recall of visually perceived forms the tendency toward symmetry is more marked when the reproductions are separated by a wide interval of time, than if the same time-interval is divided by a number of reproductions. The drawing of the material, then, must in itself serve as an added stimulation which cuts across the tendency toward symmetry, if those drawings are spaced relatively closely in time. The rise at the end indicates, however, that, although the foregoing statement holds, the symmetry tendency will appear, late in the series, even when the drawings are called for at fairly close intervals of time.

(6) Progressive nature of change. Probably the most significant fact that came to light in the present experiment was the progressive nature of the change toward a symmetrical pattern. If a particular change occurred at the outset it was progressively exaggerated or diminished, as the case might be, with each successive reproduction of the material (Fig. 6). For this reason many of the changes that occurred were trends in the direction of some symmetrical pattern, the culmination of which was never reached in the length of time used in this experiment.

We have already presented evidence which demonstrates that with a long interval of time between reproductions this progressive change is less gradual than when a number of reproductions are made over the same length of time. The curve giving the average change per drawing also corroborates this supposition. Whereas a large number of small changes are made at the outset, in the rest of the series the increase in the number of changes is more gradual. This means that the first changes are those which visibly involve minor parts of the figure, while those which come later involve more nearly the whole figure. Thus, by necessity, the later changes cannot be correspondingly as great in number as the minor changes, although the latter may still exist in the present figure.

The major changes, because of their comprehensive character, can only be a gradual modification of the changes that already exist. The changes that appeared at the outset are progressively modified in the direction of the symmetrical pattern, a pattern which involves the entire figure. As this progressive change continues in the direction of symmetry it involves certain minor changes toward symmetry. That is, certain of the parts of the figure are involved in particular ways even in a general change that involves the whole figure. But as the general change is progressing gradually, the subordinate, part-changes emerge slowly and thus account for the smooth gradual progression of the curve depicting the average increase in changes per drawing with each successive reproduction of the figure. The progressive nature of the changes, the smooth character of the increase in changes, and symmetrical drawings, all represent evidence for some important suppositions about the memory process. They indicate that the energy pattern functioning in any memory process

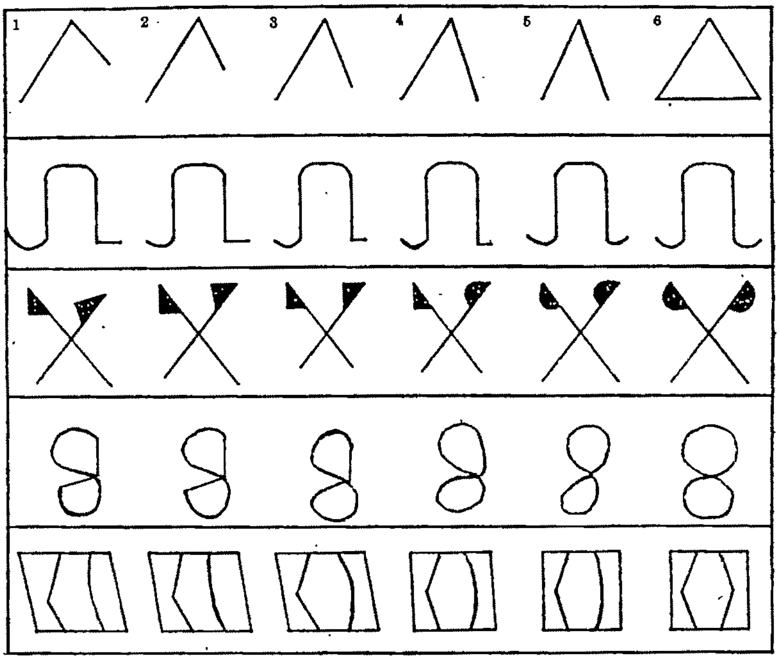

FIG. 6. EXAMPLES OF SUCCESSIVE REPRODUCTIONS
The numbers represent the position of the recall period with respect to the series.

is one that is dynamic in character and not static; a pattern that changes at a gradual rate and not by a sudden shift. The significance of such a supposition will be pointed out in a subsequent section of the paper.[9]

DISCUSSION

Gibson's interpretation of the memory process. Gibson,[10] in his paper on the reproduction of visually perceived forms, following the work of Wulf,[11] finds certain points on which he would differ with Wulf in his interpretation of results, although there is nothing contradictory in the results themselves. The present experiment yields data in harmony with those of both preceding experiments, and in addition, seems to present evidence that would lead in the direction of the interpretation given by Wulf, not that given by Gibson.

The difference between Gibson and Wulf hinges about the concept of structurally conditioned change. Wulf claimed that certain changes which he observed were due to the *Struktur* or form of the figure. Inasmuch as this type of change seems to interfere with other changes attributed by Wulf to certain modes of apprehending the figures, Gibson argues, "this type of change is independent of the way in which the figure is perceived."[12] Although Gibson is no doubt correct in interpretating the form that Wulf speaks about as something which is not contained in the stimulus, but is phenomenological, he becomes ambiguous in assuming that the phenomenological and the perceived form are two different things. Gibson has overlooked the fact that phenomenological form, certain properties of which are determined by the way in which the figure is apprehended, is the *only* aspect of the object existing for the observer. The confusion is apparent in this quotation, "one is led to infer that the structurally conditioned change, then, is due entirely to this tendency towards a specific, lucid, complete, or 'good' *Gestalt.* The configurationist's *form* is apparently something which, in its own right and independent of the experience of the observer, possesses the capacity to change."[13]

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[9] The reader will probably raise the question whether the same trend toward symmetry occurs in the recall of figures that are originally symmetrical. This question can be answered in the affirmative from various tests in the laboratory the results from which were so uniform and consistent that a systematic investigation seemed unnecessary. In successive recalls the patterns retained their symmetry but changed, in many cases, toward greater simplicity. In some instances, first reproductions made the figure more complex but the symmetry was always maintained. A change toward complexity can easily be explained in terms of the fact that the observer estimated the figure, originally, to be more complex than it was. Changes toward greater complexity generally occur when the figure is exposed for too short a time or when the observer examines the figure under distraction. In this general connection it is interesting to note that symbols, used commonly, are almost universally symmetrical, or nearly so. One does not change his memory of figures toward conventional symbols, because of habit, as Gibson implies, but the law of symmetry accounts for the habitual symbol.

[10] Gibson, *op. cit.*

[11] Wulf, *op. cit.*

[12] Gibson, *op. cit.*, 35.

[13] *Idem.*

Yet the *form* that Gibson is referring to is certainly not divorced from the experience of the observer, inasmuch as the experience provides the form. Perception and phenomenological form are two names for the same experience.

The issue is raised by Gibson; "Is the change in a reproduction of a perceived form caused by the influence of past perceptions on the perception and memory of that form, or is it a change caused by the nature of the form itself?"[14] No definite answer is given to the question. But again by implication the form is something over and apart from the perception. Gibson interprets *form*, as used by Wulf, as independent of the apprehension of form, and in addition, something which is divorced from the rest of the individual's experience. Such a dualism is an obvious fallacy. In fact the form *is* the apprehension of the object and is in no way something different in kind from the past perceptions which Gibson emphasizes. On the contrary, form is an evolutionary resultant in the continuity of the memory process.

As a substitute for the notion of form or structure, Gibson offers the following explanation of his results: "The changes observed in these experiments . . . are evidence for the existence of perceptual habits which have arisen in the individual during experience."[15] And further, "If, then, after an interval a reproduction of the figure is required, the assimilation becomes quite evident since the older traces, which fade more slowly than the new ones, are present to the exclusion of new ones."[16] It is questionable whether the changes mentioned by Gibson give evidence for the existence of old perceptual habits. We are not told exactly what a perceptual habit is, but Gibson later states that it is a process which takes place "by turning of a new perceptual process into earlier channels of perceptual activity."[17] The unanswered problem arises at once, What initiates and carries on this process of turning? Obviously Gibson must assume something more before he can approach an explanation, a something which will necessarily be the *sine qua non* of his theory. This something is adequately explained by the configurational assumption of a dynamic field functioning under the laws of fluid energy.

Gibson is in further difficulty. How is one to account for the fact that the number of changes *increase* as time goes on, and are a *progressive modification* of the earlier changes? From his statement that the changes are to be explained in terms of old perceptual habits, the only logical suppositions to make are that, first, *all* of the changes in a particular figure, showing any change at all, should appear on the first reproduction, and secondly, that all of the figures should change to the maximum on the first reproduction. Since none of the facts from Wulf, the present experiment, or even from Gibson's own experiment support this position, the theory must obviously be inadequate.

Gibson falls back, finally, upon a trace theory of memory. He says that, "the older traces, which fade more slowly than the new ones, are present to the exclusion of new ones."[18] It is impossible, from Gibson's discussion, to determine just how he envisages these traces in the organism. The absurdity of a purely structural trace, of course, has been evident for nearly two centuries. This Gibson seems to realize, for he promises us a functional explanation. But he then presents the theory that new perceptual activity takes place in terms of old perceptual habits and that these new processes are turned into earlier channels of perceptual activity. An explanation which interprets events in terms of the processes that have preceded them, is, however, a long way from a functional viewpoint; in fact, it is only a restatement of the static trace theory. If everything new is to be explained in terms of the old, wherein is it dynamic? The most significant facts that we know about the memory process, those of directional change and newness, must of necessity be disregarded by Gibson's position.

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[14] *Idem.* [15] *Ibid.*, 39. [16] *Ibid.*, 38. [17] *Idem.* [18] *Idem.*

"The notion of an old memory image fusing with a new perception, and the notion of an old configuration asserting itself over a new configuration differ essentially only in the terms used," is an assertion of Gibson.[19] His own logic, has, however, led him astray. He fails to interpret correctly the position that Wulf is taking. The *Gestalt* position would make it logically impossible for an old configuration to exist with reference to the organism, inasmuch as the only configuration that exists for the organism is the one *in the present.* That is, although the present configuration is a continuation from the past of the organism, it in no sense is caused by the past,—or is it old. This does not mean that there is an isolated configuration every second, but means that the configuration is both a spatial and temporal *unit.*

A new theory of memory. Certain facts have come to light in this experiment that suggest a general theory of memory. Such a theory, to be adequate, however, must fulfill certain requirements before it can pretend to cope with the problems in this field. (*1*) The explanation must ultimately be based on the concept of mental continuity. In fact memory and mental continuity are different aspects of the same problem. (*2*) It must be a dynamic concept in order to account for the progressive nature of the changes that occur in recall. (*3*) It must account for the direction of the changes. (*4*) It must account for the increased changes occurring with successive increments of time.

(*1*) The first problem to consider is that of mental continuity, more specifically that continuity implied between original observation and recall. How can mental continuity be envisaged in organismic terms? Observation and subsequent recall are members of a *temporal configuration.* By a temporal configuration is meant a given whole in mental life, any part or experience of which has significance only in its relationship to the total continuum within that whole. No one phase of the continuum will explain any other. The observation will not account for the recall. A past experience will not account for a present one. The two *depend* on each other, not in terms of cause and effect relations, but *by definition.* Both are necessary before there is anything to explain. The one does not exist without the other for the same reason that the first half of an inch, by definition, cannot exist without the other half. Both, in either case, belong to the same descriptive unit.

The problem then becomes; How may we account for this unit physiologically? We conceive of the physiological growth of the organism as a gradual, evolutionary process, going on under a growth potential and directed by an alignment of stresses which Child calls a physiological gradient.[20] In this growth process the particular specialized structures develop not by the addition of new entities, but as emergence phenomena differentiating from a relatively unspecialized field of energy. Likewise, it is supposed that the mental development of the organism is due to the same maturation potential. Growth is a variety of motion; it is the growth potential realized in kinetic energy. Some of this energy is realized in the growth of organic structure, some in overt bodily movement, and some in mental development. Because it is stimulation that induces the differentiation of the potentials, observation and recall are merely phases of this development separated by removal of certain modes of stimulation.

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[19] *Idem.*

Maturation, with respect to a given object to be recalled, is a continuation of that particular process of differentiation, the emergence phenomena continuing with each recall. Mental continuity is then accounted for by the fact that although the organism is different every second of its existence, there is no break between what it is one second and the next because the realignment of stresses is an unbroken continuum, even between observation and recall. So far we are in accord with the logic of Köhler's theory of the non-process change.[21]

The organism, however, is at each instant a unitary intact whole in space and in time, the parts and temporal phases of which do not combine to form the whole. The experiences that the organism has had are represented in this unitary whole only in terms of the general differentiation of the neural field, that is, in a level or degree of maturation. General structurization of the entire field is the only way in which maturation is represented. Specific structurization exists only with reference to the stimuli of the moment and passes with specific modes of stimulation. In other words, the high and low potentials, corresponding to specific experiences existing at any instant, are not represented by permanent structural changes in the nervous system. Rather, one has an experience as the potentials of the nervous system are realigned in terms of existing stimuli. There is a resolution of this potential and the experience terminates.

The most important point to bear in mind is the maintenance of dynamic balance, or movement in the direction of balance, all the while structurization is occurring. The preservation of dynamic relations *during* processes of change, when a process of change occurs in the course of reëstablishing a balance, is hard to understand and even more difficult to picture. This preservation of dynamic relations during change is the physiological basis of continuity in the maturation process, and is the only representation of the organism's 'past experiences.' But note, the dynamic situation is constantly changing, and is general in character.

What, then, accounts for the reference to the past, the feeling of familiarity, so often regarded as the phenomenological criterion of recall? There are no traces. There is nothing in the preservation of dynamic relations in a changing field that guarantees familiarity. Each feeling of familiarity represents the temporary emergence of a specific quality from the existing conscious field, integrated with the field as it emerges. The emergence is induced by the stimulus-pattern of the moment. Feeling of familiarity, or reference to the past, is no different, functionally, from reference to the future, or feeling of strangeness. Both are new responses organized with reference to the goals of the moment, and are both on the order of discoveries, involving insight into total situations.

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[20] C. M. Child, *Physiological Foundations of Behavior*, 1924, 2.

[21] Wolfgang Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 115-175. For a critical discussion see R. H. Wheeler, *The Science of Psychology*, 1929, 276 f.

The problem of reference to the past and reference to the future is no more solved by traces than is the problem of a difference between seeing red and seeing green. If we must have a trace to account for reference to the past, by the same token we need a trace to inform us when we see red, another to tell us an object is green, and so on. We have then shifted all mental processes, as emergent phenomena, back to the nervous system and are no farther along than when we started. We have made the trace accomplish what mental processes are supposed to accomplish, inform us of meanings.

Finally, new experiences are logically prohibited by a trace theory. If experience could change, the extent of the change must be known to the observer, and a comparison made with the original experience. This is because the same trace that has lasted over has been changing. Under these conditions, also, nothing different from the original experience would ever be reproduced. A changing or fading trace is a logical contradiction in terms.

To illustrate from the present experiment the preservation of dynamic relations in a changing field, consider the observation and recall of any particular figure. The retina is subject to excitation by a certain arrangement of stimuli, disequilibrating preëxisting stress-patterns in the brain. The process of reestablishing a balance corresponds to the perception of the figure. As long as the stimulus continues the disequilibration continues and the perception, likewise, in the course of reëquilibration. The stimulus is removed and the perception ceases. But the figure itself was by no means the total stimulus-pattern in terms of which the perception was formed. The perception of the figure emerged from a ground which was sustained by the remainder of the stimulus-pattern. The ground is represented in the stress-patterns previously mentioned, the figure and the ground being represented by a gradient with the figure corresponding to the region of highest potential in the gradient. Stimuli sustaining the ground were the classroom situation, the presence of the instructor and students, words used by the observer to describe the pattern as he was observing it, and instructions given by the experimenter.

Assuming that the observer does not think of the figure after it is removed, what goes on between this period of observing and a subsequent recall? The observer goes about responding to other stimulus-patterns, using the nerve structures for other purposes that were formerly employed in observing and will be employed in recall. The neural field is constantly being subjected to disequilibration and is constantly releasing its potentials in the course of reëstablishing the balance. With different stimulus-patterns, parts of the field are required to balance themselves in different ways, so that those parts of the field that shall have the highest potentials vary at different times. These must all find their balance, but, in each instance, the balance will be a different one. This continuous change toward balance, while at the same time the balance required is constantly changing, is the essence of maturation. Thus, by the time a recall situation is introduced, the stimulus-pattern setting up the recall will not have the same field to disequilibrate as did the original stimulus-pattern. But the original stimulus-pattern and the subsequent partially repeated stimulus-pattern are conditions in common both to the observation and to the recall. Thus the reason why a person is able to recall an experience is, to this extent, the same as the reason why he was able to make the original observation. The one does not depend on the other in a causal sense but both depend upon similar conditions.

Now we have the setting for the recall. The process of recall is induced (the observer coöperating) by portions of the stimulus-pattern similar to the original; but the figure itself is absent.[22] The field-arrangement of stresses, now resting upon a different base (different level of maturation), will be somewhat like the original. Incompleteness of the stimulus-pattern and an alignment of stresses upon a different base account for the incompleteness of the reproduction as compared with the original. Since, functionally, the organism is very nearly the same, that is, the maturation base maintains a certain constancy of major characteristics in the form of dynamic relationships of various parts of the nervous system to each other, the partially repeated stimulus-pattern has something of the same dynamic relation to the whole as did the original. This actual correspondence is the situation that makes possible an organized, insightful response of the observer to the repeated situation. The phenomenological characteristic of this response is his feeling of familiarity or reference of his experience to the past. Constant major characteristics of the maturation base, a continuous adjustment of a particular organism to its own environment, account for the fact that the memories for one person are different from the memories of another. In reality there has been no more of a trace than if the observer was confronting a novel situation and insightfully perceived it as new. For in the case of a novel situation a certain dynamic relation between the organism-as-a-whole and the stimulus-pattern must also be the starting point of the new judgment. Judging an experience as old is, in this same sense, a new judgment, akin to an invention.

(*2*) The progressive nature of changes in recall, and the direction of these changes, are to be explained in terms of least action.[23] The changes in successive recall were all toward symmetry. Why? Here, as Wulf would say, the answer lies in the laws of *Prägnanz*, which, we believe, should be interpreted in terms of least action. The most stable energy patterns are those which are structured symmetrically, the most stable form of all being a perfect sphere. It is also the pattern that requires the least energy to maintain. The predominance of symmetry, especially in simple forms in nature bears out this point.

Other things being equal, therefore, an observer will not only perceive symmetrical and balanced stimulus-forms more easily than others, but he will reproduce them more easily. In terms of least action the pattern of disequilibration set up by an asymmetrical stimulus will ordinarily assume more symmetry than corresponds to the original arrangement of stimuli. Thus the observer will be inclined to see the figure more symmetrical than it is.

(*3*) But we have not answered the question, Why are the errors of recall *progressively* in the direction of symmetry and simplicity? The difference in energy values between the high and low potentials of the field, corresponding to the figure and ground of the recall process, becomes greater with each successive recall. There are two outstanding reasons for this situation. First, the farther the observer is removed, in time, from the original stimulus-pattern, the fewer are the stimuli in the recall situation that duplicate those of the observational situation. Secondly, the longer the elapsed time between observation and recall the more the maturation base has changed.

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[22] For a theory based on the likeness between the original stimulus-pattern and a later one see Wheeler, *op. cit.*, 271 f.

[23] For a general application of this law to all behavior see Wheeler, *op. cit.*, 79 ff.

Imagine, now, an observer 'set' to recall under these conditions. The 'set' is the stress induced by instructions, or some suggesting factor, and exhibited phenomenologically in the effort or will to recall. But in terms of what has just been said it is more difficult than ever to resolve this stress in terms of the original stimulus-situation. Because the difference in energy values is now so great between the high and the low potentials of the system which must function in the recall, much more energy is required in shifting from the one to the other. The resolution will, therefore, take its course over a shorter and simpler route than heretofore. In other words, the reproduced figures will approach greater symmetry and simplicity.

(4) When the difference between the high and the low potentials becomes sufficiently great the recall situation, meager in stimulation, releases a limited amount of energy. All of this energy is expended in the recall, but since so much of it is required in the resolving process, little remains for the finished reproduction. This situation exaggerates, in terms of least action, the symmetry and simplicity of the figure.

This explanation accounts for the difference in symmetry and simplicity found in recalls close in time to the original observation, and recalls farther removed in time without intermediate reproductions. We may suppose that the energy value between the high and low potential during immediate recall is low and that the energy value between the high and low potentials in delayed recall is high. But this does not mean that the amount of energy available for expenditure during recall is different in both cases, for the energy of both is the energy of the total field. In fact the energy that maintains the figure in immediate recall is much greater because energy not expended in resolving the stress is used in maintaining a more complicated figure. In the delayed recall, however, more of the energy of the entire field is preëmpted in the resolution of the differential between the high and low stress and there is less energy to maintain the figure. The figure will, therefore, be simpler and more symmetrical. In each case the recall, with respect to the field as a whole, has been an all or none process.

The structurization of the field as a whole, in the course of maturation, may complicate matters considerably, leading in some cases to the availability of more energy and therefore to the reproduction of a figure of increased complexity either over the original or over a previous recall. Or, it may lead to numerous qualitative changes in the direction of other symmetries than the one toward which the observer was at first progressing.

SUMMARY

(1) The voluntary reproduction, from memory, of visually perceived forms undergoes a change toward symmetry and simplicity.

(2) The change toward symmetry varies, under the conditions of the present experiment, in proportion to the interval of time

between original observation and first reproduction, or between one reproduction and another.

(3) The change toward symmetry is progressive with successive reproductions.

(4) Based on the results of this experiment a general theory of memory is proposed that emphasizes the following principles: (a) The temporal unity of the memory process precludes the possibility of explaining recall in terms of observation. Rather, recall and observation are, like two halves of a whole, dependent by definition. Factors explaining memory are external to a temporal configuration, structured with respect to time, involving both the recall and the observation. (b) Memory is based on maturation and partially duplicated stimulus-patterns. The feature of maturation important for the phenomenon of memory is the preservation of dynamic relations in a changing energy field, that is, a continuous change toward balance while at the same time the balance required is constantly changing. The details of this process are explained in terms of physiological gradients, high and low potentials, and the laws of the resolution of differentials. These are, first, the law of least action, and its special case: the demand for stability of pattern in symmetry and simplicity of form; and secondly, the law of maximum work which is the all-or-none principle applied to energy fields.

A COMPARISON OF INDIVIDUALS AND SMALL GROUPS IN THE RATIONAL SOLUTION OF COMPLEX PROBLEMS

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The work done before 1920 in the field of group activity as compared with individual activity has been summarized adequately by Allport.[1] Since then other studies have been made principally by G. B. Watson,[2] Bechterew and Lange,[3] South,[4] and Belaev.[5] Many of these studies deal primarily with groups wherein individuals are interested in the same stimulus but there is little or no direct social intercourse and stimulation. The work of these individuals is contrasted with that done by isolated individuals.

The present study aimed to present individuals with an actual problematic situation which would call for real thinking to arrive at a proper solution. The problems selected (given in detail below) involve a number of steps all of which must be correct in order that the right answer may be found. The problems were to be given to single individuals and to small groups of coöperating individuals, in order that the abilities of these two might be compared. The problems were such as to make it practically impossible for one individual in the group to get a correct answer instantaneously by a sudden insight into the situation. Rather, they allow all to participate in arriving at a solution; they call for interchange of ideas and for acceptance or criticism and rejection of any idea put forth. At least some of the previous studies have dealt more with the performance of

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[1] F. H. Allport, *Social Psychology*, 1924, Chap. 11.

[2] G. B. Watson, Do groups think more efficiently than individuals? *J. Abn. Soc. Psychol.*, 23, 1928, 328-336.

[3] W. Bechterew und A. Lange, Die Ergebnisse des Experiments auf dem Gebiete der kollektiven Reflexologie, *Zeit. f. angew. Psychol.*, 24, 1924, 305-344.

[4] E. B. South, Some psychological aspects of committee work, *J. App. Psychol.*, 11, 1927, 348-368, 437-464.

[5] B. V. Belaev, abstract received from the author, entitled "The Problem of the Collective and its Internal Structure." Other references are: W. A. Barton, Jr., Group activity versus individual activity in developing ability to solve problems in first year algebra, *J. Educ. Administration and Supervision*, 12, 1926, 512-518; H. E. Burtt, Sex differences in the effect of discussion, *J. Exper. Psychol.*, 3, 1920, 390-395; M. P. Follett, *Creative Experience*, 1924; K. Gordon, Group judgments in the field of lifted weights, *J. Exper. Psychol.*, 7, 1924, 398-400; A study of esthetic judgments, *ibid.*, 6, 1923, 36-43; E. C. Lindeman, *Social Discovery*, 1924; A. D. Sheffield, *Creative Discussion*, 1927; T. W. Thie, Testing the efficiency of the group method, *English Journal*, 14, 1925, 134-137.

certain rather elementary tasks such as word-building, vowel cancellation, multiplication, turning fishing reels, etc., which have allowed but little constructive coöperation wherein ideas may be accepted or rejected either in whole or in part.

The problems used here are still quite far from the usual type of problematic situation met in real life. They are problems admitting of only one answer; there is only one type of best solution. They arouse none, or very little, of the emotional bias with which persons characteristically approach life situations. It can be seen at once, then, that not all of the results obtained are directly applicable to any or all group situations.

METHOD AND PROCEDURE

Subjects. The *Ss* in this experiment were the students in the class in Social Psychology at Columbia University. This group is a rather highly selected one, in view of the fact that almost all are graduate students, a large percentage of them working for advanced degrees. The experiment was divided into halves; 3 problems were used in the first half and 3 in the second half given 2 weeks later. In the first half there were 2 groups of 4 women each and 3 groups of 4 men each. There were 9 men and 12 women working on the problems individually in the same room. In the second half of the experiment there were 2 groups of 4 men each and 3 groups of 4 women each; and 10 men and 7 women worked as separate individuals in the same room. Thus data were received from an equal number of groups of men and women, *i.e.* 5 groups of each. It will be seen from the above that a group was never composed of the two sexes, but in all cases of either 4 women or 4 men. It was believed that this arrangement would, in general, make for better coöperation and more smoothly running groups. The grouping was not made for any purpose of sex comparisons; no such comparisons are made in this study.

Problems. (a) *First half of experiment.* The directions used in the first half of the experiment were as follows:

(1) Materials for this problem are in the envelope marked 'Problems I and II.' Use disks H1, H2, H3, W1, W2, W3. (For the present disregard the symbols on the reverse side.) Side 1 of the card. On the A-side of a river are three wives (W1, W2, W3) and their husbands (H1, H2, H3). All the men but none of the women can row. Get them across to the B-side of the river by means of a boat carrying only three at one time. No man will allow his wife to be in the presence of another man unless he is also there.

(2) Materials for this problem are in the envelope marked 'Problems I and II.' Use disks marked M1, M2, M3, C1, C2, RC. (Reverse side of the disks just used.) Side 1 of the card. Three Missionaries (M1, M2, M3) and three Cannibals (C1, C2, RC) are on the A-side of a river. Get them across to the B-side by means of a boat which holds only two at one time. All the Missionaries and one Cannibal (RC) can row. Never under any circumstances or at any time may the Missionaries be outnumbered by the Cannibals. (Except of course, when there are no Missionaries present.)

(3) Materials for this problem are in the envelope marked 'Problem III.' Side 2 of the card. In Circle A arrange the disks in order of size, that is with the largest on the bottom, etc., ending with the smallest on top. Using Circle B as a transfer station, transfer the disks to Circle C so that they will be in the same

order in Circle C that they are now in in Circle A. Never place a larger disk on a smaller one and move only one disk at a time. (Number the disks for reference if you wish.)

The card referred to in these problems contained on the one side a diagram of a river, for convenience in solving the first two problems, and on the other side a diagram of the three circles necessary to a solution of the third problem.

(*b*) *Second half of experiment.* The directions used in the second half of the experiment were as follows:

(1) Materials for the problem are in the envelope marked 'Problem I.' Put these words, taken from the envelope, together so that they form the last sentence (only one sentence) of the unfinished prose selection.

> In New Orleans there is a tree which nobody looks at without curiosity and without wondering how it came there. It reminds one of the warm climes of Africa and Asia. Indeed, with its sharp and thin foliage, sighing mournfully under the blast of one of our November northern winds, it looks as sorrowful as an exile.

(2) Materials for this problem are in the envelope marked 'Problem II.' These words when put in the proper order form the last three and one-half lines of the unfinished sonnet below. Arrange them as nearly as possible in the proper order.

A boy named Simon sojourned in a dale;
Some said that he was simple, but I'm sure
That he was nothing less than simon pure;
They thought him so because forsooth, a whale
He tried to catch in Mother's water-pail.
Ah! little boy, timid, composed, demure—
He had imagination. Yet endure
Defeat he could, for he of course did fail.
But there are Simons of a larger growth,
Who, too, in shallow waters fish for whales,
And when they fail

(3) A consolidated school is to be built in the rural district shown in the diagram. The capital letters (A, B, C, etc.) indicate points (not towns) where pupils are to be picked up by two school buses. The mileage between each point is indicated on the diagram. The capacity of each bus is 35 pupils and the driver. Find the most desirable location for this school and give the route each bus must take. The buses may start at ANY point and need not necessarily start from the school each morning. Following are the number of pupils to be picked up at each point:

Point	No. Pupils	Point	No. Pupils	Point	No. Pupils
A	6	D	4	G	3
B	13	E	2	H	10
C	17	F	5	I	3

The diagram for problem 3 is given below. It was furnished to each individual who was working alone and was drawn to the scale of 1 in. to the mile. Only one diagram was provided in each group; it was drawn to the scale of 2 in. to the mile.

In addition to the specific instructions given above, the following general instructions appeared at the top of each set of problems given the separate individuals working in the same room.

Below are three problems. Work them as quickly and accurately as possible. There is a correct solution to each problem. Record your answer or state briefly how you solved the problem. When you have finished one problem and are ready

> to go on with the second, record the time to the nearest half-minute by means of the record being kept on the front board. Then proceed with the second and then the third, and record the time when each is finished. Work the problems in the order listed.

At the top of the set of problems given to each group of 4 individuals were the following instructions:

> A chairman has been appointed to manipulate the necessary materials. Work together as a coöperative group to solve the three problems given below. Work them as quickly and accurately as possible. There is a correct solution to each problem. Record your solution or state briefly how the problem was solved. Each individual, including the chairman, should make his contributions to the group solution spontaneously as they occur to him. Indicate to the note-taker when you have finished one problem and are ready to proceed to the next; the note-taker does not participate in any way as a member of the group in solving the problem.

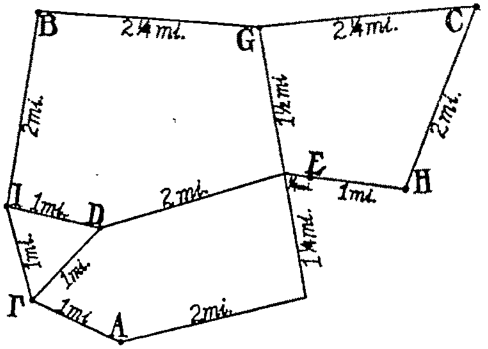

FIG. 1. DIAGRAM USED IN PROBLEM 3, SECOND HALF OF THE EXPERIMENT

Every individual in the group was provided with a separate set of the problems, but only one set of the necessary materials was given to each group; this made it more essential that all coöperate to solve the problems. The first individual in each group of 4 was appointed chairman to manipulate the materials in order to obviate the confusion and delay which might result if each attempted to carry out his own ideas. The chairman was appointed rather than being elected by the group to prevent unnecessary delay in starting to work, and because in many cases the individuals had never before met except in the more formal classroom atmosphere. It was thought that any formal or parliamentary organization of the group might tend to restrict spontaneity and coöperation and thus perhaps limit the possibilities of getting the best results.

The note-taker, referred to in the group instructions above, was for the purpose of securing more qualitative and quantitative facts concerning the group activity than the time of solving and answers to the problems would provide. In the first half of the experiment the note-taker was given the following instructions.

> You are to act as note-taker and time recorder for a group of persons solving three problems. You are to take absolutely no part in the group activity. Do not indicate by any means your approval or disapproval of their activities.

> Record the final solution of the group on each problem. Record the time required to solve each problem. Record as many of the detailed suggestions made by any member of the group as it is possible for you to get.

In the second half it was thought that the following plan might afford more quantitative data concerning the activity of the groups.

> Tally separately every suggestion made by any member of the group so that a record can be made up of the complete number of suggestions made in any one group. Get as much as is possible of each suggestion made. Record this in your notes. If the suggestion is rejected check the tally. Note whether it is rejected by the person who proposed it or by another member of the group. If the suggestion is accepted underscore the tally.

There were 10 sets of notes taken and in only 3 cases did the note-taker have any knowledge either of the problem or of its correct solution. Thus the knowledge of whether a rejected suggestion was in reality an erroneous or a correct suggestion could have but little effect on the notes taken, and can be determined by *E* only from these same notes.

Equalization of the groups. In order to equate the groups, that is, to be reasonably sure that no one group was made up of four superior individuals and that those who worked in groups were on the whole neither inferior to nor superior to those working as separate individuals, shifts were made so that the composition of the groups differed in the two halves of the experiment. Individuals making up the first group were chosen from the class roll by placing every other man and woman in a group. Certain deviations had to be made from this general procedure in the case of absence from class, since the aim was to keep the total number of men working in groups and as separate individuals approximately equal for the total experiment; the same being true for the women. Every group was composed of 4 individuals because it was thought that a group of this size would coöperate to better advantage than a very much larger or smaller one. Also the limited number of available *S*s made it impossible to increase the number much and still have enough single groups to make the comparisons meaningful. Limited room accommodations also limited the number of groups.

Table I below shows the accomplishments of every *S* as he worked alone in the first half of the experiment. Table II shows the accomplishment of the groups in the second half of the experiment. Group A′ in the second half of the experiment was composed of individuals *Y*, *G*, *I*, and *R* of the first half of the experiment; Group B′ of *K*, *U*, *W*, and an individual not before present; Group C′ of *M*, *J*, *A*, *F*; Group D′ of *Q*, *L*, *R*, *C*; and Group E′ of *H*, *T*, *X*, *B*. It will be seen that each group contained only one person who had solved a problem correctly in the first half of the experiment. Since the groups in the first half were chosen at random it was necessary to check the individuals in the second half to see that no group in the first half had been superior. From Table II can be seen the accomplishments of the individual *S*s in the second half. Group A was made up of individuals *A′*, *J′*, *N′*, and an *S* not used in the second half; Group B of *H′*, *F′*, *M′*, and *P′*; Group C of *B′*, *C′*, *X′*, and an *S* not used in the second half; Group D of *E′*, *K′*, *L′*, and *Q′*; and Group E of *D′*, *G′*, *I′*, and an *S* not used in the second half. Thus it seems probable that no group was composed of 4 superior individuals. In neither half of the experiment did a group contain more than one individual who had solved a problem correctly in the other half of the experiment, and no single individual ever correctly solved more than one problem.

TABLE I

SHOWING FOR EVERY INDIVIDUAL *S* AND FOR EVERY GROUP OF *S*s THE TIME IN MINUTES REQUIRED FOR A SOLUTION AND THE NATURE OF THE SOLUTIONS OF THE PROBLEMS OF THE FIRST HALF OF THE EXPERIMENT

		Problem 1		Problem 2		Problem 3	
		Time	Solution	Time	Solution	Time	Solution
Individuals	*D*	6.5	Incorrect	29.0	Incorrect	6.0	Incorrect
	F	4.5	"	17.5	"	13.5	"
	G	3.5	"	6.0	"	20.5	"
	H	1.0	"	3.5	"	10.0	"
	K	2.0	"	4.5	"	18.0	"
	L	1.5	"	2.5	"	9.5	"
	N	1.0	"	2.5	"	15.0	"
	P	7.0	"	5.5	"	15.5	"
	R	1.5	"	3.0	"	19.0	"
	A	6.0	"	15.0	"	14.5	Unsolved
	C	4.5	"	5.5	"	14.0	"
	W	6.0	"	18.0	"	14.0	"
	J	4.5	"	6.5	"	5.5	"
	X	4.0	"	12.5	"	13.0	"
	Y	4.5	"	10.0	"	15.5	"
	T	10.5	"	20.0	"	15.0	"
	B	3.0	"	8.5	"	60.0	Correct
	I	4.5	"	8.5	"	21.0	"
	M	5.0	Correct	8.0	"	5.0	Incorrect
	Q	9.5	"	9.5	"	5.5	"
	U	4.0	"	12.5	"	16.0	Unsolved
	Av.	4.5		9.9		15.5	
	S.D.	2.5		6.7		11.0	
Groups	A	5.0	Incorrect	7.5	Incorrect	4.5	Incorrect
	B	4.5	Correct	34.0	Correct	17.0	Correct
	C	5.0	"	12.0	"	37.0	Unsolved
	D	16.0	"	27.0	"	29.0	Correct
	E	2.0	Incorrect	4.0	Incorrect	4.0	Incorrect
	Av.	6.5		16.9		18.3	
	S.D.	4.97		11.6		13.1	

RESULTS

Table I summarizes the time (in minutes) required for and the nature of every solution presented by a group or by the individual *S*s in the first half of the experiment. It will be seen that only 5 correct solutions out of a possible 63 were returned by the different *S*s. There were 8 correct solutions out of a possible 15 for the groups. That is, 7.9% of the solutions turned in by individual *S*s were correct, while 53% of the solutions turned in by groups were correct. It was found that the false solutions to the problems could be divided into several different types. In Problem 1 two types of false solution, handed in by 13 different individuals, made an error in the first move, 4 made an error in the third move and 1 made an error in the fifth. It took 7 single moves to solve the problem correctly. No group erred on the first move; one erred on the third and one on the fourth. Group activity would seem to insure not only a larger proportion of correct solutions, but also that even in erroneous solutions the error is not made so early in the solution.

The solution to Problem 2 shows the same. It takes 13 single moves to arrive at a correct solution. Among the individual *S*s, 3 erred on the first move, 4 on the second, 6 on the third, 2 on the fifth, 2 on the sixth, 1 on the seventh, and 2 on the eighth. The last 3 who erred on the seventh and eighth, did not consider their solutions completed as all the cannibals and missionaries had not been transferred to the B-side of the river. It is interesting to note that no group got as near as this to a correct solution of this problem and then failed to reach it. Three groups solved the problem correctly; both of the others erred on the fifth move.

Problem 3 cannot be classified so easily on the basis of the first false move, since in all cases, except in one group, once the method of transfer was hit upon success was assured. In the case of 7 individual *S*s no solution at all was recorded; it may be assumed that they were unable to reach one. Six individual *S*s skipped circle B, the transfer station. Two spread the disks out in B until 4 had been transferred; then the disks were piled up and the fifth one either transferred through B to C or skipped over B to C. After this the others were again spread out and then transferred to C. Four individual *S*s slipped the disks from underneath. One group skipped the transfer station; one spread the disks out as described above. No group slipped them from underneath; 2 groups contemplated this but abandoned it as being too simple; one group abandoned spreading them out as also being too simple. In Group C, Problem 3 has been recorded as unsolved, but this does not fairly represent the group. They had successfully transferred 4 disks into circle C and the fifth into circle B, but in manipulating to get the fifth disk into C they lost sight of their real aim and became confused. They quit work then, although saying that they 'probably could figure it out if they stayed with it long enough.' The instructions for this problem appear not to have been sufficiently clear in the prohibition of certain procedures. It was felt that too many instructions here might make the correct solution too obvious, although the bizarre plan of spreading the disks out in the circles was never anticipated. The problem had been previously tried out both on groups and separate individuals, and neither the plan of spreading out the disks nor that of slipping them from underneath had occurred. The instructions had, therefore, been assumed to be adequate. It is to be noted that several *S*s indicated that they were aware of not having followed the directions exactly. For example, one individual who slipped them from underneath commented, "This seems too simple but I can't see how it can be solved without moving a covered disk." Also, after the experiment, one individual *S* was overheard by *E* saying that "since the instructions did not say that the disks should not be slipped from underneath" she solved the problem that way because she "could not see how else to do it."

Table II gives the time and number of errors made by every individual *S*, and by every group of *S*s on the three problems given in the second half of the experiment. The seventh column, which is headed "location," gives the number of miles which the proposed location is from the best location. The eighth column gives the number of excess miles which the two buses travel, the shortest possible number of miles being 12.

It will be noted that here, as in Problem 1, a greater percentage of the groups obtained a correct solution than individual *S*s. Of the solutions reported by the different *S*s 5.7% were correct; and by the groups, 27% were correct.

The incorrect solutions in this half of the experiment also are interesting when analyzed. Among the groups there were 4 correct solutions on the first problem, the single error being the transposition of the words "time immemorial" to read "immemorial time." Only 3 *S*s completed the sentence to read as it had in the original. By far the greatest number of errors lay in the placing of the single word "there," which could be put in 7 different places without disturbing the smoothness of the sentence. This error was made (often together with others) in 10 cases by individual *S*s but by no group. Five *S*s submitted solutions wherein the structure of the sentence was poor.

TABLE II

SHOWING FOR EVERY INDIVIDUAL *S* AND FOR EVERY GROUP OF *S*s THE TIME IN MINUTES REQUIRED FOR A SOLUTION AND THE NUMBER AND NATURE OF THE ERRORS MADE IN THE PROBLEMS OF THE SECOND HALF OF THE EXPERIMENT

		Problem 1		Problem 2		Problem 3		
		Time	Errors	Time	Solution	Time	Location	Excess
Individuals	*A'*	6.0	2	27.0	Unsolved	5.0	1.25	0.0
	B'	8.0	1	37.0	”	21.0	.125	.5
	C'	12.0	10	150.0	Incorrect	35.0	.25	.5
	D'	7.0	8	25.0	Unsolved	25.0	1.0	.5
	F'	8.0	1	16.0	Incorrect	16.0	1.25	.5
	G'	2.0	1	6.0	Unsolved	13.0	3.0	.5
	H'	14.0	1	20.0	”	15.0	.25	.5
	J'	6.0	1	85.0	Incorrect	30.0	6.0	1.75
	K'	9.0	2	35.0	Unsolved	12.0	4.0	1.75
	L'	7.5	1	20.0	”	24.0	6.0	1.75
	M'	11.0	7	42.0	Incorrect	18.0	1.25	1.25
	X'	12.0	3	39.0	Unsolved	14.0	4.0	2.0
	P'	8.5	9	17.0	”	12.0	1.25	.5
	Q'	5.0	1	31.0	Incorrect	10.0	4.0	2.25
	W'	6.5	0	29.0	Unsolved	18.0	Unsolved	
	I'	6.0	0	78.0	Incorrect	29.0	4.0	1.25
	N'	10.0	0	26.0	Unsolved	10.0	3.0	1.5
	Av.	8.2		40.2		18.0	2.5	1.06
	S.D.	2.9		33.8		7.9	1.9	
Groups	A'	3.0	0	28.0	Incorrect	3.0	.25	.5
	B'	12.0	0	45.0	”	10.0	4.0	2.25
	C'	6.0	1	69.0	”	00.0	Unsolved	
	D'	2.0	0	26.0	”	8.0	5.0	1.50
	E'	4.5	0	40.0	”	10.0	1.25	.75
	Av.	5.5		41.6		8.0	2.6	1.25
	S.D.	3.5		15.4		2.9		

None of the individual *S*s solved Problem 2 correctly; 11 stated that they could not solve it and presented no solution. The 6 other individual solutions varied in their degree of imperfection. One *S* simply composed a complete sentence to finish the sonnet. Three others had either 3 or 4 of the rhyme words, but an erroneous rhyme scheme. One person had the first line in accord with the original and one had the last line correct. Others made more or less serious errors here. The second and third lines were never correctly written by any individual. All groups turned in a solution. The rhyme scheme was correct in all cases; the first line was correct in 3 cases and the last one in 4. Group B had the two middle lines almost correct. The meaning was the same as that in the original, although several words were misplaced. In some group and some individual solutions the words had been so arranged as to convey a meaning almost opposite to that in the original.

Neither a group nor an individual arrived at a correct solution of the third problem. The average error in location and average excess number of miles traveled were about equivalent for the groups and individuals. In part, the absence of a correct solution may be due to the fact that it took a long time to solve the first and second problems. In many cases the completion of the third meant dismissal for the evening; thus perhaps any apparently suitable location was accepted. (On the other hand many persons took the problems home with them in order to complete the second and third. They kept time for themselves and returned the solutions the next week.) A comparison of Tables III and IV shows that among the groups the total number of suggestions was by far the least in the third problem.

Reference to the notes kept by the note-takers will give more definite information regarding the activities within the groups. All those in a group do not participate equally in the group activity. Such remarks or tabulations as the following are found in the notes from three groups in all. (The note-takers numbered the *S*s to facilitate taking notes.) "*S* 1 and *S* 3 were leaders in the solution." From the tabulation of suggestions given in the solution referred to it is found that *S* 1 made 7 suggestions and *S* 3 made 14 suggestions; while *S* 2 and *S* 4 made, respectively, 2 and 3 suggestions. Later in these same notes we find, "*S* 2 not contributing much." In another set of notes: "*S* 3 and *S* 4 offered no suggestions during the solution." In the next problem solved by this same group *S* 2 made 16 suggestions, *S* 1 made 12, *S* 4 made 8, and *S* 3 made 5. From yet another set: "*S* 1 and *S* 4 did most of the suggesting, *S* 2 and *S* 3 not working much." In solving Problem 2 in the first half of the experiment the note-taker remarks that "*S* 2 and *S* 3 draw their own diagrams and become absorbed in them: do almost no suggesting." In solving Problem 3 "*S* 3 made no comments at all, *S* 2 spoke only a few words." In one of the above groups an *S* who participated a great deal in group activity very soon assumed the task of manipulating the material in the place of the less active

S who had been appointed chairman. Possibly one could get interesting, meaningful, and perhaps quite different sets of results by using in one set of cases groups with a chairman either equal or superior to the group, and in another set groups having a chairman inferior to the group in ability.

In some groups quite the reverse situation is found, and all members coöperate splendidly. Such comments as the following are found in the notes from the three groups: "All contributing beautifully," and later, "all coöperating and making check suggestions." From another, "suggestions coming from all four about equally." Another records, "the four members coöperate well," and on another page, "splendid group work."

TABLE III

SHOWING, FOR EVERY GROUP OF *Ss*, THE NUMBER OF CORRECT AND INCORRECT SUGGESTIONS AND REJECTIONS, AND THE NUMBER OF REJECTIONS BY THOSE MAKING THE SUGGESTIONS AND BY OTHERS OF THE GROUP

(Problem 1, Second Half of Experiment)

	Suggestions			Rejections			
Groups	No.	Correct	Incorrect	Correct	Incorrect	Maker	Others
A′	29	18	11	3	11	7	7
B′	24	18	6	1	6	1	6
C′	17	8	9	0	8	0	8
D′	20	11	9	2	9	2	9
E′	31	25	6	2	6	2	6
Totals	121	80	41	8	40	12	36

In the first half of the experiment Groups A and E solved all problems incorrectly. From notes kept there was apparently but little criticism of the work in Group E. The note-taker remarks, "All satisfied with the solution." The members of Group A, however, seemed to recognize that they were not taking all specifications of the problem into consideration, but rationalized their procedure and turned in solutions. At the end of the first problem the recorder notes that "they conclude that they have solved the problem, though perhaps not in the way the directions signify." This group is one of those referred to above in which the members do not participate equally in the group activity. In Problem 2 they finally agreed that the boat's contact with the shore would not constitute a case of outnumbering by the cannibals! In Problem 3 they failed to consider circle B always as a transfer station, but skipped it whenever it was convenient to do so.

In the other groups there is much more reference to the checking of errors and meeting the conditions of the problem. One group worked a solution through three times to be sure that they had met all specifications; in the last trial they discovered an error which would have made their solution wrong. The notes on these groups also mention numerous references to the stated problem to see that all qualifications were being taken into account.

Table III above deals with Problem 1 in the second half of the experiment. Column 2 shows the total number of suggestions made in each group. Columns 3 and 4 show respectively the number which were in reality correct and the number which were incorrect. Columns 5 and 6 indicate whether those suggestions which were rejected were respectively correct or incorrect. Columns 7 and 8 show the number of suggestions which were rejected by the individual making the suggestion or by another member of the group.

It will be noted from Table II that there was only one error (a word transposition) in any group solution. In one case, Group A′, the suggestions rejected by the proposer or by another are exactly equal in number; in all other cases more were rejected by another member. Considering all groups together, three times as many suggestions were rejected by another member of the group as by the proposers of the suggestions. Five times as many incorrect as correct suggestions were rejected, whereas of the total number of suggestions made, twice as many were correct as incorrect. This fact may be considered in connection with the relative number of correct solutions among groups and among individuals. This quantitative check on rejections was not kept in the first half of the experiment, but from its results in the second half, together with the proportion of correct solutions in the first half, and the fact that notes on the groups presenting correct solutions emphasize the checking of erroneous moves, it seems as though group supremacy in the first half might have been in part due to the rejection of incorrect suggestions or the checking of errors. Also it was found that no group was composed of four superior individuals. All this would seem to indicate that one point of group supremacy is the rejection of incorrect ideas that escape the notice of the individual when working alone. Perhaps this may be the greatest point of group supremacy.

It is impossible to say with any certainty whether the rejected correct suggestions were rejected oftener by the proposer or by another member of the group. Only 8 correct suggestions were rejected, and these were all later accepted, since the solutions were with but one exception absolutely correct. (In the solution where two words were transposed, the correct suggestion concerning their position was never made: this was a case of the acceptance and retention of an incorrect suggestion.) Five of the correct suggestions were rejected by the proposer and 3 by another, but with so small a total the difference is not significant.

TABLE IV

Showing, for Every Group of *S*s, the Number of Correct and Incorrect Suggestions and Rejections, and the Number of Rejections by Those Making the Suggestions and by Others of the Group
(Problem 2, Second Half of Experiment)

	Suggestions			Rejections			
Groups	No.	Correct	Incorrect	Correct	Incorrect	Maker	Others
A′	71	32	39	10	35	18	27
B′	49	23	26	9	20	8	19
C′	76	35	41	9	29	7	31
D′	32	15	17	2	13	4	11
E′	37	17	20	2	14	3	13
Totals	265	122	143	32	111	40	101

TABLE V

Showing, for Every Group of *S*s, the Number of Correct and Incorrect Suggestions and Rejections, and the Number of Rejections by Those Making the Suggestions and by Others of the Group
(Problem 3, Second Half of Experiment)

	Suggestions			Rejections			
Groups	No.	Correct	Incorrect	Correct	Incorrect	Maker	Others
A′	17	6	11	3	7	4	6
B′	10	3	7	1	6	0	7
C′	(No solution attempted)						
D′	10	2	8	0	5	0	5
E′	13	5	8	2	4	1	5
Totals	50	16	34	6	22	5	23

Tables IV and V summarize the data received on Problems 2 and 3. Since neither of these problems was correctly solved in any case, the value of the group checking does not appear so clearly, But a consideration of these results, with an analysis of individual and group work on these two problems, brings out the same fact as above. For example, only 6, or 35%, of the individual *S*s presented a solution to Problem 2, as compared with 5, or 100%, of the groups. But perhaps more important than this is the fact that only one individual solution presented the idea conveyed by the part of the sonnet quoted; the other presented an opposite situation. (That is, they had *Simple Simon* railing "at ill luck and unkind fate.") Three groups conveyed the correct idea, while only 2 reversed the conditions. That is, only ⅙ of the individual *Ss*, as compared with ⅗ of the groups, succeeded in grasping and presenting the proper situation. It seems not altogether improbable that this is a direct result of the rejection of incorrect ideas in the group; which, it should be noted again, is done largely by another member than the proposing one (2.52 times as many suggestions were rejected by another as by the proposer).

We find in the case of Problem 3, first, that all groups met the requirement as to the capacity of the buses, whereas 2 individual *Ss* placed more than 35 pupils in a bus (one placed 43 and the other 37 in one of the buses). Other than this, however, no superiority either of group over individual or of individual over group is shown when the two are compared as a whole.

Summary

The purpose of the present study was to compare the ability of individuals and coöperating groups of 4 persons in solving complex problems. The problems involved a number of steps, all of which had to be correct before the right answer was obtained, but they are still far from the life-situations usually met. The groups were roughly equated so that no one group was composed of 4 superior individuals, but the students used were a highly selected group when compared with the population as a whole.

Upon the basis of the data and discussion presented in the foregoing pages the following conclusions seem justified:

(1) Groups seem assured of a much larger proportion of correct solutions than individuals do.

(2) This seems to be due to the rejection of incorrect suggestions and the checking of errors in the group.

(3) In groups of the size here used more incorrect suggestions are rejected by another member of the group than by the individual who proposed the suggestion.

(4) All members do not coöperate or participate equally in the solution of the problems.

(5) In erroneous solutions (where it is possible to determine the exact point at which the first error was made), groups do not err so soon as the average individual does.

TIME RELATIONS FOR THE DIFFERENT CATEGORIES OF JUDGMENT IN THE 'ABSOLUTE METHOD' IN PSYCHOPHYSICS[*]

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The question of the time relations of judgments of different categories in psychophysics is a problem on which a good deal has been written but one on which surprisingly little quantitative experimental work has appeared. Kellogg,[1] in his historical review of the literature, mentions only 7 studies, most of which do not apply directly to the present problem.

Martin and Müller,[2] and George[3] discovered that the judgments of 'doubtful,' or undecided and the like, took longer than the formation of other categories of judgment and concluded that they should be excluded from the psychophysical measurement methods. George also found that the 'equal' judgment required more time than those of 'less' or 'greater,' the order and average magnitude of the reaction times being: 'less' = 0.88 sec.; 'greater' = 0.92 sec.; and 'equal' = 1.10 sec.

In Kellogg's own study of visual intensities, two arrangements were employed: a two-category plan, in which only judgments of 'plus' and 'minus' were admitted; and a three-category plan, in which 'equal' judgments were added to the other two categories. For each of 5 *O*s the average of the reaction-times of the two-category plan were shorter than those for the three-category plan. Kellogg concluded that "the judgment times in the three-category series average throughout about 10% larger than those in the two-category series. The increase in time is explained as a function of the greater subjective difficulty of being prepared for three possible alternatives in the one case as against the relatively simpler preparation of two alternatives in the other."[4] Unfortunately for our present purpose, Kellogg in no place gives the reaction-times for each of the categories in his three-category plan.

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[*] Accepted for publication April 30, 1932.

[1] W. N. Kellogg, The time of judgment in psychometric measures, this Journal, 43, 1931, 65-86.

[2] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 196-206.

[3] S. S. George, Attitude in relation to the psychophysical judgment, this Journal, 28, 1917, 1-37.

There can be no doubt that the 'equality' judgment has been open to suspicion since the beginning of psychophysics. Much of this suspicion has a purely introspective basis, the 'equal' judgments seemed less positive, slower, and more frequently required confirmation than the judgments of difference.[5] Martin and Müller's and George's experimental findings seem to definitely confirm the introspective findings with regard to the slower relative speed of the formation of the 'equal' judgment.

Some time ago the senior author presented results which indicated the possibility of employing the 'absolute method' in psychophysics.[6] The classical method has been to present to the *O*s stimuli in pairs—a standard stimulus and a comparison stimulus. In the 'absolute method' only single stimuli are presented to the *O* and for each stimulus he is instructed to give a judgment in terms of the three categories, 'less,' 'intermediate,' and 'heavy.' It was found that if completely naïve *O*s are given such an absolute series normal curves of the psychometric functions are obtained for all three categories as the basis of the calculation of limens, although there is less precision of judgment and greater intervals of uncertainty than one finds in the usual relative series. These findings were confirmed by Davies and Babb.[7]

It seemed worth while to determine the reaction-time relations of the formation of judgments of the three categories of judgment by the 'absolute method' to discover whether there is any relation between the time relations for the absolute method and those which had been previously found for the three-category plan for the 'relative method.' The relatively larger number of 'intermediate' judgments for the absolute method as compared with the number of 'equal' judgments for the relative method (as evidenced by the greater magnitude of the interval of uncertainty) seemed a favorable condition for the present study. The importance of the present study has its basis in the acceptance of the criterion of reaction-time difference as the basis for the retention or elimination of categories of judgment in the hands of both George and of Kellogg.

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[4] Kellogg, *op. cit.*, 85.

[5] For a detailed review of the literature concerning the equality judgment, cf. S. W. Fernberger, The use of equality judgments in psychophysical procedures, *Psychol. Rev.*, 37, 1930, 107-112.

[6] Fernberger, On absolute and relative judgments in lifted weight experiments, this JOURNAL, 43, 1931, 560-578.

[7] E. Davies and W. W. M. Babb, Concerning the absolute method in psychophysics, this JOURNAL, 44, 1932, 367-369.

METHOD

Lifted weights were employed in the present study. Fourteen aluminum cylinders were cast, and drilled and filed to the proper weight. They varied, in the regular series, from 88 to 112 grm. by intervals of 4 grm. These stimulus-objects were placed about the circumference of a circular table with a revolving top so that each one could be successively brought under the reacting hand of *O.*

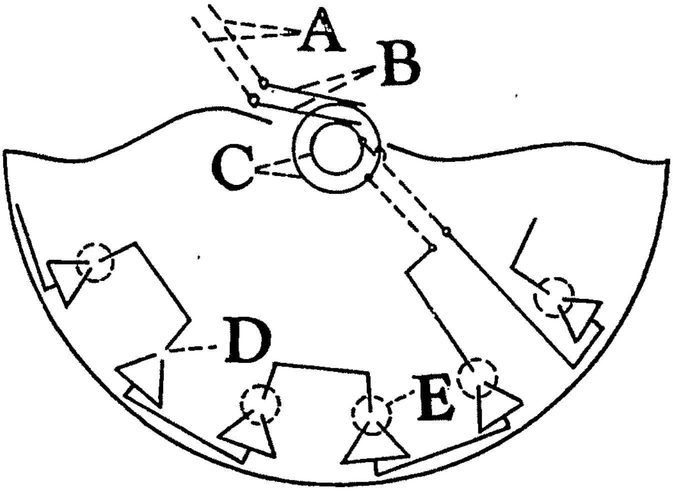

FIG. 1. DETAIL OF WIRING OF STIMULUS-TABLE
A, wires to chronoscope; B, brushes beneath table; C, collection rings beneath table; D, gap in wiring; and E, gap in wiring closed with weight in position.

Two series were employed, each carefully arranged in order to reduce the effects of interserial expectation. The order was changed frequently in order to eliminate the possibility of learning the series.[8]

These metal weights rested on copper wires fastened to the surface of the table and these wires were connected with a Dunlap Chronoscope.[9] The wiring arrangement was such that the lifting of any weight opened a gap which the weight bridged when it was on the table and so actuated the chronoscope. After considerable experimentation a bridge contact of the form shown in Fig. 1 was adopted. The wiring will be observed to be discontinuous, forming a gap which corresponded to the position on the table of every weight so that the weights closed the gap made by the three wires. The current passed through collection rings underneath the table. In order that the top of the table might swing freely it was necessary to take the current to and from the collection rings by means of brushes and these were wired directly to the chronoscope. Hence the circuit was closed only when all of the weights covered the gaps and the lifting of any one of the 14 weights would break the circuit and actuate the chronoscope.

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[8] Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 126-150.

[9] This experiment was made possible by a grant from the Faculty Research Committee of the University of Pennsylvania for the purchase of the Dunlap chronoscope and accessory apparatus.

The advantage of this three-point contact is that any weight may be moved about the surface of the table to a considerable extent and still rest on two of the three essential wires. We first tried a series of parallel wires but found that a relatively small displacement of the stimulus led to a tilting of the weight which caused a poor contact. Substitution of nickel plated parallel brass bars for the parallel wires gave no better contact and then the three-point contact described above was adopted.

Thus the chronoscope was started automatically by the lifting of a weight and it was hoped that the chronoscope might be stopped by having the judgment verbalized into a voice key. Preliminary experiments indicated, however, that this was not feasible due to the fact that the *Os*, in a long series of judgments, characteristically allow the head to drop so that the chin rests on the chest and so they fail to maintain a constant spatial relation between the mouth and the voice key. Furthermore it might be suspected, in the absence of either positive or negative evidence, that there might be differences in actuation-time of the voice key by the different words of the various categories of judgments and, if this were true, it would be the basis of a constant error. We therefore discarded the voice key and, throughout the experiments here reported, employed a pneumatic key actuated by pressing a rubber bulb for stopping the chronoscope.

The constant speed motor of the Dunlap chronoscope was run on commercial A.C. 60 cycle current. This current proved to be of such cycle constancy as to be within the limits of sensitivity of the chronoscope.[10] Thus, running on this current each unit of the chronoscope dial had a time value of 1/1200 sec.

The weights were presented successively to the *Os* except as noted below.[11] In order to allow time for the reading of the chronoscope it was not possible to have the lifting done continuously and controlled by the beats of a metronome in the usual manner. In the present experiment, each lifting was a disparate affair. *E* said "Ready, now, one, two, three, four" at a rate maintained as accurately as possible at 92 per min. On the signal "Ready, now" *O* raised his hand; on "one" the hand was lowered and the weight grasped; on "two" the weight was lifted; on "three" the weight was replaced on the table and on "four" the empty hand was lifted.

The *Os* were given the following instructions at the beginning of each experimental period:

> You will be presented with a series of weights which vary only slightly one from another. Some are light, some are heavy, and some are intermediate. Immediately after the lifting of a weight, you will report whether it was light, intermediate, or heavy. The form of your report will consist primarily of the

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[10] Fernberger, The accuracy of phase cycles in a commercial electric current, this JOURNAL, 44, 1932, 582-583.

[11] For a detailed description of the manner of lifting the weights cf. S. W. Fernberger, On the relation of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 13, 1913 (no. 61), 6-11.

pressing of the bulb. It is immaterial whether or not you verbalize your judgment simultaneously with the pressing of the bulb or subsequently.

In spite of the last part of these instructions, however, every *O* adopted the method of verbalizing the judgment simultaneously with the pressing of the bulb.

Four *Os*, all graduate students in psychology at the University of Pennsylvania, completed a long series of judgments during the autumn and winter of the academic year, 1931-1932. Those who took part were Mr. Leon Arons (*A*), Mr. Emil E. E. Folgmann (*F*), Mr. Winslow N. Hallett (*H*), and Mr. Malcolm G. Preston (*P*). *A* and *H* were highly practiced in lifted-weight procedures, and *F* and *P* were entirely without practice. None of the *Os* had practice in the formation of judgments by the absolute method before the present experiment.

A total of 12,950 judgments were obtained in the entire experiment. For *F*, *H*, and *P* 10 complete series of 350 judgments each were obtained. For *A* only 7 complete series of 350 judgments were obtained. At the end of the seventh series *A* suffered a prolonged illness and it seemed inadvisable to complete his data after the long interval.

The judgments and the reaction-times were both recorded in such a way that complete identification and correlation of the two sets of data was possible. The senior author throughout the experiment presented the weights to the *Os* and recorded the judgments and the junior author throughout the experiment read the chronoscope.

The chronoscope was read throughout to the exact unit of the dial. For purposes of calculation the reaction-time readings were treated by grouping to the nearest 10. The calculations were made in terms of the units of 1/1200 sec. and were transposed to sigma units only at the end of the calculations. The limens were treated in accordance with Urban's treatment of the results for the method of constant stimuli.

RESULTS

Limens. Although our primary interest in the present study is in the reaction-times for the formation of the judgments of the different categories, it seems worth while to include a discussion of the limens and other significant values of the psychometric functions for the sake of completeness because we have still so little information regarding the absolute method. These values will be found in Table I. In the first two columns will be found the values of the indices of precision (h_1 and h_2) for the curves of the psychometric functions of the light and heavy judgments respectively. In the next two columns will be found the values of S_1 and S_2, the lower and upper limens respectively. In the next column are entered the values of the intervals of uncertainty (I. of U.) and in the last column the points of symmetry (P. of S.).[12] In each row will be found the

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[12] We have, as the result of correspondence with Professor E. G. Boring, adopted this term for that value of stimulus corresponding to the intersection of the curves of the psychometric function of the 'less' and 'great' judgments. The older term, "point of subjective equality," is not, of course, applicable when a standard stimulus is not used.

TABLE I

SHOWING FOR EVERY *O* AND FOR EVERY SERIES OF EXPERIMENTS THE UPPER AND LOWER LIMENS, THE INDICES OF PRECISION, THE INTERVAL OF UNCERTAINTY, AND THE POINT OF SYMMETRY

O	Series	h_1	h_2	S_1	S_2	I. of U.	P. of S.
A	1	0.097	0.104	93.54	101.25	7.71	97.54
	2	0.086	0.096	93.49	102.15	8.66	98.05
	3	0.056	0.086	92.00	102.16	10.16	98.18
	4	0.079	0.070	90.84	99.98	9.14	95.14
	5	0.088	0.115	90.16	99.40	9.24	95.39
	6	0.136	0.102	91.18	98.54	7.36	94.34
	7	0.100	0.094	93.25	102.16	8.91	97.55
	Av.	0.092	0.095	92.07	100.81	8.74	96.60
F	1	0.109	0.076	91.04	103.26	12.22	96.06
	2	0.095	0.087	90.16	103.09	12.93	96.32
	3	0.117	0.076	92.91	106.67	13.76	98.33
	4	0.105	0.093	93.03	193.79	10.76	98.10
	5	0.095	0.094	93.27	105.24	11.97	99.21
	6	0.054	0.066	90.78	106.86	16.08	99.62
	7	0.086	0.095	92.21	105.01	12.80	98.95
	8	0.091	0.094	89.26	103.89	14.63	96.66
	9	0.101	0.072	91.70	105.43	13.73	97.40
	10	0.103	0.066	91.51	194.86	13.35	96.74
	Av.	0.096	0.082	91.59	104.81	13.22	97.74
H	1	0.073	0.058	92.48	103.03	10.55	97.19
	2	0.090	0.087	93.08	102.34	9.26	97.62
	3	0.048	0.071	88.57	103.43	14.86	97.38
	4	0.060	0.065	90.97	103.21	12.24	97.36
	5	0.092	0.060	93.13	105.60	12.47	98.02
	6	0.098	0.080	92.63	104.55	11.92	97.98
	7	0.079	0.081	91.98	104.09	12.11	98.07
	8	0.092	0.076	93.59	104.19	10.60	98.38
	9	0.083	0.092	94.76	105.05	10.29	99.83
	10	0.068	0.086	94.75	107.57	12.82	101.89
	Av.	0.078	0.076	92.59	104.31	11.71	98.37
P	1	0.087	0.092	90.64	102.16	11.52	96.56
	2	0.122	0.104	92.24	104.39	12.15	97.84
	3	0.091	0.110	92.13	106.57	14.44	100.03
	4	0.065	0.077	87.21	104.96	17.75	96.85
	5	0.080	0.082	91.80	107.84	16.04	99.92
	6	0.089	0.070	96.67	111.74	16.07	102.75
	7	0.087	0.099	95.23	109.18	13.95	102.65
	8	0.123	0.191	96.28	107.47	11.19	103.09
	9	0.109	0.178	99.33	107.43	8.10	104.36
	10	0.123	0.149	98.45	108.84	10.39	104.14
	Av.	0.098	0.115	93.90	107.58	13.16	100.82

values for each fractionated series in the order in which they were taken and in the last rows will be found the average values for the entire experiment.

The magnitudes of all of these significant values correspond to those found by Fernberger[13] and by Davies and Babb.[14] The intervals of uncertainty are invariably larger than those obtained by the relative method. For *A*, *F*, and *H* the values of the point of symmetry are lower than the central value of the stimulus-series (except for the value for Series 10 of *H*). An anomalous situation is found in this respect for *P*. For him 6 of the 10 values of the point of symmetry (and the average for the entire experiment) are higher than the middle stimulus of the series employed. A study of the values for the point of symmetry of this *O* indicates a shift from a negative to a positive error as the result of progressive practice. We have no ready explanation to offer for this anomalous situation with regard to the error of the point of symmetry for *P*.

TABLE II

SHOWING FOR EVERY *O* THE FREQUENCY OF OCCURRENCE OF THE DIFFERENT CATEGORIES OF JUDGMENT

O	Light	Intermediate	Heavy
A	618	672	1160
F	816	1466	1218
H	973	1238	1289
P	1083	1488	929

Reaction-times. In Table II will be found the distribution of the frequencies with which the judgments of the different categories were given by each of our *O*s. It will be noted that these frequencies vary among them. *A*, *F*, and *H* give more judgments of heavy than light—a fact evidenced by the negative error in the points of symmetry discussed above. Also, as one would expect from the positive error of the point of symmetry, *P* reported more judgments of light than of heavy. One finds that *F* and *P* gave more judgments of intermediate than they did for either of the extreme categories, while *A* and *H* gave more intermediate judgments than they did judgments of light but not so many as of heavy.

We first calculated the reaction-times for the entire experimental data for each *O*. These results will be found in Table III. Two rows are given to the results of each *O*. In the first row, in the appropriate column, will be found the average reaction-times for the three categories of judgment, while in the same columns in the row immediately beneath will be found the probable errors of these averages.

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[13] Fernberger, *op. cit.*, this JOURNAL, 43, 1931, 560-578.
[14] Davies and Babb, *op. cit.*, 367-369.

It will be observed that the average reaction-times of the intermediate judgments for *F*, *H*, and *P* are larger than for those of either light or heavy judgments. For *A* the average reaction-time for intermediate is very slightly longer than that for light and equal to that for the heavy judgments. It will also be observed that the reaction-times for judgments of light and heavy are very similar for all the *O*s. For *A*, *H*, and *P* the averages for the light judgments are smaller than those for the heavy, while for *F* the reaction-times for heavy are somewhat smaller than for the light judgments.

TABLE III

Showing for Every *O* the Average Reaction-Time and P.E. for Every One of the Different Categories of Judgment

(Times in sigma)

O		Light	Intermediate	Heavy
A	Av.	328	334	334
	P.E.	95	84	86
F	Av.	864	909	860
	P.E.	156	153	163
H	Av.	609	673	639
	P.E.	120	140	122
P	Av.	1510	1545	1520
	P.E.	104	118	122

The other marked characteristic of this table is the large magnitude of the probable errors throughout for every *O* and for all of the categories of judgment. There seems to be no regularity of the order of magnitude of the probable errors for the different *O*s. For *A* the order of the probable errors from smallest to largest is intermediate, heavy, and light; for *H* it is light, heavy, and intermediate; for *F* it is intermediate, light, and heavy; and for *P* it is light, intermediate, and heavy. Each of the probable errors for each of the three categories of judgment are thus found sometimes as lowest, sometimes as highest, and sometimes in an intermediate position. In no case are the differences in the size of the probable errors for the three categories for any *O* other than relatively small.

The differences between the average reaction-times are given in Table IV. For *A* the difference in average reaction-times are very small throughout. For *F* there is a small difference between the times for the light and heavy judgments and much larger and almost equal differences between the reaction-times for the light-intermediate and heavy-intermediate comparisons. For *H* the differences are larger on the whole than those for the other *O*s. The difference between the times for light and heavy and for heavy and intermediate are of similar magnitude and the difference between the times for light and intermediate is about twice that of the other two comparisons. For *P* the order from smallest to largest difference in reaction times is light-heavy, heavy-intermediate, and light-intermediate.

TABLE IV

SHOWING FOR EVERY *O* THE DIFFERENCES AMONG THE REACTION-TIMES OF THE VARIOUS CATEGORIES OF JUDGMENT

O	Light Heavy	Light Intermediate	Heavy Intermediate
A	6	6	0
F	4	45	49
H	30	64	34
P	10	35	25

TABLE V

SHOWING THE SIGNIFICANCE OF THE DIFFERENCES GIVEN IN TABLE IV

O	Light Heavy	Light Intermediate	Heavy Intermediate
A	0.027	0.027	0.000
F	0.011	0.113	0.118
H	0.171	0.187	0.097
P	0.118	0.032	0.097

In general, then, the differences in reaction-times are smallest for the light-heavy comparison and larger for the light-intermediate and heavy-intermediate comparisons but the relation is not as clear cut as one might expect from analogy with the results of George and of Kellogg.

It seems worth while to apply some criterion of the significance of a difference and for this purpose we have chosen the values of P, the values for which will be found in Table V. These values give the probability that the difference will not vary by an amount greater than itself and they may therefore be considered as an index of significance of the difference. They are dependent upon a ratio between the difference and the probable error of the difference and are found in a special table in Johnson.[15] In the first column of this table are given the values of P_x for the difference between the average reaction-times of light and heavy judgments for each *O* for the entire set of experimental data and in the next two columns, respectively, the values of P_x for the differences between the judgments of light and intermediate and of heavy and intermediate. It will be observed that on the basis of this criterion none of the differences are at all statistically significant. Indeed, a closer examination of the table indicates that the differences between the light-heavy reaction-times are no more significant than those between light-intermediate or heavy-intermediate.

It has seemed worth while to fractionate the results in terms of each series of 350 judgments in the order in which they were taken, and to calculate the averages and probable errors for each fraction and for every *O*. These values will be found in Table VI.

An examination of Table VI indicates, for *A*, that there is a tendency for the average reaction-times to decrease rapidly in size at the beginning of the experiment and then more slowly for all of the three categories of judgment. Also there is a very marked decrease in the size of the probable errors from the first to all subsequent series. After the first series the probable errors for this *A* remain very constant except for a more gradual decline through the second series for the heavy judgments. It will be remembered that *A* was highly practiced in lifted-weight procedure before the beginning of the present experiment.

The results for *F* are on the whole similar but show, in some respects, marked difference from those of *A*. Decreases in the size of the average reaction-times and in the size of the probable errors are found throughout. But the decreases in the size of the averages are much more marked for *F*—the averages for the last series being approximately one-half of those for the first series. Also the decreases in the reaction-times although rapid at first and slower later, continue to be observable throughout the entire experiment. One is led to believe that for *F* further experimentation would have led to further decrease in reaction-times, while in the case of *A* the reaction-times seem to be entirely stabilized long before the end of the experiment.

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[15] W. W. Johnson, *The Theory of Errors and Method of Least Squares*, 1892, Table II, 154.

TABLE VI

SHOWING FOR EVERY *O* THE AVERAGE REACTION-TIME AND P.E. FOR EVERY SERIES OF EXPERIMENTS FOR EVERY ONE OF THE DIFFERENT CATEGORIES OF JUDGMENT

(Time in sigma)

		Light		Intermediate		Heavy	
O	Series	Av.	P.E.	Av.	P.E.	Av.	P.E.
A	1	528	152	523	137	527	128
	2	306	38	343	48	347	82
	3	330	45	367	45	362	48
	4	242	40	256	36	259	38
	5	302	58	306	36	305	39
	6	292	32	288	33	294	32
	7	260	41	262	36	267	34
F	1	1362	175	1398	182	1338	153
	2	1032	75	1069	73	1007	85
	3	920	88	970	79	898	90
	4	864	67	918	68	881	89
	5	862	77	942	62	865	75
	6	782	70	858	76	764	68
	7	709	81	761	73	698	76
	8	754	59	768	45	706	51
	9	713	72	766	46	683	59
	10	655	69	700	70	638	68
H	1	892	150	1018	162	909	130
	2	741	99	862	104	746	91
	3	712	78	805	73	716	74
	4	650	67	729	89	683	84
	5	613	82	681	93	632	90
	6	528	62	578	72	550	68
	7	512	47	520	59	512	52
	8	479	68	528	60	467	51
	9	499	72	558	73	533	70
	10	512	49	548	68	533	65
P	1	1379	142	1318	119	1314	121
	2	1687	101	1688	90	1658	88
	3	1715	71	1728	92	1722	88
	4	1601	103	1652	96	1607	88
	5	1615	69	1607	84	1546	74
	6	1450	69	1472	81	1414	72
	7	1398	71	1437	70	1420	83
	8	1442	58	1500	58	1539	68
	9	1453	49	1452	51	1456	48
	10	1488	52	1488	58	1498	48

For *F* there is a marked decrease in the size of the probable errors which, however, seems to have reached a low level by the end of the experiment.

The results for *H*, an *O* also trained in lifted-weight technique, conform rather well with those of *F*. There is a decrease in the size of the average reaction-times and of the probable errors for all three categories of judgment. These decreases are great at first, less later, and seem to continue well toward the end of the experiment, where the values tend to level off and become stabilized at about the Series VII.

The results for *P* more or less conform to those of the other 3 *O*s. *P* was entirely untrained in lifted-weight technique at the beginning of the present experiment. His results indicate a marked reduction in the sizes of the probable errors, relatively rapid at first and then proceeding more slowly. If one should eliminate the average reaction-times for the first two series, the results for *P* in the last eight series entirely and completely confirm those of the other *O*s. For the last eight series, the average reaction-times start relatively high and at first rapidly and then more slowly decrease in magnitude. The reaction-times for all three categories of the first series, however, are smaller for *P* than those for any subsequent series. For Series 2 they are all considerably higher but not so large as the reaction-time for Series 3. We deemed it unwise to question this *O* during the experiment as to the possible basis of this anomalous result. Questioning after the experiment was completed elicited no positive explanation. We suggested the following explanation to this *O* to which he agreed but without any high degree of assurance: at the beginning he approached the experiment with a naïve stimulus attitude which tended to give relatively rapid reaction-times for the formation of his judgments and that, after the first day's experimentation, he assumed a more critical attitude which tended to increase the length of the reaction-times. As a bit of confirmatory evidence, we noted that in Series 2 *P* volunteered the information that "the formation of these judgments is not as easy as I thought."

Except for the anomalous first two series the rest of *P*'s results and all of the results for the other *O*s are in substantial conformity. The reaction-times are relatively slow at first and, with progressive practice, decrease rapidly in size and then more slowly. For all *O*s the probable errors are at first large and, with progressive practice, drop at first rapidly and then more slowly. The initial drop for the

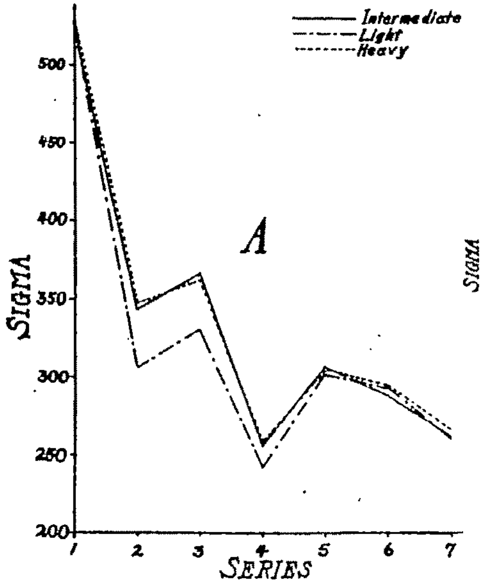

FIG. 2. CURVES FOR *A*.

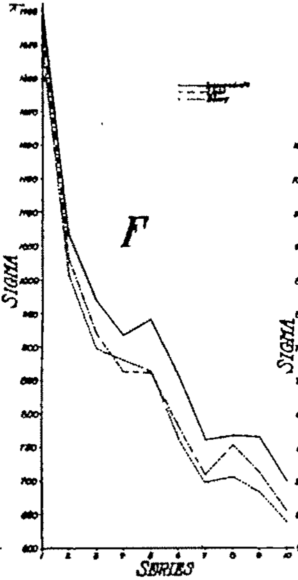

FIG. 3. CURVES FOR *F*.

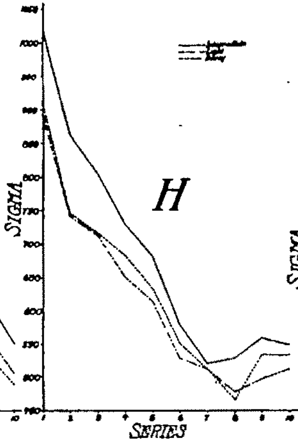

FIG. 4. CURVES FOR *H*.

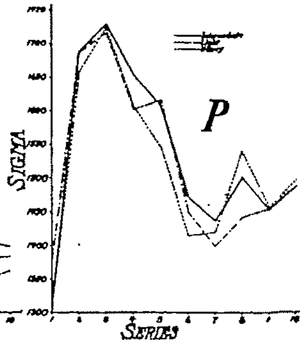

FIG. 5. CURVES FOR *P*.

probable errors is more rapid than that for the reaction-times and the values of the probable errors stabilize at a relatively low level earlier in the experimental series.

For the convenience of comparing the relations of the reaction-times of the different categories of judgment for the fractionated series, we have thrown the reaction-time results of Table VI into the form of curves shown in Figs. 2–5. The curves for each *O* are given in separate figures. It will be observed that the curves for all the *O*s are relatively of the parabolic form typical of one type of the usual learning curve, except for the anomalous results of Series 1 and 2 for *P*. The relative steepness of the curves for the different *O*s, discussed above, becomes more evident in this graphic form.

The primary problem of the present study, however, concerns the relative size of the reaction-times for the different categories of judgment, and, as it turns out, at different stages in progressive practice.

For *A* (Fig. 2) all three curves are almost identical throughout. For all 7 fractions, the curves for intermediate and heavy judgments may be considered identical. In two fractions, Series 1 and 2, the reaction-times for light judgments are slightly smaller than those for the other two categories. For the other 5 fractions, Series 3–7, the curve for light judgments very closely corresponds to those of the other two categories.

For *F* (Fig. 3) the curves are very similar in form and throughout are parallel to one another. The reaction-times for the intermediate judgments are slowest for every fraction. The times for the light and heavy judgments are very similar. In 8 of the 10 fractions, the reaction-times for heavy are slightly smaller than those for the light judgments.

For *H* (Fig. 4) it is evident that the reaction-times for intermediate judgments are invariably larger than those for the other two categories for all 10 fractions. All three curves are, however, very similar in form and are relatively parallel throughout. There is especially great similarity between the curves for the light and heavy judgments. In 8 of the 10 fractions the curve for the light judgments is lower than that for the heavy judgments.

The curves for *P* (Fig. 5) are again all very similar for all categories of judgment. The different curves cross one another so frequently that we are unable to note any special tendencies as in the cases of the curves for *F* and *H*. One must conclude that the curves for *P* are similar to those of *A* in that examination discloses no constant differences of any sort.

TABLE VII

SHOWING FOR EVERY *O* THE DIFFERENCES AMONG THE REACTION-TIMES OF THE VARIOUS CATEGORIES OF JUDGMENT FOR EVERY SERIES OF EXPERIMENTS AND THE SIGNIFICANCE OF THOSE DIFFERENCES

		Light-Heavy		Light-Intermediate		Heavy-Intermediate	
O	Series	D	P_x	D	P_x	D	P_x
A	1	1	0.000	5	0.011	4	0.091
	2	41	0.238	37	0.319	4	0.022
	3	32	0.254	37	0.304	5	0.043
	4	17	0.160	14	0.134	3	0.032
	5	3	0.027	4	0.032	1	0.011
	6	2	0.032	4	0.038	6	0.070
	7	7	0.070	2	0.048	5	0.016
F	1	24	0.954	36	0.081	60	0.123
	2	25	0.123	37	0.187	62	0.294
	3	22	0.091	50	0.223	72	0.314
	4	17	0.081	54	0.294	37	0.176
	5	3	0.016	80	0.415	77	0.406
	6	18	0.102	76	0.378	94	0.478
	7	11	0.059	53	0.254	63	0.314
	8	48	0.324	14	0.102	62	0.465
	9	30	0.171	53	0.324	83	0.542
	10	17	0.097	45	0.244	62	0.334
H	1	17	0.043	126	0.299	109	0.274
	2	5	0.016	121	0.434	116	0.429
	3	4	0.016	93	0.438	89	0.434
	4	33	0.166	79	0.368	46	0.197
	5	19	0.086	68	0.284	49	0.197
	6	22	0.129	50	0.279	28	0.155
	7	0	0.005	8	0.059	8	0.054
	8	12	0.081	49	0.284	61	0.368
	9	36	0.197	61	0.309	25	0.123
	10	21	0.144	36	0.233	15	0.081
P	1	65	0.187	61	0.176	4	0.011
	2	29	0.118	1	0.005	30	0.129
	3	7	0.032	13	0.059	6	0.027
	4	6	0.022	51	0.192	45	0.187
	5	69	0.363	8	0.043	61	0.289
	6	36	0.192	22	0.107	58	0.279
	7	22	0.097	39	0.208	17	0.081
	8	97	0.538	58	0.368	39	0.223
	9	3	0.022	1	0.011	4	0.032
	10	10	0.075	0	0.005	10	0.075

In order to determine the statistical validity of these differences we have calculated the values of P_x for each difference for every fraction of the results of all four *O*s. These values are given in Table VII. Two columns are given to the results in order for the differences light-heavy, light-intermediate, and heavy-intermediate. In the first column of each pair will be found the values of the differences and in the second column the values of P_x for that difference.

For *A* the differences are small with the exception of 4 values which occur in two series (light-heavy and light-intermediate of Series 2 and 3). These larger values of difference are due to variations of reaction-times of the light judgments. All of the other differences are extremely insignificant and this is especially true, throughout, for the differences between the heavy and the intermediate judgments. The probable errors being large all of the values of P_x are so small as to be entirely without statistical significance.

For *F* all of the differences are considerably larger than those for *A*. The averages for the three differences for the three comparisons are of considerable magnitude. These averages are: light-heavy, 21.5; light-intermediate, 49.8; and heavy-intermediate, 67.2. These results are what one might expect from a consideration of the curves of Fig. 3, which indicates that invariably the reaction-times for all fractions were largest for the intermediate judgments, and in 8 of the 10 fractions the values for heavy judgments were less than those for light. The values for P_x, however, indicate that when the probable errors, which were large for this *O*, are taken into account that the differences are not of statistical significance. The largest value of P_x is 0.542 (heavy-intermediate, Series 9). Of the 30 values of P_x for this *O*, 7 are less than 0.1, 14 less than 0.2, 20 less than 0.3, 25 less than 0.4, and only 5 greater than 0.4.

For *H* the magnitudes of the differences are again relatively large and seem to show relatively great variability for the different comparisons. The averages for this *O* are light-heavy, 16.9; light-intermediate, 69.1; and heavy-intermediate, 54.6. It will be remembered that the curves for *H* (Fig. 4) indicate that the reaction-times for the light and heavy judgments were quite similar while those for the intermediate judgments were invariably longer for all fractions. It will be observed, however, that the differences between the reaction-times of the light-intermediate and heavy-intermediate comparisons are relatively large at first and tend to decrease with progressive practice. In no case, however, are the values of P_x of great statistical significance. The largest value of P_z is 0.438 (light-intermediate, Series 3). Of the 30 values of P_z, 9 are less than 0.1, 17 less than 0.2, 23 less than 0.3, 26 less than 0.4, and only 4 greater than 0.4.

The differences for *P* are on the whole relatively large but, like those of *A*, show no constant relationship. Indeed the differences for light-intermediate and heavy-intermediate are smaller than those for light-heavy as will be seen from the averages: light-heavy, 34.4; light-intermediate, 25.4; and heavy-intermediate, 27.4. The values of P_z for this *O* are again relatively or completely without statistical significance. The highest value of P_z is the anomalously large one of 0.538. Of the 30 values, however, 15 are smaller than 0.1, 23 smaller than 0.2, 27 smaller than 0.3, and only 3 larger than 0.3.

Thus for all *O*s there seems to be no general tendency of increase or decrease of the size of the values of P_z as the result of progressive practice. For only one *O*, *H*, is there a well-marked tendency for a decrease in the size of the differences with progressive practice. In this case there is also a corresponding decrease in the size of the probable errors so that no marked changes are to be noted in the values of P_z.

We wish to mention that we recognize the fact that for *F* and *H* the values of reaction-time for the intermediate judgments are invariable higher than those for either of the two other categories for all 10 fractions. Thus the probability that this difference is not due to chance is of the magnitude of $1:2^{10}$. In spite of this, when one applies some measure of the significance of the differences such as the values of P_z it would seem that the significance of the bare probability is minimized.

Progressive practice. The effects of progressive practice on the limens and other significant values of the psychometric functions for the relative method have been known for some time. Urban,[16] and Fernberger,[17] have shown that progressive practice tends to increase the values of the coefficients of precision of the psychometric functions of both the greater and less judgments; decrease the size of the interval of uncertainty and also decrease the magnitude of the point of subjective equality. Fernberger has also shown similar effects of progressive practice for the absolute method.[18] The present set of results reveals another effect of progressive practice hitherto unrevealed in the experimental literature of the reaction-times for the formation of psychophysical judgments to our knowledge. We find a marked and relatively regular decrease in the reaction-times for all three categories of judgment for the absolute method. As revealed in Table VI and in Figs. 2–5, the decrease is at first relatively rapid and then slower until a level is reached. Hence these curves are of the relatively parabolic form so familiar with one type of the usual learning curve. Wide individual differences are observable, as one would expect, in the size of the average reaction-times at both the beginning and at the end of the practice period. Also individual differences are to be observed in the steepness of the curves. These individual differences in the steepness of the curves can be explained for 2 of our *O*s in a manner which fits entirely with the expected results. The curves for *A* show the least decrease and it will be remembered that this *O* was the most highly practiced in the lifted-weight technique at the beginning of the present experiment. On the other hand the curves for *F* show the greatest amount of decrease as the result of progressive practice and this *O* was one of the two entirely unpracticed *O*s at the beginning of the experiment. The steepness of the curves for *H* (practiced) and for *P* (unpracticed) are both intermediate to those of the other two *O*s. Whether the curve for the unpracticed *P* would have shown a greater amount of decrease if it had not been for his anomalous results in the first two fractions is something about which we have no information. With regard to these early series for *P* it is interesting to note that the values of his intervals of uncertainty (Table I) show a course more or less similar to his reaction-time curves. The values of the intervals of uncertainty start low, increase considerably in magnitude during the first three series and then decrease considerably but not regularly for the rest of the experiment.

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[16] F. M. Urban, Der Einfluss der Übung bei Gewichtsversuchen, *Arch. f. d. ges. Psychol.*, 29, 1913, 271-311.

[17] Fernberger, The effects of practice in its initial stages in lifted weight experiments and its bearing upon anthropometric measurements, this JOURNAL, 27 1916, 261-272.

Discussion

It had been shown by George that there were considerable differences in reaction-times for judgments of different categories by the relative method in psychophysics. On the basis of these, as at least one criterion, it was argued that the judgments of 'less,' 'equal,' and 'greater' might properly be retained in psychophysical procedures but that other types of 'guess,' 'doubtful,' and 'or' judgments should be eliminated. George also found that the average reaction-times for the three retained categories of judgment varied considerably from one another—that for less being fastest, that for greater in between and that for equal decidedly slower than the other two. It may be pointed out that this is what might have been expected for lifted-weights if the introspective analyses of the formation of judgments in terms of motor set is to be accepted as described by Martin and Müller[19] and by Fernberger.[20] In the presence of such a motor set, occasioned by the lifting of the standard stimulus, the basis for the less judgment in lifted-weights is found to be that the comparison weight goes up easily (seems to fly up) from the table. The basis for the heavy judgment is that the anticipatory motor set is not sufficient to lift the weight readily or that the weight goes slowly from the table and not so high. On the basis of this analysis one would expect the less judgment to have a slightly shorter reaction-time than the greater judgment. Both Martin and Müller and Fernberger found that, although judgments of absolute equality were obtained, the equal judgment was frequently a negative affair of "no difference" rather than of actual equality and further that such judgments very frequently were followed by a subsequent verification process before the judgment was reported. It is obvious then that the equal judgment should have a longer reaction-time than either of the two difference categories inasmuch as it seems to be of a less immediate character. Fernberger has shown that this difference in the nature of the formation of equal judgments and those of difference judgments is due to the fact that the normal instructions seem to set up an attitude in the mind of the *O*, of looking for difference.[21]

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[18] Fernberger, On absolute and relative judgments in lifted-weight experiments, *ibid.*, 43, 1931, 560–578.

Kellogg has gone still further and, on the basis of the criterion of reaction-times, has recommended the acceptance of a two-category plan with the inclusion of judgments of less and greater only rather than a three-category plan which includes the classical three categories of judgment of less, equal, and greater. Kellogg found that the massed reaction-times under the two-category plan were smaller on the average than the massed reaction-times under the three-category plan. Unfortunately for our present purpose Kellogg does not give the results of the reaction-times for each of the different categories within the three-category plan.

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[19] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 1-96.

[20] Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, (no. 6), 1-161, esp. 155 f.

[21] Fernberger, The effect of the attitude of the subject upon the measure of sensitivity, this JOURNAL, 25, 1914, 538-543; and Instructions and the psychophysical limen, *ibid.*, 43, 1931, 361-376.

The present writers are not inclined to accept without question the validity of the criterion of the reaction-time as indicating the validity of one or more categories of judgment and the invalidity of others. In the case of George's results, we believe that there are too few observations for the average to be of any great statistical value. A total of 2529 reaction-times were obtained in all from 5 *Os* and these are distributed among 13 different categories of judgment. A total of 697 cases is the largest for any category of judgment from all 5 *Os*. Furthermore, George publishes no probable errors or any statistical values of the significances of these differences. The present results would indicate that the probable errors of George's averages would have been large and this would tend to decrease the significance of the differences, of course. Similarly Kellogg's results, although more numerous, are treated in such a way as to fail to give the basis of comparison of the reaction-times of the categories of judgment in the three-category plan.[22]

If, however, the reaction-time differences are a valid criterion for the elimination or retention of categories of judgment in psychophysics, the present results would indicate that, for the absolute method, the choice of categories of 'light', 'intermediate,' and 'heavy' are all capable of being formed under a constant attitude and should all be retained. In no case are averages in reaction-times of these three categories significantly different for any *O* for the results of either the entire experiment or for any fraction as indicated by the smallness of the values of P_s. Our differences for the three categories for the absolute method are considerably smaller than those obtained by George for the three major categories of less, equal, and greater for the relative method.

In the case of only one *O* does the rank order of the reaction-times correspond to that of George. For *H* the rank order from the fastest to the slowest average reaction-times is light, heavy, and intermediate. The results for *F* correspond to those of George in the respect that the reaction-times for intermediate are longer than for either of the difference categories but, for this *O*, the times for the heavy judgments tend to be shorter than those for light. In the cases of the other two *O*s, *A* and *P*, there is no conformity with the results of George as the differences are not only very much smaller in the case of our experimental data but the times for the different categories vary one from another without any apparent general tendency.

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[22] We have tried to obtain the original data of the George and Kellogg experiments for purposes of further calculation. Professor K. M. Dallenbach of Cornell University informs us that George's data were destroyed a few years ago. Professor Kellogg is at present away from Indiana University. He writes that he believes that he can find his original data and that he will send them on his arrival at his University.

CONCLUSIONS

Obtaining the reaction times of the categories of 'light,' 'intermediate,' and 'heavy' judgments for lifted-weight stimuli for the absolute method in which immediate judgments are obtained for a series of slightly different stimuli but without standard stimuli, we find:

(1) There is a marked and definite shortening of reaction-times as a result of progressive practice. This decrease is relatively great at first and then proceeds more slowly giving a curve of the parabolic type. This reduction in reaction-time is completely similar for all three categories of judgment.

(2) There is a similar decrease in the size of the probable errors of the average reaction-times for all three categories of judgment as the result of progressive practice.

(3) For 2 of the 4 *O*s, although the reaction-times for the 'light' and 'heavy' judgments are very similar, those for the 'intermediate' judgments are invariably slightly longer than for either of the other two categories. For the other 2 *O*s, the average times for all three categories of judgment are invariably similar.

(4) If some criterion of the significance of a difference, such as the values of P_x, is obtained, the differences between the average reaction-times for the different categories of judgment are shown to be statistically without significance. This is true for the averages of all categories for either the entire experimental data as a whole or for any fraction of the experimental data and for every *O*.

(5) If reaction-times may be considered as criteria for the retention or elimination of categories of judgment in psychophysical procedures, the present results clearly and definitely indicate that the categories of 'light,' 'intermediate,' and 'heavy' are properly chosen categories for the absolute method and that they should all be retained in the use of this method.

WHOLE AND PART METHODS WITH VISUAL SPATIAL MATERIAL

By LELAND W. CRAFTS, New York University

The objects of the present experiment are (1) to determine the efficiency of whole and part methods for learning certain samples of visual spatial material, and (2) to secure further data regarding the influence of the material to be learned—its difficulty, unity, coherence—upon the efficiency of the learning.

The first object represents an attempt to investigate the whole-part problem with a type of material very different from any hitherto employed—with material which is not only non-verbal and non-serial, but which also is of the kind sometimes classified as perceptual rather than as motor. Perrin and Klein state that "so far as perceptual learning is concerned the results, in the case of learning to read, are decidedly against the part method" but "to what degree this finding applies to other forms of perceptual learning must be determined separately for each form."[1] One of these 'other forms' is exemplified by the visual diagrams used in the present experiment.

The second object represents a continuation of previous work of the writer.[2] His earlier experiments, together with other literature in the field,[3] suggest that the whole method may be expected to be more advantageous with easy rather than with difficult, and with related rather than with unrelated material. In the present experiment these propositions will be again tested in this new type of learning situation.

METHOD AND PROCEDURE

Subjects. The *S*s were 305 undergraduate men and women. In every one of the five experiments performed three groups of *S*s—each group a class in Psychology—were employed. Since the groups were the same in Experiments I, III, and V, the total number of different groups was nine. No attempt was made to equate the groups.

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* Accepted for publication, March 21, 1931.

[1] F. A. C. Perrin and D. B. Klein, *Psychology*, 1926, 280.

[2] L. W. Crafts, Whole and part methods with non-serial reactions, this JOURNAL, 41, 1929, 543-563; Whole and part methods with unrelated reactions, *ibid.*, 42, 1930, 591-601.

[3] L. A. Pechstein, Massed *vs.* distributed effort in learning, *J. Educ. Psychol.*, 12, 1921, 92-97; E. W. Sawdon, Should children learn poems in wholes or in parts? *Forum Educ.*, 5, 1927, 182-197; W. H. Winch, Should poems be learnt by school-children as 'wholes' or in 'parts'? *Brit. J. Psychol.*, 15, 1924, 64-79.

The data to follow will include the records only of *S*s who completed an experiment, *i.e.* who worked with a given type of material with all of the three learning methods used. The number of such *S*s was as follows:

Experiment	Group 1	Group 2	Group 3	Total
I	27	33	34	94
II	41	34	39	114
III	36	30	34	100
IV	29	35	27	91
V	32	26	28	86

Materials: (*1*) *Circles.* The diagram shown in Fig. 1 was drawn in black India ink upon a sheet of very heavy white cardboard 21⅝ × 29⅞ in. The 64 small squares measured 2.5 in. each. The circles, always 16 in number, were 1⅜ in. in diam.; they were cut from black cardboard, and were fastened by black-headed clips in the centers of 16 of the squares. They were arranged as irregularly as possible within the limits of the requirement that there should be 4 circles in each quarter of the field. Three of these arrangements, one of which is reproduced in Fig. 1, were used. The location of every one of the 16 circles was different in every arrangement.

(*2*) *Lines.* The diagram, shown in Fig. 2, was laid out upon heavy cardboard as before. The 49 points, 3 in. apart in 7 rows of 7 each, were the heads of brass clips painted black and 5/16 in. in diam. The lines, always 16 in number, were made by connecting pairs of points adjacent vertically, horizontally or diagonally with strips of black paper ¼ in. in width, which were inserted under and held down by the heads of the clips. These lines were arranged as irregularly as possible within the limits of the requirement that there should be 4 lines—one vertical, one horizontal, and one in each of the two possible diagonal directions—in every quarter of the field. Three such arrangements, one of which is reproduced in Fig. 2, were used. The location of every one of the 16 lines was different in every arrangement. The enclosure of two of the points with black circles was intended to facilitate the learning.

(*3*) *Figures.* The same outline of points as that used for the lines was employed, and the points and connecting lines were constructed in the same manner. Every figure was composed of 16 lines, arranged so as to form as irregular a design as possible. One quarter, *i.e.* 4 lines, of a figure was allotted to every quarter of the field. Three of these irregular figures, one of which is reproduced in Fig. 3, were used. They differed greatly in general shape, and the location of every one of the 16 constituent lines was different in every figure.

Instructions. The *S*s received instruction sheets, of which the following for the lines is a sample. Those for the other materials were similar.

> This is an experiment in learning. You will have displayed to you a diagram composed of a large number of black points symmetrically arranged on a white background. Certain pairs of adjacent points are connected by black lines—vertically, horizontally, or diagonally. Your task is to learn the exact location on the diagram of each of these lines. You will have only 120 sec. When the end of that period is announced, turn to the sheet, which you now have, containing an exact replica of the above diagram and reproduce thereon, in its exact location, every one of the black lines.

Presentation. The cardboards carrying the diagrams to be learned were fastened to an easel which was placed on a table in front of the class. The diagram was concealed between exposures by a large sheet of gray cardboard. When exposed it was presented (1) as a whole, (2) by parts, or (3) by combinations of parts. In the pure part method in which a quarter of the diagram was exposed at a time, presentation was always clockwise, *i.e.* upper left quarter (part 1), upper right (part 2), lower right (part 3), lower left (part 4). In the combination part method, used only with the lines and the figures, each 'part' consisted of half the diagram. These halves were presented similarly in a clockwise direction, *i.e.* upper half (quarters 1 and 2), right half (quarters 2 and 3), lower half (quarters 3 and 4), and left half (quarters 4 and 1). In the progressive part method, used only with the circles, the nature and sequence of the parts and their combinations are given in the footnote to Table I. This table also shows that in all the part methods—except the progressive part—every quarter or half was presented an equal number of seconds, and that the diagram was exposed as a whole for a brief period at the conclusion.

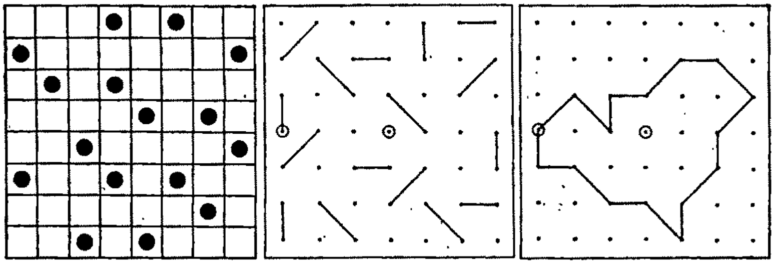

FIG. 1 FIG. 2 FIG. 3

FIGS. 1-3. SHOWING THE KINDS OF MATERIAL USED

The duration of presentation, in every experiment except Experiment IV, was such that the average reproduced score would be from 60-70% correct.

The *S*s were told at every trial what method of presentation would be used.

The diagram was exposed as a whole by removing the sheet of gray cardboard that served as the screen; it was exposed by parts—by quarters or halves—by using screens of gray cardboard from which sections had been cut of the desired size.[4]

Rotation of learning methods. In every one of the five experiments the following rotation of the learning methods was employed:

Trial	Group 1	Group 2	Group 3
1	Whole	Pure Part	Comb. Part
2	Pure Part	Comb. Part	Whole
3	Comb. Part	Whole	Pure Part

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[4] In Experiment I (circles) *E* placed these screens one at a time over the diagram, removing one and substituting another according to the prearranged plan. In the other four experiments (Experiments II-V) a screen was used that rotated upon a screw and nut device fixed in the center of the diagram and so contrived that it could be instantly removed when the diagram as a whole was to be exposed.

Hence with every kind of material every learning method was employed once in every one of the three possible temporal positions, and once with every one of the three different arrangements of circles and lines or the three different figures used.

Distribution of trials. The distribution of trials for all experiments except Experiment III was as follows: A preliminary trial with complete explanation and illustration. An interval of 5 days. The 3 experimental trials, one with each learning method, at intervals of one week. The distribution for Experiment III was identical with the above except that the 3 experimental trials were given at intervals of 2 days instead of one week.

TABLE I

SHOWING THE EXPOSURE TIMES USED IN EVERY ONE OF THE EXPERIMENTS

			Time in sec.		
Experiment	Material	Method	per part	for whole	total
I	Circles	whole		94	94
		pure part	22	6	94
		prog. part	*	6	94
II and III	Lines	whole		120	120
		pure part	28	8	120
		comb. part	28	8	120
IV	Lines	whole		94	94
		pure part	22	6	94
		comb. part	22	6	94
V	Figures	whole		35	35
		pure part	8	3	35
		comb. part	8	3	35

*In this method, the progressive part, the exposure times and sequence of the parts were as follows: part 1, 12 sec.; part 2, 12 sec.; parts 1 and 2 combined, 16 sec.; part 3, 12 sec.; parts 1, 2, and 3 combined, 24 sec.; part 4, 12 sec.; whole, 6 sec.

Reproduction. Graphical reproduction immediately followed the presentation. *Ss* were allowed as much time as they wished except that after 3 min. had elapsed they were urged to complete their work quickly.

Scoring. Every circle that was in the correct square, and every unrelated line or line that was part of a figure which connected the correct two points, was scored 1. The scoring was always of these constituent elements separately considered, and no part credits of any kind (as for accuracy in general form or pattern alone) were given.

RESULTS

Tables II and III show the whole method to have been reliably superior to either part method for learning the circles and the figures, but not so for the unrelated lines with either long or short interval of exposure.[5] Neither part method was reliably superior to the other with any kind of material. Table IV shows that with the circles and the figures a majority of the individual *S*s made their highest scores

TABLE II

SHOWING FOR ALL EXPERIMENTS THE AVERAGE NUMBER OF ELEMENTS CORRECTLY REPRODUCED AND THE S.D. OF THOSE AVERAGES

Experiment	Method					
	Whole		Pure Part		Comb. Part	
	Av.	S.D.	Av.	S.D.	Av.	S.D.
I Circles	12.1	3.15	10.2	3.36	10.6	3.01
II Lines	10.1	3.42	10.2	3.66	9.2	3.52
III Lines, short interval	11.6	3.14	11.7	3.62	10.6	3.44
IV Lines, short exposure	7.1	3.05	6.1	2.40	6.6	3.40
V Figures	12.1	3.79	10.2	4.12	9.8	4.20

TABLE III

SHOWING THE DIFFERENCES BETWEEN THE AVERAGES GIVEN IN TABLE II AND THE S.D. OF THOSE DIFFERENCES

Experiment	Comparison*	Diff.	$S.D._{Diff.}$
I Circles	Whole–Pure Part	1.9	0.474
	Whole–Comb. Part	1.5	0.445
	Pure Part–Comb. Part	−0.4	0.468
II Lines	Whole–Pure Part	−0.1	0.469
	Whole–Comb. Part	0.9	0.460
	Pure Part–Comb. Part	1.0	0.476
III Lines, short interval	Whole–Pure Part	−0.1	0.479
	Whole–Comb. Part	1.0	0.466
	Pure Part–Comb. Part	1.1	0.499
IV Lines, short exposure	Whole–Pure Part	1.0	0.479
	Whole–Comb. Part	0.5	0.576
	Pure Part–Comb. Part	−0.5	0.438
V Figures	Whole–Pure Part	1.9	0.601
	Whole–Comb. Part	2.3	0.609
	Pure Part–Comb. Part	0.4	0.629

*In every comparison the score of the second method named has been subtracted from that of the first. Hence a minus sign before a difference shows that the score for the second method was the greater.

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[5] This absence of any reliable difference between the three learning methods with the lines was the reason for performing three experiments with this particular material. It was thought possible that through variation in time of exposure or in length of interval some condition might be discovered under which one of the methods would become reliably superior to the others. But, as the data show, although decreasing the exposure time seems to have increased somewhat the effectiveness of the whole method, the attempt was unsuccessful.

with the whole method, whereas with the lines no method was superior or inferior for more than 50% of them.

Table V shows the influence of practice effect to have been definitely less with the circles and the figures than with the unrelated lines. It was therefore with the material where learning method seems to have been of least significance that the effect of practice alone, of positive transfer from one trial or diagram to another, most clearly revealed itself.

TABLE IV

Showing for All Experiments the Percentage of *Ss* for Whom Every Learning Method was Superior or Inferior

Experiment	Method					
	Whole		Pure Part		Comb. Part	
	Superior	Inferior	Superior	Inferior	Superior	Inferior
I Circles	53.8	21.0	23.6	46.2	22.6	32.8
II Lines	41.2	25.0	37.3	30.3	21.5	44.7
III Lines, short interval	42.9	24.5	42.4	26.5	14.7	49.0
IV Lines, short exposure	37.7	21.9	26.7	37.1	35.6	41.0
V Figures	56.1	15.0	18.3	42.5	25.6	42.5

TABLE V

Showing the Influence of Practice Effect by Giving the Average Scores for All Groups on the Three Experimental Trials, Learning Methods Being Disregarded

Experiment	Trial 1	Trial 2	Trial 3
I Circles	10.8	10.7	11.4
II Lines	8.7	10.1	10.7
III Lines, short interval	10.1	11.5	11.6
IV Lines, short exposure	5.5	6.7	7.3
V Figures	10.7	11.0	10.4

Discussion

(*1*) *The whole method.* As the data in Tables II, III, and IV show, the whole method was more effective with the circles and the figures than it was with the unrelated lines. Explanation of these differences in its value requires, we believe, consideration of the two factors of difficulty and relationships.

As to difficulty, an index thereof may be obtained by dividing the average reproduction score by the total exposure time, which will give the average score obtained per second of learning time. The results of such division are: Figures, 0.35; Circles, 0.13; Lines, 0.08; Lines (short interval), 0.09; Lines (short exposure), 0.08. The figures show that the whole method was more advantageous with the easier materials and much less effective with the more difficult. Since the learning in the present experiment was unspaced, such results are in principle harmonious with Pechstein's view that with a 'hard' problem the whole method with massed practice is apt to be at a particular disadvantage.[6]

The nature of these differences in difficulty was in part quantitative, even though the number of elements to be reproduced was with all materials the same, namely 16. For, as reference to the figures given earlier will show, while correct reproduction of a circle required the *S* to recall the location of only *one* square, reproduction of an unrelated line required him to recall the location of *two* separate points. With the figures it is of course evident that a given line could be recalled as an isolated line, or as a line joining another already reproduced, or as one connecting two lines previously localized, with corresponding differences in this purely quantitative aspect of difficulty.

But it would, we think, be generally agreed that these differences in the difficulty of our materials are not reducible to simple quantitative factors alone, but that they are due also to the more qualitative factor of relationships. It seems certain that the lines were hardest, not merely because their reproduction required localization of more elements, but also because they were the most difficult to relate, to organize into coherent patterns. Similarly the figures were easiest for one reason because such organization of elements into patterns was given in the material itself. In consequence we consider that our data distinctly favor the conclusion that, as is commonly supposed, the whole method may be expected to be more advantageous with the more related as well as with the easier material.

As to why the whole method was not decidedly more efficient with the figures than with the circles, we have no conclusive answer. It may be that, for reasons already given, the circles were quantitatively easier. It may also be that—as the unsolicited comments of certain *S*s suggest—they were unusually easy to organize and relate, and this in spite of our attempt to arrange them on the diagram in as irregular and unrelated a manner as possible. With reference to this second possibility it is worth pointing out that with material which is easy to organize, the relationships which *S* establishes for himself might produce patterns which would facilitate learning just as much as would patterns given objectively in the material itself. In the former case *S* creates his own forms, relates elements in ways most natural for him; but in the latter case he must for the most part accept the material as it is given to him and adjust himself to forms devised by the experimenter, regardless of the ease or difficulty which they chance to possess for himself.

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[6] Pechstein, *op. cit.*, 45.

(*2*) *The part methods.* As to why the two part methods used should have failed to yield reliably different results, we have no entirely adequate explanation. Theoretically one might suppose that since part methods of either the progressive or the combination type are intermediate in form between the whole and the pure part method, they should yield scores which would likewise be intermediate. In the one case, with the circles, in which we used a true progressive method it did occupy such a position (see Table II). But the combination method was thus intermediate in score only with the lines, short exposure. With the figures, and in both experiments with the lines, long exposure, it gave the lowest scores of all. Such results can scarcely be said to agree with those of Pechstein for mazes[7] and nonsense syllables,[8] of Reed for poetry and prose,[9] of Gopalaswami for mirror-drawing,[10] or of Crafts for card sorting[11] and for substitution tests with massed practice.[12] For in all these cases a progressive type of part method was found to be superior to the whole method, to the pure part method, or to both.[13] This divergence between our present results and those of previous experiments may be due to peculiarities of our visual-spatial material, which was very different from that used in any earlier study. It may also be due to the nature of the combination method which we used with all materials except the circles, and which was a kind of part method somewhat different from any hitherto employed. In this method, it will be recalled, the parts were halves composed of quarters successively recombined with each other. It may also be, as certain *S*s remarked, that this presentation of a quarter of the diagram in two different combinations, *e.g.* of the third quarter first with the second and then with the fourth, was 'confusing' in that it tended to force the elements of a given quarter into two different, and often interfering, sets of relationships. If such an effect was produced, the failure of the combination method to yield higher scores might of course be thereby explained.

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[7] Pechstein, Whole *vs.* part methods in motor learning: a comparative study, *Psychol. Monog.*, 23, 1917, (no. 99), 68, 76.

[8] Pechstein, Whole versus part methods in learning nonsensical syllables, *J. Educ. Psychol.*, 9, 1918, 381-387.

[9] H. B. Reed, Part and whole methods of learning, *ibid.*, 15, 1924, 107-115.

[10] M. Gopalaswami, Economy in motor learning, *Brit. J. Psychol.*, 15, 1925, 226-236.

[11] Crafts, Whole and part methods with non-serial reactions, *loc. cit.*

[12] Crafts, Whole and part methods with unrelated reactions, *loc. cit.*

[13] It might be noted, however, that J. W. Barton (Smaller *vs.* larger units in learning the maze, *J. Exper. Psychol.*, 4, 1921, 418-430) found a progressive inferior to a pure part method for learning mazes.

SUMMARY

The aims of the present experiment were (1) to determine the relative efficiency of whole and part methods for learning certain samples of visual spatial material, a kind not used in any previous investigations of the whole-part problem, and (2) to secure further data as to the influence upon their efficiency of differences in the difficulty of the materials learned and in the degree of unity and coherence which they possess. Three types of visual material—irregular arrangements of circles and of unrelated lines and irregular geometric figures—were employed. Every type was presented to three groups of *S*s by three methods, the whole, the pure part, and a combination or a progressive part. The total exposure times were brief, ranging from 35 to 120 sec.; partial learning only was secured with most of the *S*s; reproduction was immediate; scoring was of constituent elements only. The data show the whole method to have been reliably superior with the circles and the figures only; with the much more difficult unrelated lines, even with variations in the length of exposure and of interval, no method was reliably superior to any other. These results in general agree with theoretical expectation and with the results of experiments with other materials. They suggest (a) that in the field of visual 'perception' as well as in that of motor or of verbal learning, neither the whole nor any one form of a part method will invariably be superior; and (b) that the whole method can be expected to be especially advantageous with easier and with more closely related materials.

THE RELATION BETWEEN MUSCULAR TENSION AND QUALITY OF PERFORMANCE

By ELIZABETH DUFFY, Sarah Lawrence College

The relation between quality of performance and tension of the skeletal muscles has been the subject of a number of recent investigations. A review of these studies will not be attempted here; it will suffice to point out that the reports have included both the statement that increases in tension of the muscles resulted in increased efficiency of performance,[1] and the statement that the poorest performers in a given task were the *Ss* with greatest tension.[2] These statements are not necessarily incompatible, but they suggest the need for further analysis of the problem. Such an analysis is attempted in the present paper, which is based upon a series of experiments reported in part in an earlier publication.[3]

EXPERIMENTAL PROCEDURE

The *Ss*, it will be recalled, were 18 children varying in age from 2 yr., 11 mo. to 3 yr., 10 mo. (median, 3 yr., 3 mo.).[4] Graphic records were obtained of the pressure upon dynamographs held in each hand during a tapping performance and during discriminative reactions to 3 kinds of pictures (an automobile, a horse, and a street car), to only one of which the *Ss* were to respond by closing a reaction-key. Forty pictures were presented in random order at precisely 4-sec. intervals. This experiment was repeated 5 times with every *S*, within a period of 4 months.

Discriminative reactions were scored 'correct' when the reaction-key was closed at any time during the 4-sec. interval after the appearance of the picture

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The study was made possible through a fellowship from the Committee on Child Development of the National Research Council, and through the generous aid of Professor R. S. Woodworth, and of the Child Development Institute of Teachers College, Columbia University.

[1] A. G. Bills, The influence of muscular tension on the efficiency of mental work, this JOURNAL, 38, 1927, 227-251.

[2] Buford Johnson, Changes in muscular tension in coördinated hand movements, *J. Exper. Psychol.*, 11, 1928, 329-341.

[3] Elizabeth Duffy, The measurement of muscular tension as a technique for the study of emotional tendencies, this JOURNAL, 44, 1932, 146-162.

[4] Tension scores for the discriminative reactions are based on the records of 16 *Ss*, since two of the *Ss* (*P* and *Q*) failed to comply with the instructions. Performance scores are based on the records of 15 *Ss* because a third *S* (*R*), who disliked the sound of the buzzer, frequently pressed the reaction-key too lightly to close it and thus left an inaccurate graphic record of his performance. Tension scores for the tapping test are based on the records of 17 *Ss*, since one *S* (*Z*) could not be obtained for this part of the experiment. Performance scores are based on the records of 16 *Ss*, because of an error in tabulating the score of a second *S* (*G*).

of an automobile or was not closed after the appearance of the picture of a horse or a street car. They were scored 'false' when the reaction-key was closed after the appearance of a horse or a street car, and 'omitted' when it was not closed after the appearance of an automobile.

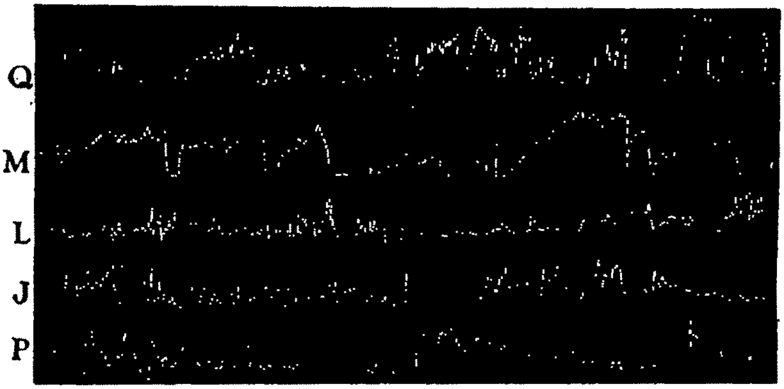
Type of tension tracing (with respect to smoothness) was determined from a classification of the tension tracings from the used hand during the tapping test, since it was here that individual differences appeared in most exaggerated form. An individual who had had no part in the experiment and was not familiar with the results was asked to group the tracings into three categories on the basis of degree of *smoothness* or *regularity* and irrespective of the height of the tracing (or *degree* of tension). The first category comprised tracings which were very irregular, the second, those which were moderately irregular, and the third, those which were very smooth. Division into categories was somewhat arbitrary, since the tracings are not sharply divisible into types. A second classification made by the same individual three months later resulted in two changes in the grouping. A tracing previously classed as Type 1 was called Type 2, and one formerly called Type 2 was classed as Type 3. Because of the uncertainty of opinion about these two tracings they were both put under Type 2, the most indeterminate category. The tracings, with their classification, may be seen in the accompanying plate.

RESULTS

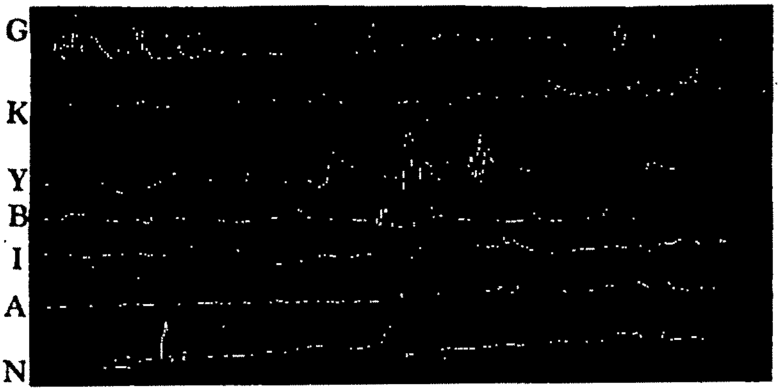
(*1*) *Degree of muscular tension and quality of performance.* The relation between quality of performance and average degree of muscular tension (as measured by pressure upon the dynamographs) is moderately close—high tension tends to accompany poor performance. The coefficient of correlation between tension and performance score is, for the discriminative reactions, −0.47, and, for the tapping, −0.48 (Table I). These coefficients of correlation were secured by the method of rank-differences. Since there were a number of shared ranks for the discriminative reactions, the relation between this performance and muscular tension was obtained also in terms of the Pearson *r*. The coefficient secured by this method was −0.54.

The correlation between chronological age (C.A.) and percentage of correct discriminative reactions ($r = 0.48$) suggests that age may have exercised a determining influence upon the quality of this performance. Age appears, however, to be unrelated to the number of taps ($r = 0.10$). Further analysis shows that age cannot account for the relation between tension and percentage of correct discriminative reactions, since C.A. and tension show no correspondence ($r = -0.10$).

The mental age (M.A.) and the intelligence quotient (I.Q.), as obtained from the Kuhlmann-Binet test, yield rather negligible coefficients of correlation with quality of performance and with average tension (Table I). Summarizing the data, we may say that in this



TYPE 1



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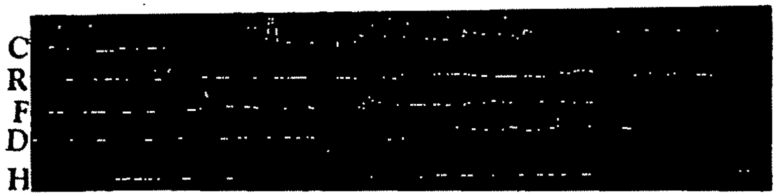

TYPE 3

FIG. 1. TENSION TRACINGS FROM THE USED HAND DURING THE TAPPING PERFORMANCE

The first half of the tracing represents the right hand; the second half, the left hand. The letters on the left refer to the *S*s.

experiment degree of muscular tension is moderately closely related to quality of performance in both discriminative reactions and tapping (high tension scores tending to accompany poor performance), and that it is negligibly related to C.A., M.A., and I.Q.

TABLE I

COEFFICIENTS OF CORRELATION BETWEEN TENSION AND PERFORMANCE

(Values in parentheses obtained by the product-moment method, the others by the method of rank-differences)

		Percentage of correct discriminative reactions	Number of taps	Av. tension in both hands (discriminative reactions)	Type of error (discriminative reactions)	C.A.
Av. tension in both hands	r	−.47 (−.54)	−.48		(.14)	
	P.E.	.14 (.12)	.14		(.17)	
Regularity tension tracing (tapping)	r	(.60)	(.23)	(−.31)	(.59)	(.43)
	P.E.	(.12)	(.16)	(.16)	(.12)	(.13)
C.A.	r	.48 (.51)	.10 (.05)	−.10 (−.12)		
	P.E.	.14 (.12)	.18 (.17)	.18 (.17)		
M.A.	r	.11	.26	−.13		
	P.E.	.18	.16	.17		
I.Q.	r	−.12	.35	−.23		
	P.E.	.18	.16	.17		

(2) *Type of tension tracing and quality of performance.* The percentage of correct discriminative reactions is related not only to the *degree* of muscular tension but also to the *type* of tension tracing with respect to smoothness or regularity. Very irregular tracings are associated with poor performance, while smooth tension tracings tend to be associated with good performance. The coefficient of correlation between quality of performance and type of tension tracing is 0.60, which suggests that *fluctuations* in tension are at least as closely related as *degree* of tension to quality of performance in such a task as the making of discriminative reactions. The close relationship between these two factors (*i.e.* fluctuations in tension and percentage of correct discriminative reactions) is the more remarkable in view of the fact that tension fluctuations were judged from the tapping records and not from the records of discriminative responses.

Regularity of tension tracing and number of taps do not appear to be related, since the coefficient of correlation between the two factors is only 0.23.

The significance of irregularities in the tension tracing is still somewhat open to question, but it appears probable that this aspect of the tracing is indicative of degree of coördination, which, in turn, is dependent upon the ability to inhibit reactions. If this assumption is tenable, the fact that smoothness of tracing appears to be rather closely related to percentage of correct discriminative reactions and very negligibly related to number of taps may be readily explained by the relatively greater demands of the former task upon the ability to inhibit and effectively coördinate responses. Coördinative ability undoubtedly increases as the child advances in age, and we find, as we should expect, a correlation of 0.43 between C.A. and smoothness of tension tracing. But this coefficient of correlation makes it possible to interpret our data in either of two ways. It suggests, on the one hand, that percentage of correct discriminative reactions is correlated with C.A. and with smoothness of tension tracing because, as the child grows older, he increases in ability to inhibit and coördinate his responses, and growth in this ability is the source of improvement in discriminative reactions as measured under the conditions of this experiment. But it suggests, on the other hand, that the correlation between smoothness of tension tracing and percentage of correct discriminative reactions may be the accidental result of the correlation of each of these factors with C.A. The latter conclusion seems less tenable than the former, both on general theoretical grounds and because the relationship between smoothness of tracing and percentage of correct reactions is somewhat closer than that between C.A. and either of these factors. But conclusions must be drawn with caution, since rather wide differences in coefficients of correlation are not necessarily significant when the number of cases is small.

(3) *Tension tracings and discriminative reactions of individual subjects.* The relation between quality of performance and degree and fluctuations of tension can be usefully investigated through an analysis of the records of individual *S*s. A study of the best performers and the poorest performers may yield information upon the factors conducive or detrimental to success. Our analysis will be confined to the discriminative reactions, since we have five records of this performance and only one record of tapping. The data referred to in this section will be found in Table II.

The percentages in the discriminative reactions are based, for all except 3 *S*s, on 5 records of 40 responses, or 200 responses in all. In the cases of *A*, *H*, and *G* special circumstances made it advisable to base the results on 4 records. The percentages in the table are given to the nearest whole value.

TABLE II

TENSION AND PERFORMANCE SCORES, TYPE OF TENSION TRACING, AND AGE

S	Av. tension (in mm.)		Discriminative reactions (in %)			No. taps‡	Type of tension tracing	Age (in mo.)
	Tapping	Discrim. reactions	Correct	False	Omitted			
A	22.7	18.3	99	1	1	330	2	37
B	31.2	22.5	98	1	1	253	2	46
C	25.7	15.2	98	2	1	291	3	45
D	17.0	17.7	97	2	2	295	3	35
Y	29.9	20.7	96	2	3	335	2	41
F	18.4	20.1	96	1	3	262	3	39
G	28.3	18.7	96	3	2	§	2	41
H	15.0	17.7	95	1	4	320	3	43
I	33.2	20.6	94	2	4	293	2	40
J	33.5	17.8	91	6	4	318	1	39
K	26.4	16.4	91	7	2	292	2	39
L	37.0	24.7	89	8	4	269	1	40
M	52.0	22.1	89	5	6	250	1	37
N	27.0	21.1	87	10	3	303	2	35
Z†	—	24.0	82	16	2	—	—	35
P†	38.5	23.4*	—	—	—	261	1	37
Q†	48.6	23.1*	—	—	—	217	1	35
R†	19.9	21.7	—	—	—	259	3	40

*These scores are not strictly comparable with the others in the column. They are the scores of the 2 *Ss* (mentioned in footnote 4) who failed to comply with the instructions for the discriminative reactions.

†See footnote 4 for an explanation of the omissions in the data of these *Ss*.

‡Not comparable with norms for tapping because of the heavy stylus used in this experiment.

§Omitted because of error in tabulation.

Best performers. The best performer in the discriminative reactions (and second best performer in tapping) is *A*, who has 99% of correct discriminative responses. His rank in tension is tenth, and his tension tracing is of Type 2, that is, moderately irregular. He holds rank 11.5 in age, there being only four *Ss* who are as young or younger than he. *B* and *C*, who have 98% of correct responses, are our next best performers. *B* ranks third in tension, has a moderately irregular tension tracing, and is our oldest *S*. *C* has the lowest tension score and has an exceptionally smooth tension tracing. He ranks second in age. Our fourth best performer, *D*, has twelfth rank in tension and shares with two other *Ss* the lowest rank in age. His tracing is exceptionally smooth.

Of these four good performers, one has a high tension score, one a very low tension score, and the other two have tension scores which are distinctly below the median but are not the lowest scores recorded. It is significant that no one of these *Ss* has a tension tracing of Type 1—that is, exceptionally irregular. Two have moderately irregular and two very smooth tracings. Good performance tends to be accompanied by tension scores below the median and by tension tracings which are relatively smooth. *A* and *D*, who excelled in spite of being among the youngest *Ss*, have low, but not the lowest, tension scores and tension tracings of Types 2 and 3 respectively.

Before considering the records of the poorest performers, it is of interest to examine the records of the two *Ss* (*P* and *Q*) who failed to comply with the instructions for the discriminative reactions.[5] *Q* seemed unable to comprehend the requirements of the situation (his M.A. was the lowest in the group), and *P* failed apparently because of emotional stress. *P* was an exceptionally excitable child, frequently involved in "scenes" at home. In the nursery school great difficulty was experienced in persuading her to go for her physical examination or for her mental test. The nursery school records state that *P*'s "chief difficulty is her inability to adapt herself easily to new situations and people. She seems terrified and cries very hard." The tension scores of *P* and *Q* are among the highest recorded, in spite of the fact that *P* seldom closed the reaction-key—a circumstance which would tend to lower her tension scores. Their tension tracings are of Type 1, very irregular. Inability to meet the requirements of the situation was associated with high and exceptionally irregular tension.

Poorest performers. Among the 15 *Ss* for whom scores for the discriminative reactions could be obtained, *Z* was the poorest performer, with 82% of correct responses. His rank in tension is second. Since he did not take part in the tapping test, his type of tension tracing is not recorded. Inspection of his other records indicates, however, that his tracings are undoubtedly of Type 1—that is, markedly irregular. He is one of the three youngest *Ss*.

N is second in inaccuracy of performance, having 87% of correct responses. He holds fifth rank in tension and has a moderately irregular tension tracing. He is one of the three youngest *Ss*.

L and *N*, each with 89% of correct responses, rank next among the poorest performers. *L* has the first rank in tension and has a tension tracing which is very irregular (Type 1). She holds rank 7 in age. *M* holds the fourth rank in tension and has an exceptionally irregular tension tracing (Type 1). His rank in age is 11.5.

These poor performers have, in every instance, high tension scores and, in three instances out of four, exceptionally irregular tension tracings (Type 1). It will be recalled that the two *Ss* who failed to comply with the instructions for the performance had also high tension scores and exceptionally irregular tension tracings. Apparently, with these *Ss* and for this type of performance, high and irregular tension tracings are either distinctly unfavorable for efficiency of performance or are the result of awareness of difficulty or failure. Only one *S* with a tension rank in the upper third gave a performance which ranked above the lowest third, and of our four poorest performers no one has a rank in tension lower than rank 5. Not a single *S* with a tension tracing of Type 1 (exceptionally irregular) gave a performance as good as the median, and all but one of our poorest performers have tension tracings of this type.

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[5] See footnote 4.

Summarizing these data, we conclude that poor performance was accompanied by high tension scores and very irregular tension tracings. Good performance was accompanied by tension scores of various magnitudes from low to high, but moderate or low scores occurred most frequently. The tension tracings of good performers were never of the very uneven type.

(*4*) *Type of tension tracing and type of error.* The smoothness of the tension tracing of an *S* is related not only to his percentage of errors in discriminative reactions but also to his predominant *type* of error in this performance.

It will be recalled that errors were divided into two categories: (1) *false* responses (closing the reaction-key when no response was demanded), and (2) *omitted* responses (failing to close the reaction-key when a response was necessary).

Disregarding the total percentage of errors and considering merely the relation between percentage of false responses and percentage of omitted responses, we can divide our *S*s into three categories: (a) those whose errors are predominantly false responses; (b) those whose errors are approximately equally divided between false and omitted responses, *i.e.* a difference of less than 2% of all responses; and (c) those whose errors are predominantly omitted responses. Relating these data to smoothness of tension tracing, we obtain as the coefficient of correlation 0.59 (Table I). There is a tendency for errors which are predominantly *false* responses to be associated with unevenness of tension tracing. Conversely, errors which are predominantly *omitted* responses tend to be associated with a smoother tension tracing. No *S* with a tension tracing of Type 1 had, among his errors, predominantly omitted responses, and no *S* with a tension tracing of Type 3 had, among his errors, predominantly false responses. Since false responses represent an excess of reaction, a deficiency of inhibition, we have here support for our assumption that degree of smoothness of the tension tracing is indicative of the *S*'s tendencies with respect to inhibition, a factor which undoubtedly plays a major rôle in that complex process which we refer to as coördination.

SUMMARY

The quality of performance of our *S*s in discriminative reactions and in tapping was shown to correlate moderately closely with the degree of muscular tension as measured by pressure upon the dynamographs. Poor performance was almost invariably accompanied by high tension, while good performance was accompanied by tension scores of various magnitudes from low to high, but moderate or low scores occurred most frequently.

The quality of performance in discriminative reactions, but not in tapping, correlated rather closely with the smoothness of the tension tracing from the used hand during the tapping performance. Smoothness of the tracing correlated also with C.A. and with type of error in the discriminative reactions, *S*s whose tension tracings were most irregular tending to be among the younger *S*s and to make errors which were predominantly *false* as opposed to *omitted* responses. It seems probable that excessive fluctuations in tension represent poor inhibition; that lack of inhibition is a severe handicap in certain types of performance; and that degree of inhibition increases with age.

DISCUSSION AND CONCLUSIONS

The results obtained in this experiment, as given here and in the earlier report,[6] suggest the following hypotheses in regard to muscular tension and its relation to performance.

Increases in tension of the skeletal muscles appear to represent increases in excitation. They may result from any type of stimulation, but are found in most extreme degree under the influence of what are commonly termed exciting 'emotional' stimuli. They seem to be particularly characteristic of states of 'effort,' and their association with the excited (or sthenic) emotions is probably due to the fact that such emotional states are notably 'striving' or 'effortful.'

If the foregoing hypotheses are correct, we should expect to find increased tension of the skeletal muscles when the individual is confronted with any problem of adjustment, provided only that he attempts to meet the problem and does not 'give up.' The more strongly motivated the individual or the more difficult the situation which confronts him, the greater would be his muscular tension, or his effort to adjust. For muscular tension is conceived to be a state of alertness or preparedness, which makes it possible to act with greater speed and force.

Granting, then, that the behavior of the muscularly tense individual will give evidence of greater energy output, will have a more 'dynamic' quality, we need to inquire as to the effects of these characteristics upon the quality of performance of a given task. Other factors being equal, we should expect a greater output of work per unit of time. We should expect also to find evidences of the expenditure of greater energy or force in the performance of the task. Some of this energy would doubtlessly be overflow energy, adding in no obvious way to the efficiency of performance. It may be, however, that even such apparently useless reactions as restless movements, pounding, and speaking with great intensity, serve the function of facilitating other more useful reactions. At any rate, during states of great excitation (*i.e.* great muscular tension), we should expect a greater output of work and a greater expenditure of force per unit of time. A common observation of such a state is found in the expression "working fast and furiously."

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[6] Duffy, *op. cit.*, 148-162.

But more important than the *quantity* of the work produced under such circumstances is the question of the *quality* of performance to be expected under these conditions. Is the quantity output at the expense of quality, or does the excess energy available result in improvement in quality as well? The answer to this question may well vary with different performances, with different individuals, and even with the same individual under different circumstances. In all except the simplest of performances (as, for example, a tapping performance of the type used in this experiment) quality of work must depend not alone upon the degree of excitation, but also, and more fundamentally, upon the quality of coördination, or the proper directing of the available energy. It seems probable, though not clearly demonstrated, that coördination of response becomes more difficult under high degrees of excitation, because under these conditions impulsion is probably greater, and it is a commonly acknowledged fact that coördination necessarily involves inhibition. It is not surprising, therefore, that many individuals, under the influence of excitative stimuli, make uncoördinated and ineffective responses. But this is not the inevitable result of great excitation, for there are other individuals who have developed, in varying degree, the ability to coördinate effectively and thus produce good work under these circumstances. Indeed, it seems probable that individuals who can successfully meet the hazard to coördination which is imposed by great excitation will find the quality of their work greatly improved by the dynamic and facilitating effects of such states. It is not, perhaps, an unusual circumstance for such individuals to produce under great excitation work of a quality superior to anything they could produce under ordinary circumstances. This may afford an explanation of such an experience as writing under stress a paper which, when read later, seems to have been written by some one else because the acuteness of analysis and the facility of expression seem foreign to the writer.

Though some individuals may find high tension conducive to good performance, all must necessarily find states of tension very wearing. As a consequence, prolonged periods of work at high tension would be well-nigh impossible, and individuals who habitually work under great tension should probably arrange for shorter work periods and more frequent periods of rest. Under these circumstances their total output of work might nevertheless remain equal to that of an individual working under low tension over longer periods of time.

In our experimental results high degrees of muscular tension were usually found associated with a performance of poor quality. It may well be that these results represent the typical situation, individuals who can perform excellently under very high tension being the exception. It is possible, however, that high tension is a greater liability for young children than for adults, since the latter are likely to have developed more highly their ability to inhibit and to coördinate reactions. In fact, even with our group of *S*s, where the age range was small, there was a tendency for increase in age to be accompanied by increase in smoothness of the tension tracing, a phenomenon which we considered indicative of increased inhibition and, perhaps, increased coördination.

It is not to be assumed that high tension is equally advantageous or disadvantageous for every type of activity. Muscular activities may be handicapped by high tension, since it diminishes ease of movement. Other activities requiring speed and alertness, but not emphasizing choice with its accompanying requirement of inhibition, may in general be favored by relatively high degrees of excitation. Sudden insights and the facile flow of words may, for example, be encouraged, while the careful weighing of evidence may be made more difficult. High degrees of excitation, and consequently of muscular tension, may increase the efficiency of performance of a definite task for which the individual is 'set,' but may decrease flexibility of reaction and thus make more difficult the adjustment to new and unexpected situations.

It appears probable, on the basis of the foregoing discussion, that a moderate degree of tension offers the greatest advantages, since very high tension probably tends to be disruptive and very low tension probably involves lack of alertness or effort. The handicapping effects of very high tension are clearly shown in our results. Those of low tension are more problematical and have not been demonstrated in this experiment. Certain other investigators have reported, however, instances of increased efficiency under increased tension. A. G. Bills,[7] for example, has shown that a group of individuals performed certain tasks more efficiently under a voluntarily assumed and individually different 'optimum' tension. Whether voluntary and involuntary tensions of the muscles are comparable in their effects may be debatable, but it seems reasonable to suppose that tension could be too low as well as too high for efficient performance. Perhaps diurnal fluctuations in the curve of work could be explained on the basis of tension changes and their relation to efficiency. Tension may in general be too low immediately after waking and too high later in the day, and individual differences in tension variations may account for the observed differences in the work curve of different individuals.

Subsequent investigations in this field should consider, among others, the following problems: (1) To what extent does the relation between muscular tension and quality of performance vary with (a) different tasks, (b) different individuals, and (c) adults and children? (2) Are voluntary and involuntary tensions comparable in their effects? (3) What is the relation between various degrees of excitation, or of muscular tension, and the ability to inhibit and coordinate reactions? Answers to these questions should give us important information about optimum work conditions for different tasks and for different individuals.

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[7] Bills, *op. cit.*, 229-250.

PRACTICE AND THE WORK DECREMENT

By EDWARD S. ROBINSON and FLORENCE R. ROBINSON

In a recent paper Glaze shows that where a highly monotonous task is performed every day or on alternate days for a considerable period, the decrement within the day's work grows progressively less marked.[1] Practice thus seems to develop a resistance to those losses in efficiency which characterize continuous work. The task studied by Glaze consisted in the writing of *ababab*, etc., as rapidly as possible for a period of 20 min. One *S* did this work every day for 36 days, another every day for 32 days, and a third on alternate days for 23 days. The average work decrement in terms of speed of writing is much more pronounced during the first 5 days than it is later. This is true for all of his *Ss*.

While common observation would indicate that it is possible to build up through practice a resistance to work decrement, the experimental study of this process is interesting and important. In the first place it is desirable to test the conclusion of casual experience. In the second place, there is a certain sense in which this increasing resistance to decrement runs counter to logical expectation. Practice typically brings about an increased average speed of performance and one might expect, therefore, that the practiced performance, since it is carried on at higher speed, would be more, rather than less, likely to suffer decrement. It is possible that experiment may reconcile the generally accepted fact of decreasing decrement with this contrary but not unreasonable expectation. There is, in short, the possibility that experimental efforts to demonstrate the influence of practice on the decrement may give clues as to the more precise mechanism of that influence.

Certain of the experimental results that we shall present in this paper are similar to those of Glaze and to a considerable extent simply corroborate his findings. We have also considered changes in the amplitude of the writing of our *Ss* and the data thus secured introduce additional facts that need consideration in connection with this problem.

Subjects. Fifteen men, all Yale undergraduates, served as *Ss* in our experiment. These men were recruited from the employment office and worked for pay. The records of only 10 of the men were utilized; the other 5 were dropped because of absence from one or more of the experimental sittings.

Procedure. The *Ss* worked as a group. The sittings began at 8:30 a. m. on 5 days a week and continued for 3 weeks, *i.e.* for 15 days. The task consisted of writing *ababab*, etc., as rapidly as possible for a 20-min. period. The writing was done in a small, bound composition book. The page was turned at the end of each minute, the time for the turn being called out by the experimenter. Each *S* was provided at the beginning of a sitting with three sharpened pencils.

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[1] J. A. Glaze, The effects of practice on fatigue, this JOURNAL, 42, 1930, 628-630.

The *Ss* were directed to work for speed. Nothing was said about the maintenance of a given quality of handwriting except that the *Ss* were told to keep writing on the ruled lines of the composition book. After certain general explanations had been given, the special experimental 'set' was established by the following verbatim instructions which were spoken by the experimenter:

> You are to write the letters, *ababab*, as fast as you can until I tell you to stop. Keep your writing on the line. Use your natural handwriting and write the full length of the line at each end. When I say 'Turn', turn over the page thus and keep going. If you make a mistake, don't stop to correct it, but keep going. Do your best all of the time. Is this clear? I will tell you when to stop. Are there any questions? When I say 'Go!' open your notebook thus and start writing. Are you ready? Go!

At the end of the sitting the *Ss* were asked not to practice the writing or discuss the experiment between sittings. After the first day the instructions were simplified by deleting all of the above paragraph between the command: "Do your best all the time" and "Are you ready?"

Of the men whose records are included in our results, all seemed to be putting forth something like a maximum effort throughout the experiment.

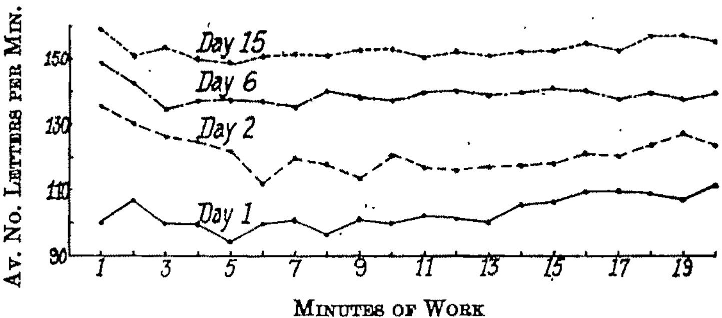

Fig. 1. Work Curves for Days 1, 2, 6, and 15, in Terms of Speed of Writing (Average performances of 10 *Ss*.)

Results

In Fig. 1 we present composite work curves for the group of 10 *Ss*. In the interests of simplification we have given the curves for Days 1, 2, 6, and 15 rather than for the entire series. The curve for Day 1 shows a possible slight early decrement. A more definite tendency, however, is a gradual and continuous rise in letters written per minute. This does not mean that the factors normally producing decrement were absent, but rather that even within the single experimental sitting the factors producing improvement were very strong. Evidence for such an interpretation is presented by the great increase in general speed from the first to the second day.

The curve for Day 2 shows the characteristic work decrement. The improving influences of practice had by this time diminished as shown by the smaller day to day improvement following Day 2. Such decrement as appeared had spent itself by the middle of the 20-min. period, however, and from that point on there is evidence of further practice improvement.

The curve for Day 6 again shows a decrement though it is less in magnitude and duration than that for Day 2. Improvement following the decrement is not definite. This is presumably due to the fact that a practice level for the general function was by this time near at hand. Support for this interpretation appears in the relatively small change in general level of efficiency from Day 6 to Day 15.

During Day 15 there is a small early decrement and no statistically certain practice improvement.

We may summarize our reading of these curves by saying that there was at the beginning of the experiment no certain work decrement, which fact was probably due to a masking effect exerted by factors making for practice improvement. As the experiment progressed and the factors making for practice improvement became less effective, a pronounced decrement appeared. This decrement then diminished until at the end of the experiment it was considerably reduced. Whether a diminution in the causes of decrement was actually taking place even in the first days of the experiment can not be proved. It seems altogether likely that this was the case, however, and that the apparent increase in decrement was due more to the cessation of practice improvement (which always tends to mask decrement) than to any real increase in that decrement.

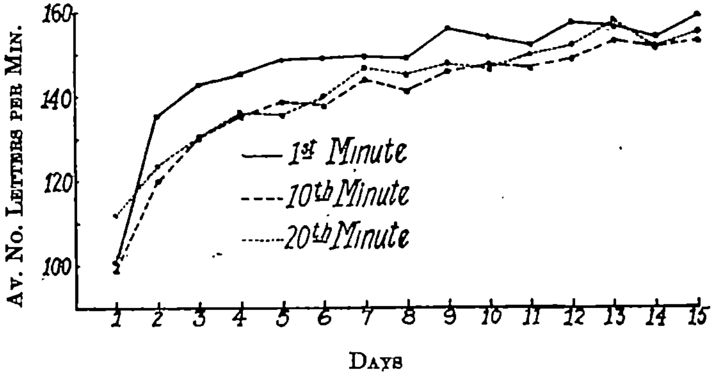

Fig. 2. Practice Curves for the 1st, 10th, and 20th Minutes of Work in Terms of Speed of Writing
(Average performances of 10 *S*s.)

In Fig. 2 we have presented in different form the essential facts brought out above. We have plotted practice curves for the 1st, 10th, and 20th minutes of work for the entire experimental series. The amount of decrement within the work curve for any day is shown by the distance between the curve for the 1st minute and those for the 10th and 20th minutes. The small decrement for the 1st day is shown by the fact that the curves for the 10th minute and 20th minute are at that day about as high in level as that of the 1st minute. The diminishing decrement as the experiment progressed is shown by the converging of the three curves. This figure shows clearly that the tendencies brought out in our examination of the four sample work curves hold for the experiment as a whole.

In an earlier investigation of continuous writing it was noted that the writing increases in size during a period of continuous work.[2] It was not found, however, that this increase influenced the magnitude of the decrement in speed. Work conditions showing relatively slight decrement manifested this change in size of writing as markedly as did those conditions in which the decrement was more pronounced. In spite of this fact we felt that changes in size of writing ought to be taken into account in connection with the present study.

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[2] E. S. Robinson and A. G. Bills, Two factors in the work decrement, *J. Exper. Psychol.*, 9, 1926, 415-443.

In Fig. 3 we have presented results on the size of writing at three points in the work curve, the 1st minute, the 10th minute, and the 20th minute. Each curve in the figure represents those changes in size brought about by practice. The tendency of the writing to increase in size within the single 20-min. period is shown by the distance between the curves at any given day.

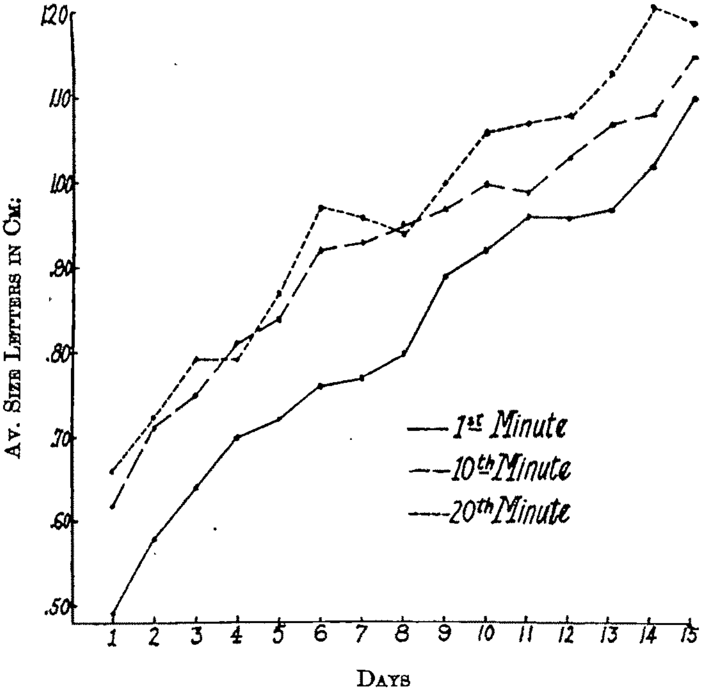

FIG. 3. CHANGES IN SIZE OF WRITING THROUGHOUT PRACTICE
(Curves represent average performances of 10 *Ss* for 1st, 10th, and 20th minutes).

As shown by all three of the curves, the size of the writing increases markedly with practice. The question is thus raised as to whether the decrease in speed decrement which also accompanies practice is a function of this other change, *i.e.* the increased size of the writing. If the intra-period change in size increased from day to day we should have direct evidence that this were the case. The change in size of writing from the 1st minute to the 20th minute, however, remains approximately the same throughout the experimental series and the change from the 1st to the 10th minute seems actually to diminish. It would appear, therefore, that such resistance to the speed decrement as develops with practice is not due simply to an adaptation whereby the S comes to meet felt losses in speed by promptly increasing the amplitude of his movements.

We can hardly conclude that the larger size of the writing at the beginning of work on the later days has nothing to do with the increased resistance to decrement in speed. It is possible that with large early writing it is necessary to increase the size only moderately to protect against decrement. That is to say, an absolute increase which is insufficient to protect against decrement when the early writing is relatively small, may be sufficient when the early writing is large, as it was in the later days of the experimental series. It seems to us that this possibility is, however, only a remote one. In the investigation cited above it was definitely discovered that decrement in speed may be independent of changes in size of writing.[3] Since in the present experiment there is certainly no direct relation between intra-period changes in size of writing and the decrement, it seems likely that such changes in size as occur with practice do not constitute the principal cause of that reduction in decrement which also accompanies practice.

SUMMARY

We have confirmed Glaze's finding that the decrement in speed which occurs in the continuous writing of *abab*, etc., for 20 min. is diminished by practice. We have considered another product of practice—the tendency to write larger and larger letters—but we have not found any direct evidence that this altered size of writing is an explanation of the diminished decrement.

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[3] *Op. cit.*, *J. Exper. Psychol.*, 9, 1926, 422.

HUNGER AS A FACTOR IN LEARNING

By W. A. Bousfield and M. Sherif, Harvard University

It is conventional to speak of the reflex as if it were a simple, point-to-point correlation between the stimulus and the response. At the same time it is practically taken for granted that most behavior which the behaviorist might want to call reflex, or at least to express in terms of stimulus and response, is dependent upon the general state of the organism, such as drive, fatigue, emotional factors, etc. In eating, for example, the hungrier the animal the less easily it is distracted from its eating; in general terms, the stronger the drive, the less easy is distraction. However, since the relation of stimulus and response forms the setting of most experimental work and theoretical behavioral discussion, it seems well to indicate the importance of what is more or less obvious to common sense by presenting definite quantitative data upon the eating reaction. These results also suggest a method by which drive can be measured.

The experiment consisted of the determination of progressive changes in the times of cessation of eating, after a loud noise, and when the animals had been deprived of food for determined intervals of time. The *Ss* were 6 chickens and 6 rabbits.

The following technique was employed with the chickens. At 7:30 A. M. a supply of food, considerably in excess of what the chickens would eat at one feeding, was placed in the coop. Chicken No. 1 was removed and carried in a box to a point about a quarter of a mile distant, where it was placed in a cage also containing a liberal supply of food. At 8:30 A. M. the five remaining chickens in the coop were caged so that food could no longer be touched. The *E* then returned to Chicken No. 1 and waited, when necessary, for it to return to food. When it was evident that the eating was definitely under way, he fired a blank cartridge over the chicken's head, simultaneously with starting a stop-watch. All of *E*'s movements, as well as the sight of the pistol, were hidden by means of suitable screens. When eating was once more evident after the firing of the shot, the stop-watch was stopped, thus measuring the cessation time of eating in seconds. Chicken No. 1 was then allowed to remain in the cage for an additional interval of about 15 min. It was then removed, and after all remains of food had been cleaned up, Chicken No. 2 was placed in the cage. Following a fast of 1½ hours, food was presented to Chicken No. 2. The shot was fired as soon as the eating was definitely in progress, and the time of cessation was obtained with the stop-watch as in the case of Chicken No. 1. This procedure was repeated for the remaining four chickens, with periods of fast lasting 3, 4½, 6 and 7½ hours respectively. Since food was continuously present in the case of Chicken No. 1, the period of fast has been arbitrarily designated as 0.

The parallel experiment with the rabbits differed essentially only in one respect. The chickens were used on alternate days, and the rabbits on successive days.

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*Accepted for publication March 16, 1931.

The results are shown in Figs. 1 and 2. In each figure the times of fasting increase from No. 1 to No. 6—for the six chickens in Fig. 1 and the six rabbits in Fig. 2.

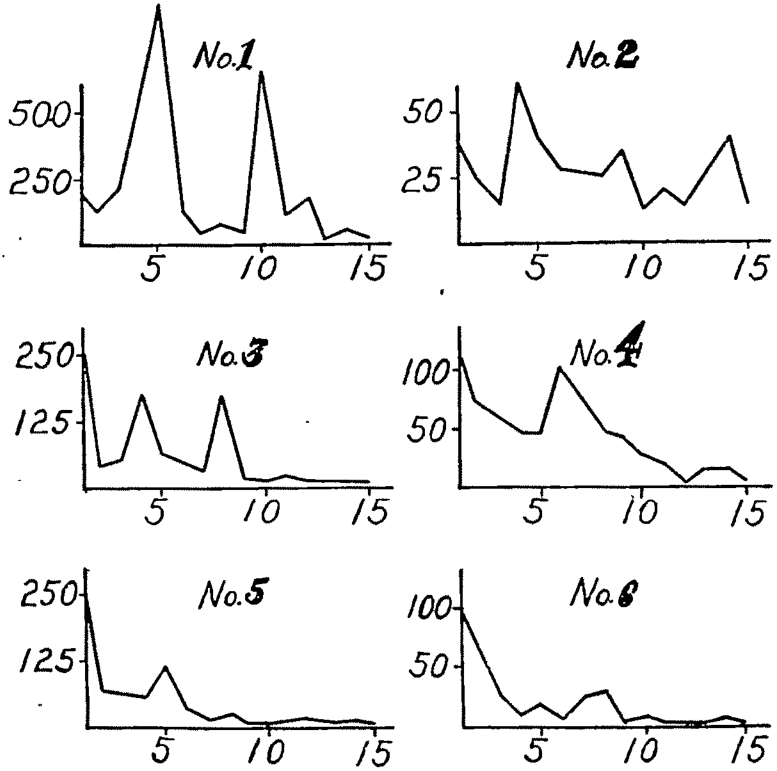

Fig. 1. Results for the Chickens
(Abscissae: Trials. Ordinates: Time, in sec., of cessation.)

It is evident that the times of cessation tend to decrease in successive trials, that the decrease is greater or more rapid or both as the period of fasting increases, and that cessation of eating at the pistol shot may fail to occur (as in the case of Rabbits Nos. 4, 5, 6, Fig. 2).

The figures suggest that the rabbits learned more rapidly than the chickens. However, it must be remembered that the rabbits learned on successive days so that there was less chance for forgetting. Moreover, it may have been easier for a rabbit to resume chewing upon food already in its mouth than for a chicken to initiate the entire pecking response.

Conclusion. The data of this experiment give an univocal quantitative indication of the influence of the condition of the organism as a factor in what may be designated as a type of learning. Specifically we may conclude: (1) that the greater the degree of hunger, the sooner will a loud noise become relatively ineffective in inhibiting the responses to food; and (2) that the time of cessation of observable eating responses is a function of the number of trials in the cases of the longer fasts.

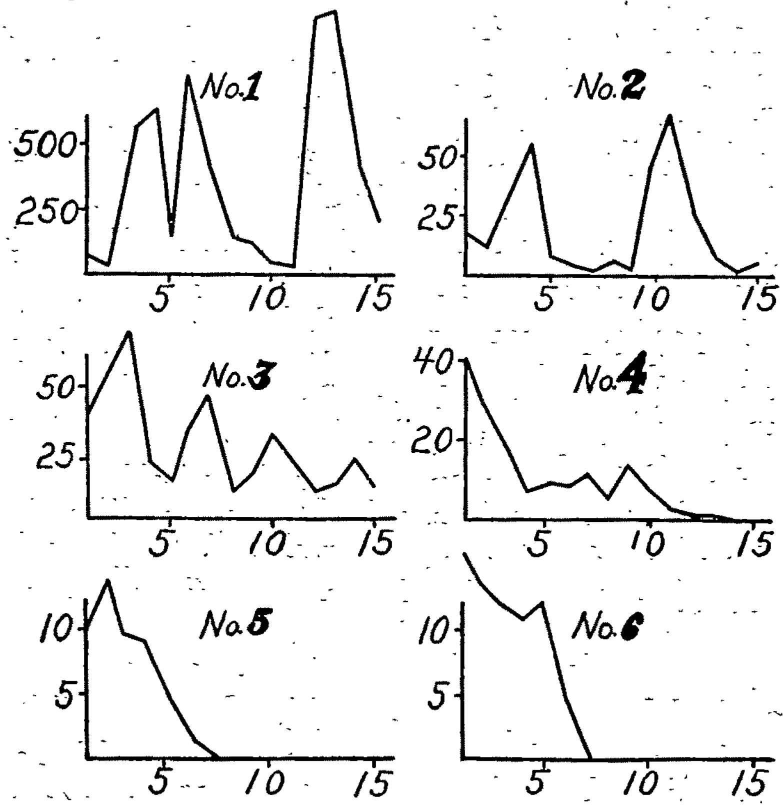

FIG. 2. RESULTS FOR THE RABBITS
(Abscissae: Trials. Ordinates: Time, in sec., of cessation.)

APPARATUS

THE CONTROL OF EYELID MOVEMENTS IN VISUAL EXPERIMENTS

By SIDNEY M. NEWHALL, Yale University

Blinking and winking, particularly during the course of visual adaptation or fatigue experiments, have not received the technological consideration that they merit.[1] This view was reached while reading the literature on visual adaptation and while trying to control the lid in adaptation experiments. In some reports of elaborate studies giving evidence of great precision in other respects, the matter of lid movement is either not mentioned at all or else dismissed in cursory manner. Even in studies which are not primarily adaptational, but in which the visual stimulus is of some duration, lid movement might prove disturbing. The considerable literature of these movements contains techniques for recording the blink or wink, results on the effectiveness of various stimuli and situations in eliciting the blink, correlations between movements of the eyeballs and the lids, data on the temporal relations of lid movements, and surprisingly little concerning devices for restraining the movements.[2] Consideration of such accumulated data in the light of the present problem may lead to improved control, and incidentally uncover results of more general interest. Some relatively simple control devices and certain other details which may be familiar to the expert are included for the possible benefit of others less oriented in this complex field.

First of all something should be said concerning the relation of the lids to the pupil. The entire pupil is unobstructed in the case of the normally open eye. The amount by which the lids clear the pupil depends partly upon its diameter, and that of course varies with the degree of light, and is affected by accommodation and emotion. Ordinarily, the lower lid is not particularly close to the pupil, and seems never to intercept normally entering light. The upper lid usually clears by a much narrower margin, and in a comparatively small number of heavy-lidded cases actually does partly cover the pupil. In the usual wink or blink the upward component of the lower lid movement is relatively slight, and probably never reaches the lower edge of the effective pupil. Therefore our especial concern in control is with the upper lid.

Synchronous with both reflex and voluntary lid action, however, there occur regularly movements of the eyeball which inevitably upset the line of sight.

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[1] The term 'blink' is used to designate the reflexive lid closures while 'wink' is used for the movements made voluntarily or with intent.

[2] The relevant literature includes the citations by W. R. Miles, Elevation of the eye-balls on winking, *J. Exper. Psychol.*, 14, 1931, 311-332, and by J. Peterson and L. W. Allison, Controls of the eye-wink mechanism, *ibid.*, 14, 1931, 144-154.

Opportunities for coöperative research in the Institute of Human Relations were utilized in the preparation of this paper.

Miles recorded very rapid elevation of the eye simultaneously with the descent of the upper lid, followed by a slower reverse movement, as the lid was lifted. According to his measurements taken during the usual periodic reflex in reading or looking at a picture, the upward excursion of the cornea consumes about 50 σ and the downward about 110; while the total vertical displacement is in the order of 10 to 15° of visual angle. If such eye movement occurs only in conjunction with lid movement innervation, correction of the latter would eliminate fixation lapses due to the former. For the present, attention may be concentrated solely upon the problem of lid control.

The object in lid control in adaptation experiments is to minimize undesired interference with the incidence and effective duration of the experimental stimulus. A wink or blink in progress produces changes of the illumination situation in the region of the corneal surface. As angles change, light reflected from the lids varies, and there will be possible some change in the composition or quantity of flux entering the pupil. But it is the sudden reduction of the main experimental stimulus, with the resultant recovery from stimulation, which constitutes the most serious consequences of lid or ball movement. The duration of this cut-off may be important.

Actually the time relations of the lid movement are variable, both among individuals, and in the given individual. Consequently it is difficult to secure values which can be regarded as generally representative. The duration of the complete blink, however, *i.e.* from the commencement of closure to the end of opening, seems usually to be over 150 σ. In case of the common periodic closures during reading, Miles suggested 250 σ as representative of the time from the beginning of the movement until fixation is again established, although at 150 σ the cornea is back to within 2° or 3° of its original position. During the blink there is first the decreasing phase while the lid gradually covers the pupil, followed by the somewhat slower increasing phase while the lid rises at the end of the blink. These phases are somewhat protracted by the reverse ball movements already mentioned; in partial blinks the phases may be incomplete. Since the upper lid clears the pupil but slightly when the eye is normally open, during a full blink the pupil must be more or less obstructed for nearly the complete duration of over 150 σ, and for a certain part of the time the pupil is wholly covered, and the external light stimulus is largely cut out. Miles estimated this latter time to be in the neighborhood of 100 σ.

The wink is more variable than the blink because it is more affected by fatigue of the local mechanism and depends so much upon the *Aufgabe* of the *S*. Indeed, an individual may close his eyes at pleasure and hold them closed indefinitely; if it were legitimate to consider such actions winks, then winks would be indefinitely variable. The duration of a complete wink, according to Garten's average values, varies from 308 σ to 420 σ. He gives times for the lowering of the upper lid ranging from 75 σ to 91 σ, and times for the raising from 142 σ to 177 σ. Times of total coverage of the pupil are from 133 σ to 201 σ. The time for partial coverage is probably not much less than the total time of the wink, for the same reason as in the case of the blink.

This cut-out of stimulation by lid movement would not be a serious difficulty if the lid would leave the pupil clear for a duration sufficient to meet the requirements of adaptation experiments; the trouble is that the usual rates of blinking are so high that a sufficient stimulation period cannot possibly be interpolated between two blinks. Table I indicates what short durations are likely to occur between normal, unrestrained, automatic blinks under 'ordinary' conditions.[3] Blinks were counted at all hours of the day and under a great variety of conditions, with the hope of getting something representative. The left column in the table gives the number of blinks per min.[4] The succeeding columns to the right show frequencies of occurrence of these different rates in the various *S*s observed. *A*, for instance, blinked from 10 to 14 times per min. upon 28 of the occasions when his blinks were counted, 15 to 19 times on 52 occasions, and so on. It is clear that ordinary blinking will interfere with stimulation at frequent intervals.

TABLE I

FREQUENCIES OF BLINKS PER MIN. UNDER UNCONTROLLED CONDITIONS

Blinks per min.	Subjects						
	A	*B*	*C*	*D*	*E*	*F*	*G*
5–9						2	
10–14	28				3	11	
15–19	52	1		2	10	7	
20–24	36	8		3	12	7	2
25–29	15	19		8	7	6	8
30–34	11	49		8	5	3	6
35–39	3	36	2	6			7
40–44	2	12	4	2	1		6
45–49		9	8	3			9
50–54			12	2			1
55–59			11				
60–64			7				1
65–69							
70–74			6				

If the assumption is made that during the average blink the lids more or less cover the pupillary aperture for 150 σ, and if the *S* is blinking at the rate of say 20 blinks per min., then the stimulus is more or less reduced for as much as 5% of the total time. Along with this there is the doubt, discussed by Miles, concerning the adequacy of vision for 100 σ or so immediately following the lid movement. Therefore it seems not impossible that the lid movement may interfere for as much as 10% of the total time. Figures of this order in themselves point to the desirability of special control.

Though it is the retinal recovery due to lid or ball movements that concerns us especially here, we must note that those movements provide also for what Miles aptly terms the 'servicing' of the corneal surface, with the consequence of maintaining or restoring its condition of relatively uniform transparency in the region before the pupil. Both this corneal and the retinal recovery have a bearing upon the discrimination and acuity of vision which suggests a possible source of error needing to be investigated, viz., that when the eye is held open for a protracted period, what appears to be a consequence of retinal adaptation or fatigue may be in part a matter of mounting irregularities in the transmission of the optical system due to drying or particles collecting on the corneal surface before the pupil. Any such effect might be factored out by suitable experiments.

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[3] All tabulated data are taken from unpublished studies by F. A. Almquist and the writer.

[4] A measure sometimes used is the 'inter-blink period,' and this is supposed to give the time elapsing between blinks. Unfortunately the measure is equivocal, since what it actually gives is the duration from a given phase of one blink to the corresponding phase of the succeeding blink; one or both blinks being actually in progress during the so-called inter-blink period. The measure is secondary in the sense that it is calculated on the basis of blinks per unit time.

Of course, the control of lid movements is really justified only if they constitute a sufficiently large source of error. This they do if they create an error which is large relative to the other errors of experiment or to the differences investigated. What constitutes a significant error must depend upon the procedure and problem at hand. Blinking during dark adaptation, for instance, would not be as serious as during light adaptation. Blinking in the early stages of color adaptation, where the rate of adaptation is typically rapid, would probably have a more retarding effect than in the later stages. Blinks during a short period of adaptation might be more upsetting than in a long period. A satisfactory answer to the question of what is a significant error cannot be made in general terms, but a certain justification of the present paper may be offered and a warning sounded by presenting some data showing the magnitudes of effects ascribable to lid movements in some actual experimentation.

S fixated monocularly the center of a disk-shaped 3° field which was divided by its vertical diameter, the one half being of low purity and appearing pinkish, while the other half was neutral, though of equal brightness. *S*'s task was threefold: (1) to look at the center as steadily as possible; (2) to wink uniformly at rates presented by a metronome, but not to blink between times; (3) and to report any disappearances of the color apart from those synchronous with the wink itself. *S* estimated the fading out of the color by comparison with the adjacent neutral half of the field.

Different rates of winking were prescribed in order to discover whether or not there would result noticeable corresponding differences in degree of adaptation reported. One measure employed of the effect upon adaptation of the winking was the number of seconds elapsing before the color faded out. Table II summarizes results based upon experimentation with the left eye and the right eye during exposure periods of 5 and 10 min. for both. It shows that when *S* was winking at the rapid rate of once per sec., total de-saturation was not once achieved; that when, in another series, the rate was reduced to 15 per min., the time of disappearance became finite and averaged 129 sec.; and that when the rate was dropped to 7.5 the time was only 84 sec. Unpublished data suggest that without any blinking or winking, under comparable conditions, the adaptation time would fall to around 40 or 50 sec. According to the measure here employed, winking seems to have had a pronounced effect upon the course of adaptation.

TABLE II

WINKS PER MIN. *VS.* ADAPTATION TIME

Winks per min............	60	15	7.5
Av. adaptation time (in sec.)	—	129	84

A second measure of the effect of winking upon the adaptation process was the number of disappearances of the color during the time of exposure. The color would rarely disappear for very long, and during the exposure period of 5 or 10 min. it might reappear and disappear a number of times. Table II simply referred to the initial disappearance. Table III shows the average numbers of disappearances when winking at the three respective rates. There were no fade-outs (*i.e.* disappearances) during the fast rate of winking, an average of 6 during the medium rate, and 15 during the slow rate. In other words, the more frequently the stimulus was cut off by winking, the fewer times the color experience reached zero. On the basis of this measure too, the rather small differences in winking would produce observable effects upon adaptation. Effects of reflex blinking would probably be of the same sort, though less in degree.

TABLE III

WINKS PER MIN. *VS.* FREQUENCY OF FADE-OUTS

Winks per min............	60	15	7.5
Av. no. fade-outs	0	6	15

Again and again in the course of color adaptation experiments, various *Ss* including the writer have observed and reported an obvious increase in saturation or brilliance following a wink or a blink. In some instances the color had already completely adapted out, and the wink seemed to provide the rest which again lowered the absolute color threshold; in other cases adaptation had been but partial, and the wink was followed merely by greater saturation or brilliance. Such introspective data seem to be unmistakable evidence for a degree of restitution ascribable directly to the interruption of stimulation by *a single lid movement.* Winking and blinking would seem to constitute a source of error sufficiently large to merit special efforts at control.

Procedural controls. Lid movements can be controlled to some extent without any special instrumental appliances. One simple expedient is to wink several times, or merely to sit with the eyes closed, for several seconds preceding the experiment, and to open them only just in time to begin. The voluntary closure is supposed to relieve the strain, make blinking less urgent, and so help prevent irregular lid movements. Sometimes it is feasible for *S* to determine himself the moment when the adaptation period shall begin, and thus derive maximum benefit from his preëxperimental rest.

A rather common practice is to have *S* wink at prescribed times during the adaptation period and try to refrain from blinking at other times. Prescribed moments for winking may be regularly spaced throughout the period and be made known to *S* by auditory or tactual signal. A variation is to present to *S* every *x* seconds two signals *y* seconds apart, and instruct him to make lid movements only within the *y* periods. This use of prescribed periods has the advantage of giving *S* time to make several winks together, which is often what he feels like doing. In any procedure where the winks fall within the adaptation period they should be made as uniform as possible so that the assumption can be made that they are having about the same duration.

Voluntary winking at scheduled times seems a fair control where the investigator will be satisfied with the relative values resulting; but if 'absolute' values are needed, this method is not directly applicable. About the best that can be done with it is to have *S* wink at different frequencies in different series, and from the group of adaptation rates resulting, attempt to extrapolate values for an absolute curve.

Whenever *S*'s winking is regulated by a fixed time schedule, care is necessary to so regulate the spacing that he will be able to refrain from blinking for the duration of the non-wink periods. One might suspect that the individual's normal or usual rate, *e.g.* Table I, could be used in predicting this ability to refrain from blinking; but actually it does not appear to be true that the slower the blink rate the greater the ability to inhibit blinking. Indeed, there is some small evidence for a slight inverse relation between these two variables.

The direct thing to do in adjusting the length of the non-wink periods is to test the individual's ability to refrain from blinking under the given experimental conditions. Table IV exhibits average values for the non-blink span of 3 *S*s taken at different hours of the day. The wide variations among individuals, and particularly the variation in the individual case, suggest the wisdom of allowing a wide safety margin in setting up a time schedule for spaced winking. Winking times for *A* would clearly have to be much more closely spaced than for *C*. In the case of any *S*, ability to refrain from blinking is so variable that the arbitrary non-wink period must be made relatively short, *i.e.* as short as the shortest non-blink span, if all trials are to be counted. Longer non-blink spans than in the table are to be expected under favorable laboratory conditions, but the variability would remain high.

TABLE IV

MEAN NON-BLINK SPAN IN SEC.

(Taken hourly)

	A. M.					P. M.									
Subject	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10
A	18	8	9	11	9	13	8	12	12	9	12	11	12	12	8
B	34	26	22	16	22	22	12	12	13	20	14	16	12	10	10
C	85	71	33	49	33	36	17	31	24	66	44	78	56	48	34

Another but related procedural control is merely to have *S* refrain from blinking for the whole of the experimental adaptation-time. This is probably the simplest and most satisfactory procedure of all when the duration and conditions are not such as to produce a disturbing degree of discomfort or a significant change in the optical properties of the corneal surface. Judging from Table IV, one might be inclined to say at once that *S* probably could not inhibit blinking regularly for a sufficient period to meet the requirements of any adaptation experiment. Still, due to the great variability of the span, almost any *S* will now and then succeed in refraining for a long period; if only such periods were counted, requirements might be met. This would be a wasteful expedient, but perhaps otherwise quite satisfactory.

The expedient of making the non-blink span of maximum length is a very practical one from the standpoint of adaptational technique, because, up to a certain point, the longer the mean span the larger the proportion of trials in which *S* could keep his eye open for the required duration, and consequently the more efficient that phase of the procedure. Some of the controllable factors affecting the duration of the non-blink span are mentioned below.

The instructions and *S*'s reaction to them are of primary importance. Those *Ss* whose results are presented in Table IV, for instance, were asked to fixate steadily and to refrain from blinking "as long as possible." The last phrase permits of different interpretations; properly perhaps it should be taken literally to mean until the lids can no longer be held open; actually, one *S* might make the culminating lid movement as soon as discomfort became noticeable, while another might wait until he experienced severe pain or longer. As any one knows who has tried, the non-blink span is characterized by a mounting experience of strain or pain which in time reaches a more or less unbearable degree. The strain sensations may be wholly absent from the early part of the duration, but there is generally a progressive increase in the latter part anyway, sometimes with very rapid acceleration towards the very end. Discomfort is often intermittent, increasing and decreasing until the final intensive phase which always culminates in the alleviating lid movements. According to the writer's experience, some *Ss* make these movements sooner than necessary. While an inordinate degree of discomfort would naturally upset fixation and the experiment as a whole, a moderate amount probably does little harm.

The fact that athletes by habituation and practice develop marked ability to keep their eyes open in the presence of disturbing stimuli suggests the possibility of prolonging the non-blink span in that manner. The situation in the case of the adapted *S* is, however, quite different. Whereas the athlete becomes capable of continuous vision during a relatively brief, sudden, and intensive stimulus, as when the boxing opponent's glove passes close to the eyes, the adapted *S* must typically refrain from blinking during much longer periods of comparatively steady and moderate stimulation. Such observations as we have made suggest that mere practice in restraint is of little avail except in so far as it leads to the development or discovery of very special expedients, *i.e.* aids of the sort mentioned below.

In monocular experiments, relief may sometimes be had by closing the unstimulated eye. If there is enough difference in illumination this will change the diameter of the stimulated pupil, and might therefore require the use of an artificial pupil. Another expedient is to wink very slightly when the strain has mounted to an uncomfortable level. The objections to this expedient are that it must be highly practiced and repeatedly controlled to make sure the upper lid is not partially hiding the pupil during the wink. Then, too, the synchronous reverse ball movement might occur even though the wink were slight, and so upset fixation even though the pupil were not occluded. It is safer to have the *S* keep his eyes partly closed continuously, because then there is less chance of ball movement and it is so much easier for *E* to check upon the degree of closure. Holding the head tilted back somewhat during the adaptation period is nearly equivalent to partially closing the eyes; in both cases the exposed area of eyeball is decreased. One other expedient in prolonging the non-blink span is to voluntarily assume a visually relaxed attitude; for this, too, seems to retard the development of the strain sensations and sometimes reduces them when they are already present.

Even if the non-blink span has been prolonged as much as feasible by the use of various expedients, it still may not be long enough. There remains, as before suggested, the possibility of extrapolation. In different trials the adaptation times may be made of different lengths ranging from very short to the maximum of which the S is capable. If a sufficiently close relation is found between the measures of adaptation and the lengths of the adaptation periods, then values for still longer periods can be extrapolated. It is well known that extrapolated values are not necessarily dependable because of the sudden changes sometimes found in extreme situations.

A control which is simple, economical, and which appeared to give fairly satisfactory results in estimating rates of color adaptation, is described below. S adapts without any winking or blinking until the strain mounts to the point where he feels fairly certain that he is within a few seconds of his 'breaking point.' He then makes his comparison or match rapidly and thus brings the trial to an end. On different trials he will naturally be able to refrain from blinking for different durations, so that different adaptation times will eventuate, but time and amount of adaptation may be plotted against each other. In this way, S's very longest non-blink spans will contribute to the data. If E finds that he is not getting enough shorter adaptation times for plotting purposes, he may instruct S to make some of his comparisons before he approaches his breaking point. The following scatter from the writer's data represents results secured with this control and the following procedure:

S, in a darkroom, faced the 3° vertically divided field aforementioned, and observed it in the Maxwellian view. This small field was surrounded by and concentric with a 30° neutral field. By means of two optical trains and two rotating disks bearing sectors of Munsell red 4-11, it was possible to adjust the brightness of the large field and both the brightness and purity of either half of the small central field.[5] On any trial, both large and small fields were first set at neutral with a uniform brightness of about 1.5 ml., and S was light-adapted. Then a degree of purity represented by 180° of red sector would be suddenly introduced by E into one half of the central field, and a color adaptation period would commence. As soon as S felt that his breaking point was imminent, he would turn a handwheel, rapidly raising the saturation of the erstwhile neutral half until it balanced the other half. The Y-axis in Fig. 1 shows the degrees of sector or quantity of color which had to be introduced into the neutral half to match in saturation the 180° half to which S had been adapting. The baseline represents the time of color adaptation in seconds. These are results of several weeks of experimentation, for only a few trials were feasible upon any given day. Each plotted circle represents a trial. A filled-in circle indicates expression of doubt by S as to the accuracy of his match on that trial. In view of the difficulties inherent in the rapid match process, and the fact that each individual trial is plotted, the trend of the data seems fairly definite.

Instrumental controls. There are a variety of controls which depend primarily upon some special device or apparatus. Some lie near the boundary between what we have called the procedural and the instrumental, but may be included

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[5] The original apparatus, of which the present is an elaboration, was constructed and described by R. Dodge and S. M. Newhall, Colored after images from unperceived weak chromatic stimulation, *J. Exper. Psychol.*, 10, 1927, 2-6.

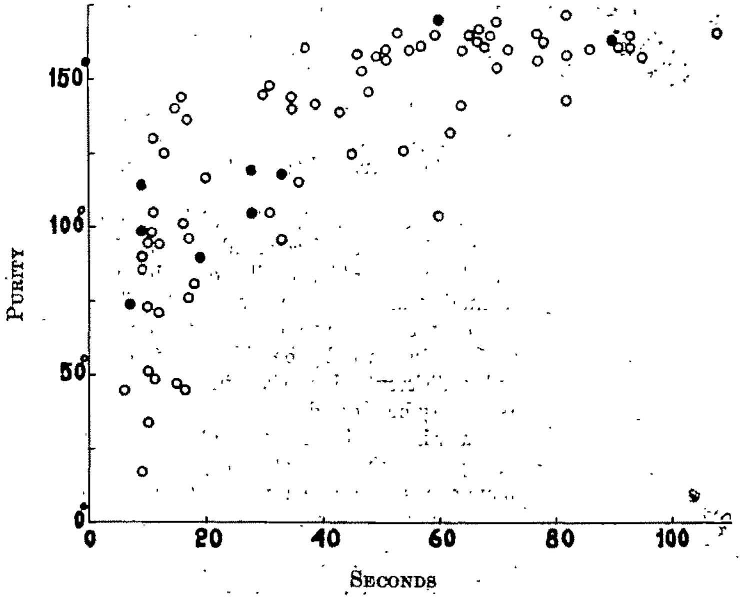

Fig. 1. Relation between Time and Degree of Adaptation to Pigment Red

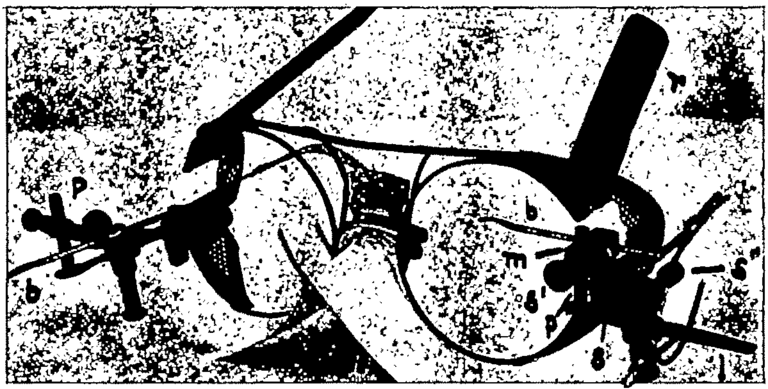

Fig. 2. The Ocular Speculum or Lid-Holder

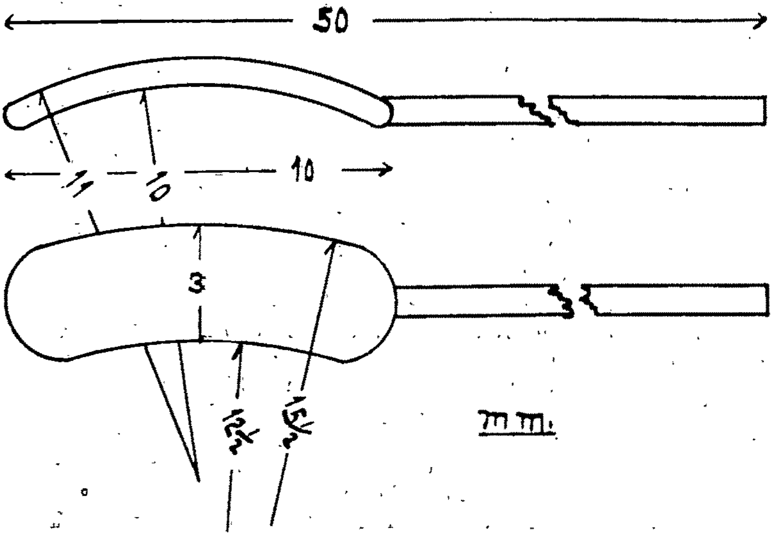

FIG. 3. LARGE SCALE DRAWING GIVING DETAILS OF THE BAR

FIG. 4. WINKING WHILE WEARING LID-HOLDER

here since, after all, the classification is quite arbitrary. Of these, the most effective means of eliminating lid movements is by drug injection which paralyzes the lid muscles. The levator which raises the upper lid, and the orbicularis which closes the eye, both must be rendered inactive. Drug injection is not to be recommended generally, however much it may appeal medically. It necessarily incapacitates *S* for periods before and after the experiment, it is inconvenient, particularly when *S* must serve repeatedly, and it interferes too much with the normal servicing of the cornea.

Drawing back the upper lid and holding it in place with surgeon's plaster is another simple means which is likely to suggest itself. But the repeated application of the plaster is somewhat trying and time-consuming, it is not effective unless drawn uncomfortably tight, and, unlike drug injection, spasmodic contractions may still occur and shift the position of the eyeball.

A more elaborate device for restraining lid movements, one which was designed particularly for that purpose, may be described in detail. This instrument might be called an ocular speculum or lid-holder, for it consists essentially of a small especially shaped bar for holding the upper lid clear of the pupil. This bar—one for each eye—is attached to a reënforced, blackened spectacle frame, and may be adjusted within the three spatial dimensions and swung quickly in or out of position. Ordinarily, for monocular experimentation, only the bar of the concerned eye is used. Fig. 2 shows the lid-holder complete, with one bar in the position it would occupy in holding the lid, and the other bar swung out of position. The large-scale line drawing in Fig. 3 gives the dimensional details of the bar for a right eyelid. It is made of brass and then given a coat of silver which is oxidized. This provides a surface which does not reflect much stray light, which is suitable for contact with the eyelid, and which can be cleaned with alcohol before use.

With reference to Fig. 2, the shaft of the bar, b, passes through a hole in the member m, and is held rigid in the hole by set-screw, s. With s loosened, the bar may be adjusted both laterally and rotationally. There is a second hole in m, permitting vertical adjustment upon the post, p. Member m may be set at any position on the post by simply tightening the set-screw, s′. That is the means for making the vertical adjustment of the bar. The post p is integral with a lever of the first class, l, by means of which *S* or *E* can swing the bar toward or away from the *S*'s cornea. On the side of the lever nearer *S*'s face, and bearing against the lever near the post, there is a third screw, s″. By means of this screw the closeness of approach of the bar to *S*'s cornea can be regulated. All three screws are provided with knurled heads, making a screw driver unnecessary, and adjustment by *S* himself possible.

Ordinarily *S* uses a mirror in adjusting the position of the bar. The instrument is first put on and the spring braces, r, set to give maximum rigidity of the frame. Then, with *S* looking straight into the mirror, the vertical adjustment is made by means of s′; next, by means of s, the lateral and rotational adjustments are completed; finally, with s″, the distance between the bar and the cornea is regulated. The positioning of the bar is of course all-important; it must be close to both the cornea and the projection of the pupil, clearing each by a millimeter or less. It should support the upper lid at the roots of the lashes. *S*, in making the last adjustment, holds his eye rather wide open and looks straight into the mirror as he swings the bar toward the eye. But screw s′ has first been turned toward the lever far enough to so limit its swing that the bar will not come very close to the cornea. Then, in a few successive trials, the screw is gradually turned back until at length the bar will just sufficiently clear the cornea when the lever is slowly swung up to contact with the screw. Once this stop-screw is properly set, the bar may be rapidly swung up to, or away from, the cornea in total darkness. To make the several adjustments quickly and satisfactorily is a matter of practice; if desired, all can be made without removing the spectacle frame. Good positioning becomes rather evident from the feel of the contacts. Two or three tentative winks will usually show whether or not the bar is close enough to the cornea to prevent the lid working down between them.

After the lid-holder has been adjusted, some objective test of pupillary clearance is desirable, under the experimental conditions and usually before and after the experiment itself. With the experimental intensity of illumination falling upon the eye, it is often sufficient for *S* to observe his own pupil in a mirror set up in the experimental line of sight at the proper distance. In some dark adaptation experiments it was found necessary to momentarily illuminate *S*'s eyeball in order that he might have sufficient light to observe the image in the mirror. Under such circumstances the bright light would temporarily narrow the pupil, and a greater clearance would have to be allowed between the lower side of the bar and the upper side of the pupil. Another case in which the bar must somewhat more than clear the natural pupil is when the stimulus subtends a large visual angle, for otherwise the bar might intercept the obliquely entering light. The appropriate use of an artificial pupil will sometimes render unnecessary the precaution of greater clearance. The bar itself may serve as mechanical support for an artificial pupil.

Trials of the lid-holder under experimental conditions indicate that if it is carefully adjusted it may be worn for as long as 10 min. without great discomfort. It has the advantage of affording a control not only upon the movement of the upper lid but also upon its resting position. Another advantage is that pre-experimental fatigue is minimized by the facility with which the bar can be swung into position at the commencement of the adaptation period.

The chief difficulty with the holder is that it cannot eliminate spasmodic contractions of the lid muscles. Even with the most careful adjustment these will occur at intervals throughout the period of restraint. If the synchronous ball displacements already referred to occur along with these spasmodic contractions, fixation is upset and the value of the holder may be considerably reduced. The point was tested by setting up a neon glow lamp in a dark-room at 5 meters from the *S*. *S* estimated the amount of displacement of the lamp image under various conditions, particularly when wearing the lid-holder or not wearing it. By reference to a series of vertical stripes placed 1° apart, *S* could make his estimates in angular terms. A few results from 5 *S*s seem to indicate that the image displacements in the case of the restrained lid movements were of the same order of magnitude as in the case of the full voluntary wink, *i.e.* somewhat less than 1°. This is a favorable indication because the displacements of the neon image must more nearly represent the full ball movement when the eye was constantly held open by the holder than when, in ordinary winking, the eye closed while still moving. If the image displacements were of the same order in both cases, the actual ball movement when the lid-holder was worn was probably less than the movement during the ordinary unrestricted wink or blink. The holder, then, appears to reduce ball movement even though it cannot eliminate the muscle contractions.

But in view of the large displacements of 10° to 15° recorded by Miles during unrestrained lid movements, it seemed imperative to secure more objective information concerning displacement during movements restricted by the holder. Therefore several hundred frames of motion picture were taken during which *S* winked suddenly and rather vigorously a number of times.[6] These winks were at least as intensive as those which occur spasmodically while the lid-holder is being used. The film was examined under magnification to find frames in which winks had occurred. Since the left lid only was restrained by the bar, more or less droop of the right lid could indicate the crucial frames. That frame showing the maximum droop of the right lid caught by the 16 mm. camera, and the frames immediately preceding and following, were enlarged photographically, and sections showing the eyes are reproduced in Fig. 4.

The important point is the position of the eyeball in the middle frame as compared with its position in the preceding and succeeding frames. Measurements from the edge of the iris to fixed reference points on the lid-holder and forehead show no vertical displacement whatever! There does appear to be a slight nasalward movement. But allowing for error in measurement, any actual ball displacement here could hardly have amounted to more than 1° or 2°. According to this test the lid-holder not only restrains the lid satisfactorily but also reduces ball movement to the point where even relatively small stimulus areas are feasible. The latter is particularly true if there is not much retraction or backward displacement of the ball. There is known to be some backward displacement during unrestricted lid movements, but one cannot tell from the motion picture its extent when the lid is held open. The upward turn of the lashes in the middle frame indicates added pressure of the lid on the holder during the restricted wink. This and the somewhat smaller area of eye-white visible do make it seem probable that the ball has been pushed backward slightly.

The instrumental controls mentioned up to this point aim to actually restrain or render impossible the obstructing lid-movements; they are devices which more or less successfully prolong the non-blink span. There are, on the other hand, a number of devices which serve as controls by indicating or recording the lid movements when they do occur, but without interfering with them any more than necessary. Sometimes these may be useful for indicating slips in procedure, *e.g.* failure of *S* to keep his eye open during a non-wink period. Sometimes *E* may want *S* to blink normally during the course of the experiment and at the same time desire, either as a check on its normality or to discover what normality may be, a record of the blinking. There are, too, investigators who feel that no restraint or inhibition of the ordinary lid movements should be exercised during any visual experiments. They assert that the data are not typical unless secured when the organism is functioning naturally.

One of the simplest indicating controls is the signalling of *E* by *S* when the latter makes a lid movement. The signal may be executed orally, by the tap of a pencil, or by pressure of a key. In a series of observations in which the *S* could not know that his blinks were being counted, Almquist and I employed the silent key elsewhere described.[7] One *E* stationed beside *S*, and holding the key in his hand, depressed it at each observed blink. The other *E* in an adjoining room counted corresponding clicks of a single-stroke buzzer, the buzzer being connected in series with the key and a dry battery. Were the temporal spacing as well as the number of blinks required, the key could be connected with a kymographic marker instead of the buzzer.

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[6] This extraordinarily clear motion picture was taken by Professor H. M. Halverson.

In some of the observations just referred to, *S* was necessarily in darkness, and therefore invisible. Narrow strips of surgeon's adhesive tape were cut from a roll and coated with luminous paint. One strip was stuck lightly on the upper lid at the roots of the lashes, and the other was attached above the cheekbone. The luminous strips were visible in darkness, and change in the distance between them served to indicate lid movement.

Peterson and Allison state that "a little training enables one to count one's own blinks rather semi-automatically, using a stop-watch." We also found this to be a usable procedure. As already suggested, one person may count the blinks of another, either with or without his knowledge. In doing that, however, there is a source of error which might tend toward underestimation of blinking. The fact that the counter blinks as well as the *S* presents the probability that at some times they will blink simultaneously, or nearly so, and the *S*'s blink will not be seen. If the assumption be made that *E* and *S* both blink irregularly at a mean rate of 20 blinks per min., and that the blinks are about 200 σ in duration, then theoretically the probability of *any* degree of overlapping in the blinks of *E* and *S* is only about 0.0067. In other words, the two blinks could be expected to overlap more or less on an average of once every 150 blinks or once every 7.5 min. of adaptation time. The chance of sufficient overlapping to prevent *E* from noticing *S*'s blink might be even less, and in any event the resultant error would be negligible. This strengthens an observation of the authors cited above that when several counters are employed their results are in close agreement, and the accuracy of the method is demonstrated to be satisfactory.

There are a number of less simple techniques in which the lid makes its own graphic record. These appear less generally useful for adaptational purposes than controls previously described; but some of them may be mentioned. Exner's is among the earliest. He glued a thread to the lid and attached the other end to a delicate lever which wrote upon a revolving drum. Mayhew attached platinum foil to the lid in such a way that movement of it would break an electrical contact which would actuate a magnetic recording device. Cason used a combination of lever and electrical contact, while Peterson and Allison mounted a similar arrangement upon a specially devised spectacle frame. Garten photographed a spark displaced by lid movement. Miles photographed light reflected from minute mirrors, attached to the lids. Dodge took pictures of the excursion of an artificial lash of writing paper, 1.5 × 20 mm., affixed to the lid. He and Miles used a touch of gum arabic for attachment. Scientifically, the photographic techniques are the most satisfactory. The others set up situations so artificial that there is some question whether or not *S* becomes sufficiently adapted to them for such representative results.

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[7] Newhall, A silencer for laboratory keys, this JOURNAL, 41, 1929, 646-7.

Stimuli to blinking. Various studies and common experience suggest that almost any intensive or unexpected sensory stimulus may elicit a blink. Consequently it behooves *E* who is trying to arrange for a long non-blink span or for regularly spaced winking to eliminate such stimulation. Ponder and Kennedy and others believe that even in the absence of special external stimulation the ordinary blinking goes on through automatic impulsion from a center in the striate body, unless, of course, it is voluntarily inhibited. According to Peterson and Allison, this blinking occurs even in the case of glass eye or blindness, and the spasmodic contractions continue for a time even after the eyes are closed. An obvious thing to do, then, in attempting to prolong the non-blink span, is to control the controllable stimuli and so allow *S* to concentrate voluntary restraint against these otherwise uncontrollable excitations. To this end something may be written concerning stimuli or situations about which something is known and which have likelihood of being encountered in visual experimentation.

The effect of type of activity of *S* upon the blink rate has been investigated. Almquist made about 40 observations each on 4 men students without their knowledge. Whenever he happened to be near one of these individuals the investigator counted the man's blinks and also noted the type of activity, viz. reading, talking, or simply sitting. The mean blink rates during sitting and talking were almost identical, but over 20% greater than during reading. Peterson and Allison also found blinking to be more frequent during 'not reading' or 'unoccupied' than during reading; indeed, of 5 activities studied by these writers, the highest blink rate occurred when *S* was 'unoccupied.'

Another of their results which is of considerable interest from the standpoint of visual experimentation concerns the distance of fixation. Seventeen *S*s gave a mean interblink period of 13.12 sec. with near fixation, as compared with 21.06 sec. during distant fixation. The corresponding values in terms of blinks per min. are 4.57 and 2.85 respectively; and their difference is statistically significant. It seems unfortunate that *S*s should have to blink so frequently during near fixation and when relatively unoccupied, because these are common conditions of the adaptation experiment. Possibly the adaptation procedure can sometimes be arranged to provide for distant fixation and some preöccupation of the *S*.

There is a paucity of data concerning the precise effects of simple sensory stimuli upon blink rate. Of all those that might affect blinking only one or two can be mentioned here. Light and change in intensity of light seem important because so frequently present in adaptational procedure. A simple test was made of the effectiveness of light, using 4 *S*s and the general procedure following. *S* was admitted to a darkroom and dark-adapted. Then for a period of 10 min. his blinks were counted in consecutive 15-sec. groups, using the darkroom technique for counting already described. During the first 2.5 min. of the 10, *S* was still in darkness with his head in a rest; during the second 2.5 min. he was exposed to a 6° disk-field of a brightness of over 1000 ml. and during the final 5 min. *S* dark-adapted.[8]

Fig. 5 exhibits average results based upon all the data; the ordinate gives frequency of the blinks per min., while the abscissa shows the three conditions just mentioned. The jagged lines connect the 15-sec. points and show the course of change in blink rate during the experiment. Particularly interesting is the fact that the blink rate is high both at the beginning of the light period and the succeeding dark period; just why it should also be rather high at the beginning of the first dark period is not clear. though possibly that reflects some habituation to the headrest. In the beginning of each period there is a rapid fall in blink rate to a lower more stable level which persists throughout the remainder of the period. This means increased blinking on a sudden and considerable change in the intensity level of illumination, with consequent rapid habituation. The horizontal bars in Fig. 5 represent the general mean blink rates as under the three respective conditions. Evidently under the intensive light stimulation the blinking does not drop back to the average under the dark conditions, but maintains a higher rate for the 2.5 min. of exposure. Furthermore, the level during the final dark period has not in 5 min. reached that of the first dark period, though we should expect it to do so eventually. In general, the results are about what one might expect, but the data are insufficient to be more than suggestive. They suggest the wisdom of special vigilance in guarding against errors due to blinking in the early stages of light adaptation, the stage when it is progressing most rapidly.

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[8] Dr. T. W. Cook supplied the stimulation for this experiment, and also helped in other ways.

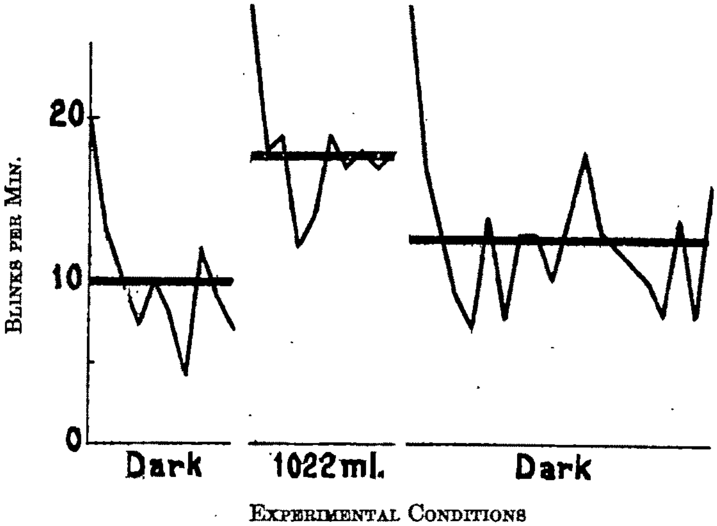

FIG. 5. AVERAGE BLINKS PER MIN. DURING EXTREMES OF LIGHT AND DARKNESS

Dryness of the cornea has been referred to repeatedly as characteristic stimulation for the reflex lid movement; but probably no more emphasis should be laid upon its efficacy than upon that of heat or many other factors. Some light might be thrown upon the question of the effectiveness of the aqueous condition of the outer surface of the cornea, it was thought, by studying the possibility of variation in the blink rate under various degrees of temperature and humidity. Exposure of the corneal surface to high humidity would naturally ensure greater moisture than on hot dry days. According to the old idea, if other things were equal there should be less blinking on the moist days.

A record was kept of the blinks per min. of one *S* on 20 different days, running over a period of weeks. Each day when the blinks were counted, the temperature of the surrounding atmosphere was taken in degrees Fahrenheit and the absolute humidity was measured in grains per cubic foot. The ranges of temperatures and humidities thus secured were not particularly wide; but the readings were taken in the laboratory and might be said to constitute working ranges in the sense of being values likely to be found during actual experimentation. The temperatures ranged from 60° to 77° and the humidities from 2.5 to 4.6 gr.

Table V gives the Pearson and partial correlation coefficients for blinks per min., (*b*), temperature (*t*), and humidity (*h*). Whereas blinking and temperature show an ordinary correlation of 0.64, blinking and humidity only correlate positively to the extent of 0.28. When the third variable is held 'statistically constant' by the conventional Yulean process, the one coefficient remains about the same while the other is reduced to around zero. Thus it would seem that coolness is more important than moisture. But these results are inconclusive and show no marked tendency in the ranges of variation ordinarily encountered in the laboratory building. If more cases and longer ranges were employed, it is possible that the coefficients would still hold their opposed signs and increase in size.

TABLE V

CORRELATIONS BETWEEN BLINKS PER MIN., ATMOSPHERIC TEMPERATURE, AND ABSOLUTE HUMIDITY

	b and *t*	*b* and *h*	*t* and *h*
Zero order................	.64	.28	.53
Partial....................	.60	−.06	.47

Air movement in the region of the eye seems to favor blinking and also tends to reduce the non-blink span. In one instance a rotating electric motor in some optical apparatus produced a slight draft in the vicinity of the cornea. Elimination of this air current seemed to produce increased comfort and greater ability to refrain from blinking. Similarly, air currents rising from a radiator and passing by the face may have a disturbing effect.

If there were a time of day during which the *S* did not have to blink as much as at other times, the *E* might be glad to know of it and to schedule his sessions accordingly. Neither in the case of the blink rate nor the ability to refrain from blinking have we been able to discover any marked tendency in that direction. Table IV above gives hourly values for the non-blink span from 8:00 A.M. to 10:00 P.M., and they indicate nothing definite save possibly some falling off from the first hour or two. A similar table, which there seemed to be no particular point in publishing here, shows the mean blink rates of the same three *S*s for each hour. The only indication is a slight and very irregular tendency for the rate to rise through the day. Further data might uncover something more definite; there may be optimum times for individual *S*s, or there is the possibility of rhythms in blink rate or non-blink span.

Concerning the question of choosing *S*s, a few words may serve to conclude these notes on lid control. Appropriate selection of *S*s contributes to control, as much as employment of special expedients and instruments. Whereas that combination of procedural and instrumental controls which will be most appropriate depends very largely upon the particular problem at hand, the principles for selecting *Ss* are relatively few and constant. Of course the investigator is after *Ss* whose vision is satisfactory, and who do not blink often, or who can refrain from blinking for a long time. The great individual differences suggest at once the desirability of giving prospective *Ss* a preliminary test to determine whether or not they are relatively proficient. The test should preferably be under the actual experimental conditions. There are *Ss* to be found, Geldard says, who without much discomfort can refrain from blinking for minutes.

Other things being equal, it seems well to reject heavy-lidded candidates in favor of those who naturally keep their eyes rather wide open. If there is observable space between the upper lid and the top of the pupil, so much the better. This is to avoid the likelihood of the upper lid partially obstructing the stimulus, and to make the wearing of a lid holder, if necessary, more comfortable and effective.

Age and sex appear to have some influence upon blinking, as indicated in the results of Peterson and Allison. Blinking tended gradually to increase with age, and women seemed to blink more frequently than men, on the whole. Where otherwise feasible, this of course favors the use of comparatively young male *Ss*. Emotional stress sometimes has a marked influence upon the blink rate; obviously, then, it is well not only to select *Ss*, but also to arrange conditions in which such stress is not likely to arise.

A VARIABLE-PATTERN PAPER MAZE

By D. F. SHOWALTER, Arkansas State College

The maze shown in the accompanying figure can be made on a mimeograph so that a large variety of forms may be produced at a small expense. The pattern shown is a ten unit, T-type maze with nine culs-de-sac. The correct path, which is shown as a solid line, is found by connecting the numbers from zero to sixty in order. The culs-de-sac are shown with broken lines. The path can be varied by changing the length of the lines, or the order of changes in direction, or by following an irregular route and returning to the starting point. A further modification may be produced by having either the letters of the alphabet in order or a single letter from a path through a maze of letters.

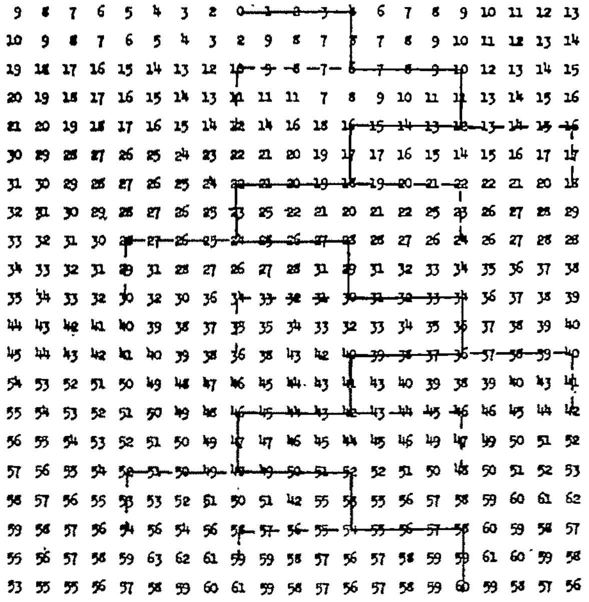

FIG. 1. THE VARIABLE-PATTERN PAPER MAZE

The path may be traced with a pencil or, if a carbon paper is used, with a glass rod. The glass rod leaves a faint mark which, though discernible, does not allow the pattern to be seen as a whole. This mark is entirely eliminated when the tracing is made of a sheet of transparent celluloid laid over the maze.

NOTES AND DISCUSSIONS

Are Emotions Teleological Constructs?

Nearly forty years ago William James married the emotions to the instincts so solidly that a divorce seemed to be impossible.[1] James' work was indeed but the giving of official sanction and due publicity to a union which had been tentatively announced by Charles Darwin in his *Expression of the Emotions in Man and Animals.* Apparently, the indissoluble bond has since been dissolved; but as a matter of fact, it is the death of one of the contracting parties that has broken the bond. Instincts have quietly passed away after a brief and feverish illness, and the widowed emotions have been left.

The detailed exposition of James resulted in a general acceptance of the scheme of emotions as organic results of responses. In so far as responses occur as unlearned patterns of specific and unique types, that is, as the manifestations of 'instincts,' we should expect 'emotions' of equally unlearned and unique types, capable of being named distinctively. In so far as responses are modified by the learning process, we should expect emotions correspondingly modified.

This analysis of the problem, indeed, seems as sound today as it did in 1882. If there is, for example, an instinct which may be called *flight* which manifests itself in different ways in different circumstances, but always in such a way that it is recognizable as *flight*, and definitely discriminable from any other instinct, such as the fighting instinct; then there should be a corresponding emotion, which we might call *fear*, unique in its nature and generically distinct from any other emotions such as 'anger.'

This doctrine, prior to 1918, was so well established that social psychology was said to be 'founded' upon it. It was so thoroughly ingrained into general psychology that the first behaviorism, although soft-pedalling on 'the emotions,' based itself upon 'the instincts' without an apprehensive question.

Then something happened. When I expressed before the American Psychological Association in 1919 my doubts concerning the reality of 'instincts,' my whole point seemed as fantastical to all but two or three of the members present as if I had expressed scepticism concerning the existence of spectroscopes. In a very few years, however, 'instincts' were banned from American psychology, and the revolt against instincts has even gone to the unwarranted extreme of putting a taboo on such perfectly good concepts as 'instinct' and 'instinctive,' which are really not bound up in the old 'instincts' at all.

Only a few psychologists, at the present time would base anything on 'instincts.' Apparently, the doctrine that 'an instinct' is nothing more than an arbitrary classification of responses in accordance with external results or external situations in which certain results *may* be accomplished, (*i.e.* a teleological class-name); and that even the results by which classification is made are arbitrarily selected by the classifier; is generally accepted except by those who hold to a spiritual doctrine of 'instincts' as energies put out by a substantial soul.

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[1] Presented before the Southern Society for Philosophy and Psychology, New Orleans, December 30, 1931.

After such a revolution, a logician might expect that emotions as uniquely different facts would have disappeared along with the 'instincts' to which they were obviously bound. This result, however, has not followed. Psychologists still talk of this 'emotion' and that 'emotion' as if they were *sui generis;* and the behaviorist has newly established behaviorism on the foundation of a few unique 'emotions' from which others are developed; the foundation which had been demolished by psychology being thus replaced by more of the same material.

I have from time to time suggested that the situation is illogical. I want to insist that it is absurd. 'Emotions' can be named: we can speak for example, of anger, fear, and grief. But these names represent no psychological entities, no unique affective processes. They are names applied to varying complexes in the affective content, and they are teleologically chosen. The search for 'primary emotions' is as much an anachronism in psychology today as is the search for the soul: and it is a search of the same sort. We must face the fact that the 'emotions' are names to which correspond no concrete realities, but designate abstractions of the most arbitrary type.

Let us consider 'fear' as an illustration. What is 'fear'? Its outward expression, we already realize, runs the gamut of responses. It may be manifested in flight; in immobility; in fighting; in caressing; in anything. On the other hand, the same outward expressions are characteristic of many emotional conditions. One fights when angry; when afraid; when in love. The responses to different external situations may become somewhat differentiated with advancing age: but the baby makes the same response to being dropped; to having a pin stuck in him; to being deprived of his bottle; to a violent shrill sound. No one but a behaviorist would be so rash as to conclude, with no evidence whatever, that the baby's 'emotion' is the same in all these cases. But, one will say with justice, these outward expressions are of slight consequence. It is true that fleeing 'fear' is different from fighting 'fear.' But the difference is unimportant as compared with the inner (bodily) states and processes. That, it will be said, is the same in all 'fear.'

Now just what *are* these inner states? I do not doubt that there are inner states: states of the muscles of the trunk, arms, limbs; states of the alimentary canal; states of the other tissues. I do not doubt that these states are experienced: that they are stimulus-patterns which initiate responses. But are they the same, or measurably the same, in all 'fear?'

So far as objective experiment goes, the answer seems to be: *No.* Further; the states or processes which do seem somewhat constant are also found in 'emotions' of quite different names. Shall we turn to introspection? Then the negative is still more emphatic. Different 'fears' do not feel the same. Not even 'fears' which arise in apparently similar circumstances.

So far as introspection goes, two 'emotions' of the same name are often more different from each other than they are from other 'emotions' of officially different names. This is not conclusive: introspection raises problems, never settles them. But the problem raised in this case will not down easily. If it is ever to be solved, experimental attacks must be devised to fit the problem, and not shaped along the lines of pseudo-problems. Emotion, however, is a fact quite aside from questions about 'the emotions.' We do get 'stirred up,' 'moved:' we *feel.* All introspection agrees on this point. What, then, do the names of 'the emotions' signify?

Nothing more than the situations in which we are emotional. In one circumstance, there is a threat or danger. Something, some person, some occurrence, threatens to limit me, to injure me, to deprive me of something I value. This is a definite constellation and the emotion I experience in this situation, I call 'fear;' although the actual emotion may vary. But hold, wait a bit: I do not always call it 'fear.' Sometimes I call it 'anger.' Yes; but then certain features in my environment are emphasized, which are less conspicuous in the situation in which I call my emotion 'fear.' If a person threatens me, and I emphasize his part in the portended happening I may still 'fear' him, if I admit his greater power. Or I may be 'angry' at him. It all depends on the way I think about the situation, not on the actual state of feeling. In still other ways of apprehending the situation I may call my emotion 'resentment.' In still others I call it 'grief.' It is not for nothing that we may sob in any of these cases and even in 'joy;' for 'joy' is merely the name we give to any emotion which occurs when we are specifically conscious of additions made to life. The vital differences, so far as feeling itself is concerned, are those between calm 'joy' and ecstatic 'joy;' cold 'grief' and frantic 'grief;' chilly 'fear' and frantic 'fear.' The differences otherwise are minor. Frantic 'fear,' frantic 'joy,' frantic 'grief' are much more alike, as emotional states, than frantic 'grief' is like to depressed 'grief,' frantic 'joy' to quiet 'joy,' or excited 'fear' to cold 'fear.' That in which the various 'fears' resemble one another, the various 'griefs' resemble one another, and the various 'joys' resemble one another, is in the likeness of the situations in which these states arise, or rather, in the perceptual and ideational apprehension of these situations.

To recapitulate: The emotions as classified in daily life, and in our archaic psychology are classified and named in accordance with our estimation of the situations in which they arise. I may estimate a situation today as different from an extremely similar situation tomorrow. Then I call 'the emotion' in the two cases by different names, corresponding to the situations as estimated or apprehended.

I may apprehend my situation primarily as one in which my welfare is threatened. I call my inner responses *fear*. You may apprehend my situation as one in which I fight against the threat; you say I am *angry*. Which is correct? The answer may be: Both! You name my emotion correctly in so far as you connect it with any factor in the environment which is really efficacious in stirring me up, or any actual intellectual attitude of mine towards the environment, or on any demonstrable result of my emotion.

It is no wonder that experimental attacks on the problem of emotions have produced no results. Our problems have been formulated in teleological terms, although we have supposed we were studying emotion as such. Our methods have been determined by our misconception of the problems. We have eagerly sought, for example, for characteristic organic symptoms of the conventionally named 'emotions,' overlooking the fact that the differences for a single so called 'emotion' should be expected to be as great as between two differently named 'emotions.' We have asked patient observers to report their 'emotions,' and have been misled by not knowing that the language in which the reports are given has mainly an environmental significance, and secondarily a cognitive reference.

In studies of the 'personality' type, we gravely record such statements of the patient as that he feels 'more cheerful' today; that he feels 'depressed,' or 'hopeful,' etc., never suspecting that the reports have little bearing on the actual affective state, but signify primarily the patient's judgment of situations. As indications of the patient's intellectual attitudes, these reports are of great value. Even the answer to the question: "Do you feel depressed?" may be significant, in spite of the fact that the same person will answer "yes" this week and "no" next week, but the significance is intellectual and not emotional.

I have been much impressed by the statement, in a recent report, of the method employed and results obtained in ascertaining employees' emotional states. The investigator first guessed the individual's state on the basis of his behavior. Then the individual was asked to report his state. If the two characterizations agreed, the rating was accepted as valid. If they disagreed, the individual was questioned later in the day, when it was usually found that his report agreed with the investigator's judgment!

In various ways, we find individuals tending to report their emotional states in terms of the ways they think they should feel in specific circumstances, as well as ways which they deem appropriate to the external conditions (which do not always amount to the same thing). Even when verbal report is ruled out, and other types of responses are interpreted, the interpretations are confused by the fact that the same response means different things at different times.

When a person is whistling lively tunes at his work, is he intrinsically 'cheerful'? Unless by 'cheerful' we mean merely that he whistles, the significance of his behavior is ambiguous. Often he is depressed. But usually, he thinks he should be cheerful, in a more inner sense, or that he should be optimistic, bucked up or looking on the brighter side. The determinants of classification seem in the predominant number of cases to be not mainly the actual inner feeling, but the external circumstance, and the individual's judgmental attitude toward the circumstances.

In another direction our confusion has been more deadly. We have, during the last two decades, more and more emphasized the emotional factor in life, at the expense of the cognitive. We have insisted, for example, that the perceptual and ideational qualifications of the child are not paramount: that his emotional attitudes in the home, in the school, among his mates, are of profound importance in determining his development, his learning, his adaptations. We have made a great pother about this discovery, in education and child guidance, and have created over night a lot of 'experts' in directing the child's emotional development. But when the results are checked, we find that little progress has been made, and that little in defiance of the theories. We are really dealing in this field with matters of understanding, judgment, and thinking generally. Solomon is justified in his maxim: practical success has been attained by getting the child to understanding his situation, to think accurately, and adopt healthy ideals. As we drop the ballyhoo about 'emotional attitudes' and face the problems as they actually are, we shall make still more progress.

There are emotional states or processes (whichever you choose to call them). These emotional items, moreover, actually do vary. Neither of these statements can be proved: but they are good working hypotheses. That there are different 'emotions,' discretely distinguishable in kind from one another, aside from their cognitive associations, is a different hypothesis, which time has shown to be sterile. The more plausible hypothesis is that the variation is of the type which would occur in a complex in which there are *n* elements, any one of which may vary in a graded way, so that the total variation is poly-dimensional but without discriminable steps or jumps. The value of *n*, and the nature of the elements, if any, remain to be determined. It is safe to say, however, that if affective elements are ever discovered, they will not be items to which we would apply the conventional names of 'the emotions.'

It would seem that the best attack on the problem of emotion (not 'the emotions'), should be through the study of actual variations, regardless of emotional names. As a matter of fact, beginnings have been made in this line, It is unfortunate that we have to make these beginnings through classification; but if we hold resolutely to the fact that classifications are only classifications, we may even so make classification a useful servant.

We have noted that certain emotions, regardless of names, are pleasant, others unpleasant; some are exciting, others are the reverse. There are obviously dimensions of emotional variation which are by no means independent of external conditions and results, or of ideational processes; but which we can actually study as psychological features, collating them with cognitive factors, and not confusing them. Perhaps if we forget the conventional teleological classifications, and actually study their obvious dimensions, we shall learn something about them and discover other dimensions.

Such study as I urge must of course be psycho-physiological, rather than neurological. If we change the terms slightly, the James-Lange theory of 'the emotions' vanishes; but the modified James-Lange theory of *emotion* remains a permanent contribution to the armament of psychology. There is neither hope nor good sport in investigating emotions except as organic or bodily states: products of response to the environment, not intermediaries in response. The sooner we recognize that the traditional 'instincts' and traditional 'emotions' were really yoked together and that the passing of 'instincts' left 'emotions' so forlorn as to be hardly serviceable even for behaviorism and other reactionary psychologies, the sooner will our study of emotion become productive.

Johns Hopkins University KNIGHT DUNLAP

INSIGHT AND THE CONTEXT OF GESTALT THEORY

In the April number of this JOURNAL,[1] Ogden presents an attractive defense of the notion of insight from the standpoint with which he has identified himself. With the greater part of his discussion I am in substantial agreement. During the course of his exposition, however, he disposes very swiftly of ten points which I had raised in an earlier attempt to deal with some of the difficulties which the doctrine of insight creates for the middle-of-the-road psychologist.[2] A number of these questions, in fact, are summarily dismissed with the brief statement that they fall outside "the context of Gestalt theory" and are therefore meaningless, uninteresting to the configurationist, and devoid of fruitful experimental possibilities. Since the limitations of space forbid detailed answers to all points, my reply will be confined to the more outstanding instances of disagreement.

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[1] R. M. Ogden, Insight, this JOURNAL, 44, 1932, 350-356.

[2] G. W. Hartmann, The concept and criteria of insight, *Psychol. Rev.*, 38, 1931, 242 f.

As I see it, the major weakness in Ogden's position is expressed in the assumption that any critique of the Gestalt position, either in general or in detail, is null and void unless it first accepts the theoretical postulates upon which it is based. To maintain that a certain positivistically-worded logical or experimental problem has no connotation within the "context of Gestalt theory" is to evade the issue by merely denying its pertinence to psychology. The question is: Does the particular problem fall within "the context of psychology as such," or of "science as a whole," or even of "human experience in general"? No contemporary technician feels alarmed when one asks if the hydrogen ion concentration of saliva is raised or lowered during emotional stress, despite the fact that three conceptually distinct worlds of chemistry, physiology, and psychology are involved. Then why reject as illegitimate the perfectly harmless and rational first inquiry, "Are specific insights the outcomes of prior trial-and-error?"—especially when the contrasted principles of behaviorism and configurationism are both comprehended within the universe of psychology? To assert this seriously would work havoc with all efforts at reconciling opposing hypotheses within our discipline. Such a stimulating possibility as Watson's view that thought is simply the action of laryngeal mechanisms—to mention but one among thousands of similar 'fruitful' proposals—would never have appeared had Ogden's view prevailed. Surely no Thomist or existentialist could brush aside this question with impunity because it fell beyond the scope of his own private theory or framework of principles. Instead, the problem has generally been recognized as interesting and valid in its own right. Had I asked, "Do kittens write textbooks with left-handed clam shells?" then it would have been proper to say that this statement falls outside the "context" of everything except the universe of nonsensical language or of Alice in Wonderland.

Turning to the second statement, I believe the configurationists hold that *all* learning requires some insight—a bold assertion of universality which sorely needs confirmation. Insight implies a sense of the fitness and inevitability of the means for obtaining a solution. Yet what appropriateness does a child experience when it learns that pressing a button produces light—especially when the same effect can be secured by pulling a string or turning a wheel? Thorndike taught his cats to escape from confinement by scratching themselves. Can a more arbitrary or less insightful sequence be imagined? Nevertheless, learning in the broadest sense of attaining a goal certainly occurred.

With respect to the third item which concerns the best single criterion of insight, I am inclined to agree with Ogden that the discrimination of adequate means for attaining a desired end constitutes a valuable "earmark" of the process, although (to paraphrase another one of his statements) 'it presents more of a problem than a solution.' Numerous and disparate criteria of insight have been suggested as any casual survey of the literature shows. If that be the case, why is not the question of the most significant index of the process—as measured perhaps by the degree of correspondence between external event and inner experience—both conceivable and profitable?

My fourth question asked, "Is insight necessarily accompanied by ideas?" Ogden rejects this phrasing as implying a needless loyalty to an outworn creed, but I see no reason why a catholic psychologist of today should hesitate to avail himself of introspective data in the interests of a richer description of the event, even if these accessories do fall outside the pale (or "context") of configurationism! The use of the term "ideas" is not intended to convey any theoretical bias; it is probably most safely employed in the general Lockian sense. What is said here applies also, *pari passu*, to Ogden's comments on the fifth and sixth items of my original list.

So far as the seventh problem is concerned, I am inclined to believe that a careful examination of *bona fide* cases of "association by similarity" may afford some of the finest testimony to the reality or unreality of the insight factor. Failure to appreciate this is very much like cutting off one's nose to spite one's face. A flat refusal to equate old and new terminology, just because the former does not fit the context of Gestalt, would involve turning one's back upon a great historical tradition, a step which Descartes could not literally take even though he tried. Most of us are happy to pour old wine into new bottles whenever an opportunity permits, as in Jaensch's suggestion that the eidetic image may have been genetically the transitional mediator between the "impression" and the "idea" (in Hume's sense).

The solution of some of these problems as I have stated them may demand a greater reliance upon narrower introspective procedures than many would be willing to allow. But the Gestalt theorists ought to be the last to protest on this score. Whatever differences there may be between introspective reports and phenomenological analyses (Wertheimer's historic paper of 1912 is a classic example of the latter) are probably not of sufficient import to affect the outcome of any investigation employing either or both procedures—or to prevent one from going ahead with the study.

Pennsylvania State College GEORGE W. HARTMANN

HELSON AND FEHRER ON "THE RÔLE OF FORM IN PERCEPTION"

Helson and Fehrer point out in a recent number of this JOURNAL,[1] that the problem of form has long been an important issue in theoretical and experimental psychology; but in recent times, this problem has assumed definite significance because of the work of the configurational school. This school emphasizes the view that perceptual and bodily responses of the organism *form* a whole, a unit, or configuration. Helson and Fehrer criticize this view, asserting that the configurationists have over generalized and over emphasized the rôle of form in perception. They also affirm, on the basis of their results, that circular forms are not preferred to squares and rectangles. Hence they are led to disagree with Köhler's hypothesis regarding preference for the 'simplest' symmetrical forms. According to their experimental findings, the perception of light at the absolute threshold for vision is formless, and only at higher levels of brilliance are forms just perceived; at still higher illuminations the certain form of the stimulus emerges.

This study stimulated the writer to reëxamine experimentally the problem of form. The writer is convinced that Helson and Fehrer's experimental work and theoretical conclusions leave much to be desired in a scientific attack upon the problem. The evidence advanced by them that forms are not first or primary in perception seems inadequate and misleading.

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[1] H. Helson and E. V. Fehrer, The rôle of form in perception, 44, 1932, 79-102.

If Helson and Fehrer's conclusions cannot be admitted, the problem remains, "What is the actual relation of the perception of form to the lower limen of vision?" Experiments carried out by the writer seem to answer this question. In the course of these experiments, it became evident that the perception of light at the absolute threshold for vision is *not* formless as Helson and Fehrer assert but that it is formed or organized in a whole with definite figure-ground properties. Thus, there is definite evidence that the correct explanation of the rôle of form in perception lies in quite a different direction from that given by Helson and Fehrer.

In the first of the writer's experiments, 4 *Os* were used. *O* was seated in a dark room before a test object in which different figures (squares; rectangles; triangles; L-shaped figures; jagged, irregular figures, and semi-circles) of the same approximate area had been cut. The figures were observed through a small artificial pupil. Four red dots served as fixation points. The visual angle subtended by the open figure fell well within the fovea. Monocular vision alone was employed. A reflecting screen was used which allowed independent illumination of the figures. Starting with an illumination which fell below the threshold of visibility, the brightness of the figure was gradually increased to a just perceptible brilliance. *O* was then instructed to draw and describe the appearance of the perceptual field. Invariably, the *Os* drew a lighted circular form on a dark ground, and described the figure as a circular area of light on a dark ground.

A modification of this experiment consisted in independently illuminating the area surrounding the figure. Here the figure was dark. In this case, the *Os* reported the appearance of a dark circular area on a lighted ground. There is thus positive evidence that light at the absolute threshold for visibility is 'formed' rather than 'formless.'

In another experiment, the surface of the test object surrounding the figure was illuminated and the brightness of the test object increased until the field appeared a uniform brilliance. It was then possible to investigate the emergence of a lighted figure on a ground of lower or higher brightness. The results of this experiment show that a figure emerging from a uniform ground of brightness appears first as circular in form. In still another experiment, the test object was greatly reduced in size by the use of a lens and *O* was required to draw and describe the appearance of the test object at the extreme limit of visibility. Again the figure was reported as a round lighted object on a darker or brighter ground. Further experiments, this time on the differential threshold for form where figure and ground were of different colors (hues), brought out the same results.

The assertion of the configurationists that forms are first and primary in perception finds experimental verification in this series of experiments. Helson and Fehrer appear to have confused form as assumed in the stimulus-pattern with form as it exists in the perceptual field. Figures objectively contoured and perceived as formed constitute only a small percentage of formed experiences. A bias of the *O* toward the stimulus-pattern (stimulus-error) —looking for 'objectified' form, rather than for form in the perceptual field as such—would explain Helson and Fehrer's results. The writer has been experimenting in vision intensively for several years and has yet to find in his own experience or that of his *Os*, an 'un-formed' perceptual field. By no means will the form seen and the form of the stimulus-pattern always agree, a point the potency of which Helson and Fehrer have apparently overlooked.

University of Kansas WARREN W. WILCOX

"A QUESTION OF ETHICS:" A REJOINDER

In the April number of this JOURNAL,[1] Willis D. Ellis offers several criticisms of my introductory chapter to the *Listener's Notebook* prepared for the first series of broadcasts sponsored by the National Advisory Council on Radio in Education. Ellis' minor objection seems to be that my chapter contained erroneous generalizations. His major objection, however, is that such misstatements reflect not solely upon their author, but upon psychology and psychologists in general, since the radio pamphlet was, in a sense, an "official" publication. This misrepresentation of modern psychology he considers to be serious and unethical.

Ellis' objections are based upon a misinterpretation of what I actually said. On page 11 of my introductory chapter I wrote, apropos of several animal experiments which I had previously described, that "animal psychologists have found little evidence that animals in such situations 'reason' or 'think out' the correct solutions." Does this statement misrepresent present opinion among animal psychologists, as Ellis seems to think it does? As I am not a comparative psychologist, I shall cite from several recognized authorities in the animal field. In his radio address delivered on March 12, 1932, Thorndike says,[2] with reference to animal learning "In such learning there need be, and usually is, no reasoning, no inferring, or comparing. There need be, and usually is, no thinking about things, no putting this fact and that fact together. There need be, and usually are, no ideas; the animal need not think of the box or the food, or the acts he performs. . . . . Animals learned by trial and success millions of years ago. Most of the animals in the world today do most of their learning in this way." Hunter,[3] after reviewing Köhler's ape-experiments, states his decision that "we are thus forced to the conclusion that no causal factor of insight has been demonstrated, and that to resort to such a hypothesis is to desert the straightforward experimental attack upon the determining conditions of behavior from which alone an adequate solution can be derived." Carr,[4] in his presidential address before the American Psychological Association, criticizes the view that insight is a method of learning separate and distinct from the so-called trial and error or motor method of learning in the following words: "Used in this way, the term insight will come to imply, I fear, some mystical, intuitive power of mind that unerringly, unfalteringly, and inevitably guides our mental footsteps in the paths in which they should go. To attribute this sort of insight to animals, is to endow them with powers not possessed by human beings. I am perfectly willing to humanize animals upon occasions, but I do most heartily object to deifying them."

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[1] W. D. Ellis, Psychology today: A question of ethics, *op. cit.*, 44, 1931, 365-367.

[2] E. L. Thorndike, Intelligence of animals and men, *Radio Lecture, Psychology Today*, (Ser. No. 20), 1932, 2.

[3] W. S. Hunter, *Foundations of Experimental Psychology*, 1929, 575.

[4] H. A. Carr, The interpretation of the animal mind, *Psychol. Rev.*, 34, 1927, 87-106.

These quotations are from men whose work, I believe, nearly every psychologist would agree is representative of modern trends in the comparative field. I leave it to my fellow psychologists to judge whether my sentence, as given above, is "unethical" in that it misrepresents modern psychology. What Ellis meant to say, I imagine, was that I had "misrepresented" his own personal views of animal learning.

Ellis' remarks directed against my treatment of the topics of the emotions and intelligence are interesting as examples of logic, but hardly worthy of comment. It would be easy to show, by citing authorities, that he is apparently not well informed on these topics. The examples of animal learning given above will suffice. Ellis seems not to recognize fully that I wrote these sections for the general reader without psychological training. That they are sketchy and leave much to be desired, no one appreciates more than I.

In conclusion, Ellis comes, it seems to me, very close to violating the "ethics" of fair play. A psychologist should at least acquaint himself with current knowledge in the field before he undertakes to inform—or reform—other workers.

Columbia University HENRY E. GARRETT

FALLIBLE SCORES AND THE STANDARD ERROR OF A DIFFERENCE

It is desirable occasionally to determine the significance of a difference between two 'true' measures, one or both of which have been determined from fallible scores. The usual formula for the standard error of a difference is inadequate for this purpose. There are four possible types of differences which may arise, namely: (1) when one variable has been measured by 'true' scores and the other by fallible scores, but the variables are uncorrelated; (2) when one variable has been measured by 'true' scores and the other by fallible scores but correlation exists between the variables; (3) when both variables are measured in terms of fallible scores, but the variables are uncorrelated; and (4) when both variables are measured by fallible scores and correlation exists between the variables.

The standard error of a difference between two measures when they are uncorrelated is

$$\sigma_d = (\sigma_1^2 + \sigma_2^2)^{\frac{1}{2}} \ldots\ldots\ldots\ldots [1]$$

In many problems in education correlation exists between the two variables, which as pointed out by Walker may exert a not inconsiderable influence on the result.[1] When correlation exists between the measures formula [1] becomes

$$\sigma_d = (\sigma_1^2 + \sigma_2^2 - 2\sigma_1\sigma_2 r_{12})^{\frac{1}{2}} \ldots\ldots\ldots\ldots [2]$$

Now if the variables are perfectly reliable r_{12} will equal $r_{\infty\omega}$; σ_1 will equal σ_∞; and σ_2 will equal σ_ω. If, however, either or both variables are not wholly reliable, the attenuation in σ_1, σ_2, and r_{12} must be considered. We need now, before we can proceed, the true standard deviation, the correlation coefficient corrected for attenuation, and finally the correlation coefficient corrected for attenuation in one variable.

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[1] H. M. Walker, Concerning the standard error of a difference, *J. Educ. Psychol.*, 20, 1929, 53-60.

The formula for the true standard deviation has been given by Kelley[2] as:

$$\sigma_\infty = \sigma_1 (r_{1I})^{\frac{1}{2}} \ldots\ldots [3]$$

Spearman[3] gives the correlation corrected for attenuation as:

$$r_{\infty\infty} = r_{12}/(r_{1I}\ r_{2II})^{\frac{1}{2}} \ldots\ldots [4]$$

The formula for the correlation corrected for attenuation in one variable as given by Kelley[4] is:

$$r_{2\infty} = r_{12}/(r_{1I})^{\frac{1}{2}} \ldots\ldots [5]$$

We may now by appropriate substitutions in formulas [1] and [2] secure the desired standard error of a difference.

The standard error of a difference between a true and a fallible measure when they are uncorrelated is given by formula [6].

$$\sigma_{td} = (\sigma_1^2 r_{1I} + \sigma_2^2)^{\frac{1}{2}} \ldots\ldots [6]$$

If correlation exists between the two variables then formula [6] becomes:

$$\sigma_{td} = (\sigma_1^2 r_{1I} + \sigma_2^2 - 2\sigma_1\sigma_2 r_{12})^{\frac{1}{2}} \ldots\ldots [7]$$

We now desire the standard error of a difference when both the original measures are fallible. This is given by formula [8] when correlation is absent and by formula [9] when correlation is present.

$$\sigma_{td} = (\sigma_1^2 r_{1I} + \sigma_2^2 r_{2II})^{\frac{1}{2}} \ldots\ldots [8]$$

$$\sigma_{td} = (\sigma_1^2 r_{1I} + \sigma_2^2 r_{2II} - 2\sigma_1\sigma_2 r_{12})^{\frac{1}{2}} \ldots\ldots [9]$$

It should be noted that in all cases the standard error is reduced, but that as the reliability coefficients approach unity, the values given by formulas [6] and [8] approach that of formula [1], and similarly the values given by formulas [7] and [9] approach that given by formula [2].

Fordham University JACK W. DUNLAP

THE NEW YORK MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The twenty-eighth annual meeting of Experimental Psychologists was held at Columbia University on March 24 and 25, under the chairmanship of Professor R. S. Woodworth. Nineteen members from fourteen universities were present and reported upon the work in progress in their laboratories. The 1933 meetings will be held at the University of Pennsylvania under the chairmanship of Professor S. W. Fernberger.

K. M. D.

THE ACCURACY OF PHASE CYCLES IN A COMMERCIAL ELECTRIC CURRENT

In a recent experiment we ran the constant speed motor of a Dunlap Chronoscope by a 60 cycle A.C., current supplied by the Philadelphia Electric Company. The current is now employed so widely for use with telechron clocks and with synchronous motors used for other purposes that great efforts are made to maintain a constant rate of alternation. On the basis of the current which we are using the normal reading of the single unit of the Dunlap Chronoscope measures 1/1200 sec. It has seemed worth while to determine the accuracy with which the electric company maintained the cycle ratio.

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[2] T. L. Kelley, The measurement of overlapping, *ibid.*, 10, 1919, 459.

[3] C. Spearman, The proof and measurement of association between two things, this JOURNAL, 15, 1904, 88.

[4] Kelley, *op. cit.*, 460.

The electric company very kindly supplied me with graphic records of their cycle rate covering the period of an entire week. The records show that the cycle rate was maintained within an error of 1/10 cycle with a high degree of accuracy. This means that if one were measuring reaction-times of the magnitude of 200/1200 sec. the range of accuracy would be between 199.8/1200 and 200.2/1200 sec. During the week the accuracy deviated more than 1/10 cycle only 23 times. The deviation was never as great as 1.5 cycle. The large deviations are of only short duration and apparently are due to sudden changes in load. Hence the largest deviation would give an accuracy of from 199.6/1200 to 200,4/1200 which is below the sensitivity of the Dunlap Chronoscope and certainly below the sensitivity of the accuracy required of practically all chronoscopic work in psychology.

University of Pennsylvania SAMUEL W. FERNBERGER

Harald Höffding: 1843-1931

The Danish philosopher, Harald Höffding, was born in Copenhagen on March 11, 1843. He passed the state examination in theology at the University of Copenhagen in 1865, took his doctor's degree in philosophy in 1870, was habilitated as *Dozent* in philosophy in 1880, and was made Professor of Philosophy in 1883. In 1914 he was elected to the honorable residence of Carlsberg, founded for the most eminent Danish citizen. In 1915 he resigned his professorship. He died on July 2, 1931.

The main trend of his philosophical development was directed away from theology toward a naturalistic philosophy, with a positivistic and critical (Kantian) influence from Sören Kierkegaard, Comte, and Stuart Mill.

His psychology is a "psychology without soul" which has as its main object the mental phenomena, and which regards introspection as it chief method. He regarded the experimental method with friendliness, but thought it applicable only to the most elementary phenomena.

Höffding was a thorough determinist. Concerning the relation between body and soul he maintained empirically the identity hypothesis: the mental and the material world are two manifestations of one and the same being, both given in experience. Metaphysically he was inclined to maintain (with arguments from the theory of knowledge) that the *mental* stands closest to the innermost essence of existence.

The main content of his psychology resembles the older association psychology. Höffding thought that he had avoided atomism by regarding the so-called mental elements as abstractions, and by considering the mental life as having an active and a passive side. "The passive side corresponds to the diversity of elements, the active side to the unity of all conscious content, whereby the individual elements are connected and brought into interaction. Kant, therefore, rightly characterized consciousness as a synthesis, which always presupposes a given manifold and combines individual sensations into percepts, forms representations into concepts, and so forth." A special case of this synthesis appears in his law of relativity: "From the moment of its first coming into being, the existence and properties of a sensation are determined by its relation to other sensations." Among the three mental elements, feeling, cognition, and will, he regarded the will as the most fundamental, and as identical with the activity of the synthesis which in every individual case is directed to a definite, more or less conscious, end. As the will is not a single element, but the form of conscious activity, it is not directly observable. The association of ideas by contiguity, similarity, and contrast are regarded as forms of the fundamental law of totality.

Höffding's main contribution to psychology is the emphasis he gave to *recognition*. His "quality of recognition" (*Wiedererkennungsqualität*) points, by way of the *Gestaltqualität* of Ehrenfels, to more recent developments in psychology.

His *Outlines of Psychology*, which first appeared in Danish in 1882, has gone through many editions, and has been translated into many languages (English, 1891). A part of its success is ascribable to the prominence of more philosophical analyses, which makes it a sort of *Einleitung in die Philosophie*.

The other writings of Höffding contain a large amount of valuable psychological reflections. This is specially the case with his *Ethics*, *Philosophy of Religion*, and *Human Thought* (his main work upon the theory of knowledge). His little book, *The Great Humour*, gives an interesting psychological and philosophical analysis of his own fundamental emotional attitude towards existence.

A bibliography by K. Sandelin of all Höffding's writings of psychological interest will be found in the third volume of the *Psychological Register*. A bibliography by the same author of all writings of Höffding, and most of the writings about him, will be found in *Harald Höffding in Memoriam* (Copenhagen, 1932), which also contains speeches about Höffding by E. Rubin, J. Jörgensen, V. Kuhr, and F. Brandt.

Copenhagen EDGAR RUBIN

Max von Frey: 1852-1932

Max von Frey, the sponsor of the classical theory of cutaneous sensibility, died at Würzburg on January 25, 1932, at the age of 79. He was born at Salzburg, November 16. 1852, a close contemporary of Stumpf, Ebbinghaus and G. E. Müller, but twenty years younger than Wundt. Von Frey went as a student to the universities of Vienna, Leipzig, and Freiburg, and was awarded the medical degree at Leipzig in 1877. In 1882 he was habilitated at Leipzig as *Dozent* in physiology, where the great Ludwig was professor, and he was made *ausserordentlicher* Professor in 1891. In these years his research had to do with the circulatory system, and he published three small monographs on this subject-matter in 1876, 1880, and 1892.

Von Frey's psychological interest in cutaneous sensation began in his later years at Leipzig. In 1894 he published *Die Gefühle und ihr Verhältnis zu den Empfindungen*, a twenty-four page brochure, in which he argued that sensory pain is the unpleasantness of feeling and that pleasure is not a feeling nor a sensation but the surcease of pain.

It was at the same time that Von Frey published his classical papers on cutaneous sensation. There were in 1894-97 four short notes, totalling fifty-two pages, in the *Berichte der königlichen sächsischen Gesellschaft der Wissenschaft zu Leipzig*, and the long paper on pressure and pain in 1896 in the *Abhandlungen* of the same society.[1] It is here that he took over the discoveries of Blix and Goldscheider about the sensory spots on the skin, verified the results, established the lesser frequencies of Blix as against the greater frequencies of Goldscheider, set up pain as coördinate with the other three modalities (pressure, warmth, and cold), invented the hair esthesiometer, showed that the true pressure stimulus is proportional, not to weight nor to pressure per unit area, but to pressure per unit of linear dimension of a small stimulus-surface, created from this finding the surface-tension theory of pressure-stimulation, noted the very exact relationship between the location of hairs and of pressure spots, and finally hunted and argued out a separate nervous end-organ for each of the four modalities (the free epithelial endings for pain, the corpuscles of Meissner and the free endings at the hair bulbs for pressure, the end-bulbs of Krause for cold, and the Ruffini endings for warmth). Not all of these findings were to remain accepted truth, and the correlation of cutaneous qualities with nerve-endings doubtless persisted much longer than it should solely because of the prestige of Von Frey.[2]

It is not clear what turned Von Frey to sense-physiology. The atmosphere at Leipzig was more psychological than at any other university. Wundt's laboratory was the psychological center for the whole world. Yet Von Frey never published in Wundt's *Philosophische Studien;* nor was he one of the psychological physiologists of the camp opposed to Wundt and Leipzig—König, Aubert, Exner, Helmholtz, Hering, and Von Kries—who participated in editing the *Zeitschrift für Psychologie.*[3]

Von Frey went from Leipzig to Zurich as *ordentlicher* Professor in 1898, and then a year later to Würzburg, where he remained the rest of his life. During his first decade at Würzburg he put out half a dozen papers, two of them with Kiesow, on the end-organs in the skin. He had acquired some interest also in the problems of cutaneous space, and with Metzner published his well-known study of the successive two-point limen.[4] Brückner's study of the simultaneous limen also was made under Von Frey's direction.[5] In 1904 he issued his textbook, *Vorlesungen über Physiologie,* which went into a second edition in 1911, and into a third in 1920.

During the last two decades Von Frey has been the author of almost two score of short papers on cutaneous, kinesthetic and organic sensations, their end-organs, and cutaneous space and movement. Almost three-fourths of these publications have been in the *Zeitschrift für Biologie,* and the whole group is almost unknown to psychologists. During all this time Von Frey maintained his original faith in the punctiform distribution of sensory spots on the skin, and sought to show how many perceptual complexes were to be accounted for on this basis. Thus in 1923 he contributed to the *Festgabe* for Von Kries a paper in which—since the *Festgabe* was an issue of the *Psychologische Forschung*—he most appropriately aligned himself with *Gestalt* psychology.[6]

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[1] The classical references may be given in full, since they seem not to be thus given in any standard secondary source: M. von Frey, Beiträge zur Physiologie des Schmerzsinns (i), *Ber. d. kgl. sächs. Gesell. d. Wiss. zu Leipzig, math.-phys. Cl.,* 46, 1894, 185-196; *idem* (ii), *ibid.,* 283-296; Beiträge zur Sinnesphysiologie der Haut (iii), *ibid.,* 47, 1895, 166-184; *idem* (iv), *ibid.,* 49, 1897, 462-468; Untersuchungen über die Sinnesfunktionen der menschlichen Haut: I. Druckempfindung und Schmerz, *Abhandl. d. kgl. sächs. Gesell. d. Wiss. zu Leipzig, math.-phys. Cl.,* 23, 1896, 169-266.

[2] Cf. K. M. Dallenbach, The temperature spots and organs, this JOURNAL, 41, 1929, 344.

[3] Of course Von Frey could hardly have participated in the founding of the *Zeitschrift* in 1890, while he was still known only for research on the vascular system.

[4] M. von Frey and R. Metzner, Die Raumschwelle der Haut bei Successivreizung, *Zsch. f. Psychol.,* 29, 1902, 161-182.

[5] A. Brückner, Die Raumschwelle bei Simultanreizung, *ibid.,* 26, 1901, 33-60.

The *Psychological Index* lists 38 articles by Von Frey alone and 14 more in which he collaborated with another author. Of this total of 52, there were 14 in the period 1894-1910 and 38 in 1911-1930. It is clear that the psychological bibliography of Von Frey, the physiologist, is considerable. All these papers deal with somesthetic sensation and perception, except for one article on smell and another on sleep. In general, however, Von Frey lost contact with the psychological world. Nevertheless the psychologists will honor his memory as the E. H. Weber of the latter nineteenth century. No well educated psychologist can ever draw his pen across the back of his hand and feel the little thrills of cold without thinking of Von Frey, even though Blix discovered the spots, even though there are no Krause end-bulbs there.

E.G.B.

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[6] Von Frey, Über Wandlungen der Empfindung bei formal verschiedener Reizung einer Art von Sinnesnerven, *Psychol. Forsch.*, 3, 1923, 209-218.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Reflex Action: A Study in the History of Physiological Psychology. By FRANKLIN FEARING. Baltimore, Williams & Wilkins Co., 1930. Pp. xiv, 350.

In an introductory chapter, this general problem of the book is set: "Is the body animated by an immaterial agency, called 'soul or spirit,' or does it run like a machine, the functions of which are determined by the parts?" Thus to state the problem in terms of a dualism from which, by the way, the author never extricates himself, seems particularly unfortunate. A modern experimental psychologist would be embarrassed to choose between these alternatives, because both are obviously wrong. A scientist would prefer to remain uncommitted to any preconceived solution. The concept of reflex action, particularly as conceived by Pavlov, is at once introduced, since Pavlov's "position is typical of current physiological theorizing," and some of the objections to this solution are briefly indicated. The current mechanistic-reflex theorizing regarding the nature of the higher integrations of animals and man is interpreted as "a reaction against the older dualistic and animistic conceptions involving mind, will, or any type of conscious process" (p. 3). The content of the volume and the statement that "The story of the development of the concept of reflex action and of the phenomena on which it is based, is the object of the present historical survey" (p. 7), show that the author is seeking to study the reflex mechanism in a very wide sense. But why then are such significant investigators as Cannon, Carlson, Coghill, *et al.* omitted from modern contributors? If the present study is particularly interested in how "the body (is) animated" what is the reason for the neglect of the work on autonomic mechanisms and activities? On the other hand, the old-time classifications of voluntary and involuntary movements are frequent throughout the book, and the author seems to identify the 'involuntary' with reflex movements. There is always a sort of implication that 'voluntary actions' are run by something else than a stimulus—*e.g.* by 'consciousness,' 'mind,' or what-not. The author never refers to literature which attempts experimentally to throw light on 'volition;' nor does he attempt himself to clear up this unexplored something to which one class of actions is constantly referred. It is, of course, to be conceded that complete consistency and inclusiveness cannot be expected in a book like this one; but we intend here only to indicate what seem to be some weaknesses. Clear definition and distinction of terms are scarce indeed; and mysticism is an ever-present menace.

After brief indications of some pre-scientific conceptions of neuro-muscular action, the author divides the historical development of the reflex arc concept into the following stages: (1) the pre-scientific period, before the time of Descartes in the 17th century; (2) the speculative period, ending with the appearance of Robert Whytt's book on the *Involuntary Motions of Animals;* (3) the period of nascent experimentation (1750–1831), ending with Marshall Hall's work on the spinal cord reflexes, 1832; (4) the period of increasing knowledge of the structural components of the reflex arc (1832–1906); and (5) the modern period, dating from the publication of Sherrington's *Integrative Action of the Nervous System*. Developments in each period are treated in considerable detail. Chapters 2 and 3 on the 17th century developments have earlier been published in the *Psychological Review* and in this JOURNAL, respectively. Chapter 13, *The Reflex Maintenance of Posture*, has been published in the *Journal of Mental and Nervous Diseases;* chapter 14 on *Motor Autonomisms and Reflex Actions*, in the *Psychological Bulletin;* chapter 15, *The Tendon Reflexes*, in this JOURNAL. In passing over the first two of these chapters, it may be relevant to ask the author's grounds for saying that "Descartes rejected Harvey's conclusions regarding the circulation of the blood" (p. 29), in view of his own statement that Harvey's discovery was understood by those "whom the authority of the ancients had not entirely blinded." (B. Rand, *The Classical Psychologists*, 1912, p. 171). The localization of function in the nervous system is traced down through the 17th century from Thomas Willis (1621–1675), who elaborated the principles of mechanical action as stated by Descartes and assigned to the cerebellum the rather independent autonomic and reflex functions (including the contributions of Robert Boyle, Perrault, and others who demonstrated the persistence of various vital functions after the destruction of the brain). Boerhaave indicated autonomic activities ('habits') "which taking their first action from the command of the will, do nevertheless afterwards continue to operate in a manner as if it was spontaneously," and "of which by continued use we are insensible" [by no means the first recognition of such acts], thus enlarging the range of recognized 'mechanical' activities. To these the 18th century researches and speculations by Astruc of Montpelier, the first to use the term 'reflex' in 1736, Albrecht von Haller, Robert Whytt, who particularly demonstrated the reflex functions of the spinal cord, Hartley, La Mettrie, Unzer, and others added details as to specificity of functions and carried the mechanistic movement to the point of inviting critical reactions against it.

Other bodily functions and reactions come in for consideration. Samuel Farr (1741–1795) suggested that, although as a "wise provision of nature" many activities did not require conscious guidance, the soul nevertheless was governor of the entire machine. The two Alexander Monros found that various actions of different levels of complexity could not all be accounted for "on the yet known principles of mechanism." Then come the discoveries of Sir Charles Bell and Magendie, as to specific functions of the posterior (sensory) and the anterior (motor) spinal columns. As to claims of priority the author quotes statements which are, in agreement with Boring's view (*A History of Experimental Psychology*, 1930, p. 36), in favor of Bell whose general conclusion, says Boring, was arrived at in 1807. It is to be noted that Fulton, a physiologist, takes the opposite position in his *Source Book of Physiology*, 1931. "Though one hates to pass judgment upon claims to priority," he says, "one cannot doubt that Bell was dishonest and that the chief credit of elucidating the function of the posterior as well as the anterior roots belongs to Magendie." It is unfortunate that this difference in opinion still exists. In considering the contribution of Johannes Müller and of Marshall Hall, on the elements of the reflex arc, Fearing characterizes Müller as dogmatic regarding "the mechanical aspects of the reflex act" and says that Müller "seems to have *recognized the possibility of psychical activity* *as a modifying factor in reflex action*" (p. 145, italics mine). Modern writers are surely not forced to choose between alternatives like these in the reconciliation of historical contributions. A similar openness by the author to mystic factors as one of two alternatives is shown in his treatment of the controversy between Pflüger and Lotze. Nowhere can the reader find a clear definition of either of these factors or how it would operate in different degrees of uniformity of activity. Obviously some sort of clarifying criteria should be offered before the author could refer seriously to, or consider the "effect of concomitant psychical processes on reflex action," . . . "the opinion that psychic forces might be set free in any part of the brain or spinal cord" (p. 165). The reader wonders how far he can rely on what purports to be an impartial consideration of the various sorts of historical and controversial data presented.

In Chapter 12, *The Inhibition of Reflex Action*, the author recognizes the problem concerning 'psychic forces' as essentially one of the "modifiability of of reflex action," seemingly because it has become in the latter part of the 19th and the first of the 20th century so complex that no simple alternative can be presented; but one may well object when Fearing asserts that as against the crystallizing of problems of variability in psychology "around the concept of inhibition," these problems relate themselves in psychology 'to phenomena of attention and the effect of cortical or 'voluntary' processes upon concomitant reflex or involuntary activities" (p. 187). The situation is not exactly thus in psychology; the alternative to 'mechanical' reflex is not still that of a baffling dualism. Fearing generally speaks of "the effect" (singular) when he refers to supposedly 'psychic,' 'voluntary,' and other mystic and ill-defined factors. These factors seem to him somehow to be unitary, and possibly arbitrary. To the reviewer it seems somewhat questionable also to refer to inhibition in general as *negative response* (p. 188), except from a particular standpoint to which an historical writer can hardly be limited. What are negative and what are positive reactions from the standpoint of the data on such theories of inhibition as the 'center' theory of Setschenow, the 'interference,' the 'metabolism,' the 'refractory phase,' the 'drainage,' and the 'chemical' theory traced in Chapter 12? The unfortunate statement regarding the refractory phase effect "that the subsequent *stimuli* find the nervous conductor in a state of lowered excitability (refractory phase) as a result of the preceding impulse and hence fail to 'get through' " (p. 198, italics mine), will not clarify the situation to the uninitiated.

Following chapters 13, 14, and 15 (previously printed in journals), there is a final chapter on modern concepts. It contains a brief sketch of the expansion of the mechanistic physiology through Jacque Loeb, Bethe, von Uexküll, Pavlov, and others; and some indications are mentioned which, to the author, point to its decline. Fearing finds that in modern experimental work, the term 'reflex' is undergoing a diversity of meanings and concepts and he classifies these conceptions of avenues of approach into rather similar classes, both for physiology and psychology, such as the mechanical approach, the specific approach, etc.

Some general criticisms that one might make of the book are that references could have been put in the form of dates, so that the chronological perspective would at once be clear from the text without going to the bibliography at the end of the book; that foreign quotations be uniformly translated, etc. The reviewer's experience with American psychologists and graduate students in review work would indicate the desirability of translating at least all unambiguous statements quoted from literature in foreign languages. There seems to be no advantage in such cases in maintaining the original untranslated statements of the authors. The style of the book is somewhat monotonous, and the author's reactions are rather uncritical. Occasional obscurities and at least one misspelling ('accomodation,' p. 82) occur. J. P.

Selected Readings in the History of Physiology. By JOHN FARQUHAR FULTON. Baltimore, Charles C. Thomas, 1930. Pp. 317.

In this skillfully organized volume Fulton, formerly Fellow of Magdalen College, Oxford, and now Sterling Professor of Physiology in Yale University, has brought together a collection of readings intended to introduce the student of physiology at first hand to some of the great names and great experiments in the past and present of physiology. The volume is illustrated with well selected portraits and drawings. The whole collection is of real interest to any scientist of catholic taste, but as this review is intended mainly for psychologists it will limit itself for the most part to an evaluation of those portions of the work whose contents are important for the student of physiological psychology.

In the first chapter certain of Lawrence J. Henderson's views on the fitness of the environment are reprinted. The conceptions of this writer must always be of interest to the theoretical psychologist. Passing over chapters 2, 3, and 4 on circulation, the capillaries, and respiration, we must note in the fifth chapter on digestion the excerpts from Pavlov's *The Work of the Digestive Glands* and from Cannon's 1898 paper, *The Movements of the Stomach Studied by Means of the Röntgen Rays,* which are, at least by inference, important.

The two chapters of the work previous to the final chapter are in the main significant to the psychologists, as they deal respectively with "Muscle and Peripheral Nerves" and "The Central Nervous System."

In the first selection from Galen's *De motu musculorum,* Fulton has chosen a passage which shows that this writer had already come to the conclusion that muscles can perform work only when they contract. The next selection is from William Croone (1633-1684), founder of the Croonian lectureship. In this selection Croone who, as the editor says, "despaired of ever knowing how the soul acted upon the body" held, admittedly as a speculation, however, that a liquid passing from the nerves into the muscles leads to their contraction. Certain of Willis's views on muscular contraction and Francis Glisson's proof that the volume occupied by a muscle does not increase during contraction are presented. The views of the Scottish physiologist, Alexander Stuart, upon muscular motion, which are included, show that even in 1739 the view of reflex action, earlier proposed theoretically, had not yet been accepted in physiology. Stuart believed that when he forced a probe into the *medulla spinalis* the resulting motion of the limbs was due to the fact that fluid in the cord was forced out through the nerves to the muscles, thus causing contraction. He further asserts that "voluntary muscular motion in a living animal is begun in the same manner, by an impulse of the mind or will on the animal spirits through the nerves, into the muscles." A selection is next given showing Galvani's fundamental experiment in electrophysiology. The remaining selections in this chapter are from the works which may be considered contemporaneous. These excerpts include Bowditch on the all-or-none principle of contraction of cardiac muscle, Fletcher and Hopkins on lactic acid in muscular activity, Lapicque on chronaxie, Forbes and Gregg on action currents of nerves which are brought into activity in reflex response, Pratt on the all-or-none law in muscle, Hill and Hartree on heat production in muscle, Kato on decrementless conduction based upon studies in the narcotized nerve, Erlanger and Gasser on the analysis of action currents in peripheral nerve, and Adrian and Zotterman on adaptation and the results of stimulation of a single receptor by a natural stimulus.

In the chapter on the central nervous system, the first section, after the able introduction by the editor, is a translation from Descartes on his views of reflex action and reciprocal innervation. The Edinburgh neurologist, Robert Whytt, is represented by an excellently chosen excerpt which shows his conception of reflex action. The next selection is from Prochaska, who, as the editor says, "occupies an important position in the history of psychology for his introduction of the conception of a 'sensorium communi'—that region of the central nervous system which 'reflects' to the motor nerves the sensory impressions received by the brain." Alexander Walker, from whose excellent translation the selection by Prochaska is taken, occupies a strange position in the history of the development of the knowledge of reflex action. This is deftly indicated by the editor in a few introductory sentences and in a short quotation which shows Walker's strangely true and yet strangely false view in regard to the anatomical basis of reflex action.

In introducing an unusually long selection from Sir Charles Bell's *Idea of a New Anatomy of the Brain*, Fulton states the opinion upon the Bell-Magendie controversy that, "Though one hesitates to pass judgment upon claims to priority, one cannot doubt that Bell was dishonest and that the chief credit of elucidating the function of the posterior as well as the anterior roots belongs to Magendie." From Magendie himself is a well selected and important quotation which also bears upon the question of the function of the roots of the spinal nerves. Herbert Mayo, whose work, in the opinion of the editor, marks a true advance in the history of reflex action, is also represented by two significant paragraphs. Flourens is quoted on the physiology of the cerebellum, and Marshall Hall's most important contribution on reflex motions that are "independent of sensation and volition" is next chosen. After this follow selections from Ernst Heinrich and Eduard Friedrich Weber on vagus inhibition of the heart, Brown-Sequard on the functions of the spinal cord, Ferrier on the localization of cerebral function, Sherrington on correlation of reflexes and the principle of the common path, the proprioceptive system, and myotatic reflexes, from Harvey Cushing on the human motor cortex, and from Rudolph Magnus on animal posture. The last chapter of the book deals with certain miscellaneous contributions to physiology.

The bringing together of a work of this sort is a notable service. Beginning students too often lose sight of the deep roots of present knowledge. It seems hardly open to question that there is no way in which the student of any science can adequately comprehend the full significance of present conceptions if he is ignorant of the process by means of which these conceptions evolved to their present form.

Brown University LEONARD CARMICHAEL

Psychophysiology, II. Sensation. By LEONARD T. TROLAND. New York, D. Van Nostrand Co., 1930. Pp. xvii, 397.

This is the second volume of the author's projected tetralogy on the facts and theories of *Psychophysiology*, the 'science which endeavors to ascertain the laws that link consciousness or immediate experience with the variables and phases of (neural) response.' The first volume, it will be recalled, dealt with the general problems of psychology, and included a treatment of perception (the relationship between consciousness and the objective world). The second volume treats of the problems of sensation (the indirect relationship between experience and the afferent arc, beginning with the sense-organ stimulus and ending at the sensory projection areas of the cortex). The author has provided skillfully for the overlapping of these two fields by adding to the later volume an appendix of 'notes' in which are reproduced necessary sections of the first volume—a procedure which enhances materially the value of the book.

Although "Psychophysiology" has been selected as a group title for the entire series, intimate acquaintance with the contents of the present volume will convince the reader that the title is in this case a misnomer, and that 'psycho-biophysics' would have been a much more accurate description of the author's point of view. There is little of physiology in his treatment of the various departments of sensation, and, indeed, one feels that the psychological aspects are secondary; in the chapters on vision and audition especially, the approach is always from the side of biophysics, and the author's interest lies plainly in a correlation of physical stimulus and experience. This method of approach may be irritating to some readers, and they may find still further cause for dismay in the author's apparently ready and uncritical acceptance of results reported by Hecht, Fletcher, *et al.*

One may say without hesitation that Troland's volume, regarded as a whole, is an accurate digest of most of the more important experimental facts and theories in the realm of sensation, taken from the point of view just described. Troland makes an attempt to arrange the facts of the several departments of sensation under the categorical headings, Component Sensation, Composite Sensation and Temporally Conditioned Sensation. In this way he obtains a parallel structure which is followed with profit through the various sensory departments; it is never rigid, and, when necessary, it is sacrificed for the continuity of the treatment. Troland's style is labored only at rare intervals, but he proceeds from point to point with a speed and a presupposition of background-knowledge which may well leave the untrained reader breathless. The book is patently intended for his colleagues and for their more advanced students, and by them it will be received as an important addition to their libraries.

The opening chapter (XIII) deals with 'The Concept of Sensation.' Its first section serves to define the term after a brief historical survey; this is followed by a section on the subdivisions of the afferent arc. A succeeding section follows Nernst, Lillie, Lapicque, Lucas, Adrian, and others in formulating a theory of permeability and polarization of nerve in terms of which the author can explain the more important phenomena of nerve action.

Chapter XIV undertakes a survey of the experimental and theoretical investigations of visual sensitivity. One is shocked to read a treatment of the 'psychophysiology' of vision which contains a dozen references to Hecht, but none to G. E. Müller and Ferree, and only very casual references to Hering, Helmholtz, and Von Kries. Does this mean that the work of these investigators is to be included among the scientific 'duds' which the author warns us he will omit? Following the general plan of arrangement described earlier, Troland treats the stimulus, the refractive, and the neurological relationships of brilliance and of chroma (this latter term being used as a synonym for chromatic experience). The chapter concludes with a section on binocular sensory relationships. It is eclectic in point of view, and the author presents the more important visual theories, although it is apparent that he inclines toward a modification of Hecht's general theory, together with a color-theory which adapts the Helmholtzian interpretation to modern knowledge of nerve action. Troland's discussion of the classical visual theories is brief to the point of curtness, and it is perhaps this brevity which makes his style labored at times in comparison with the lucidity of the preceding chapter.

The chapter on auditory sensation also is written from the biophysical point of view, and it follows his general plan of classification. It will be a singularly disturbing piece of reading to those who find Fletcher's methods inadequate, for Fletcher it is who serves as the guiding genius of this chapter. On the theoretical side, Troland leans towards a pressure-pattern theory in which pitch is represented by impulse-frequency along the nerve path and intensity by total number of pulses per second whether due to number of paths or frequency of impulse.

Succeeding chapters deal with facts and theories of taste, smell, cutaneous, kinesthetic and organic sensitivity, and are more nearly 'psychophysiological' in flavor. The author shows no acquaintance with the negative results of the more recent investigations of Von Frey's correlation of cutaneous experience with receptor and he presents this correlation without adverse comment. For some reason, he makes no reference at all to Boring's classic investigations of internal sensitivity in his chapter on organic sensation, and neither of his volumes affords an adequate presentation of the better-known facts of composite tactual sensation.

A more detailed review might list additional sins of omission and commission and might include a criticism of Troland's translation of various German terms; but such a review could not detract from the importance of the contribution Troland has made in this volume. As a concise, accurate and well-documented textbook of the biophysical background of experimental psychology, it leads one to anticipate with high hope the appearance of the author's succeeding volume on cerebration and action.

Cornell University JOHN G. JENKINS

Human History. By G. ELLIOT SMITH. New York, W. W. Norton & Co., 1929. Pp. xviii, 472.

The book relates man to his primate ancestors, describes the fossil remains of early specimens of *Homo*, recounts the primitive industries, distinguishes the great racial migrations, describes the dawn of human civilization in Egypt, and traces the main stages of development in Asia, Crete, and Greece, telling the story of how "mankind acquired culture and with it social unrest, dangerous practices, and methods of cruelty."

The author approaches human history as an anatomist. He writes "in a scientific laboratory where the principal device for the study of mankind is the process of dissection." But since the "true study of mankind" must look to "the thoughts and feelings that provide the motives for men's behavior," the history is more than an account of the body and of its organic functions. It is, in part if not in essence, psychological.

Smith's theory of human derivation, with its emphasis laid upon cortical expansion and efficiency of the receptors, has been elaborated in earlier works. Except for the author's insistence upon certain early Tarsioids in the ancestry, the theory is commonly held by anthropologists. Little is added to earlier writings in the present general exposition, where the author's main concern is to develop the doctrine that man endured, with but little modification, for hundreds-of-thousands (possibly millions) of years "decent, generous, and peaceful" and wanting any of the "customs, beliefs, arts and crafts, social and political organization of civilization." For this view and for the materials supporting it, Dr. Smith makes a heavy acknowledgment to W. J. Perry, the author of *The Children of the Sun* and other studies of early civilizations. It cannot be said that the anatomical method has here added much to our lean information upon the actual behavior and accomplishments of early man. Smith depends (as other anthropologists have) upon tools and other artifacts and upon the want of evidence of social living. He finds primitive man but slightly inventive and explains the wide spread of common implements and other cultural products as due to diffusion throughout the earth. He has no working scheme of the psychological functions of primitive man to lead him toward a plausible depiction of this primate's daily life. Instead he makes use of the assumption that man's final approach to a civilized state arose from his "endeavors to understand the constitution of his own body and to preserve the life that animated it." This studious lust in man, fed by an instinct for self-preservation, suggests a sophistication among the Egyptians of about 4000 B.C., for which the dull and peaceful ploddings of preceding millenia do not seem to prepare either primitive man or the reader of *Human History.*

The *History* contains a great deal of substantial and orderly knowledge about early man. It may well be read by the layman along with E. A. Hooton's *Up from the Ape* (1931), an entertaining and dependable account, written in terms of physical anthropology, of primate history and of the place therein of the various species of Man.

The new arguments adduced for the Golden Age of pre-civilization must be left to the ethnologist and the psychologist. It is a striking fact that the family (the only really primate and primitive form of organization allowed by Elliot Smith) should be made the locus of primaeval 'decency, generosity and peace' just at the time that it is being made, as a part of the battle of the sexes, the main seat and occasion of disorder, maladjustment, and psychotic disease.

Cornell University MADISON BENTLEY

L'hallucination: Études cliniques. By PIERRE QUERCY. Paris, Félix Alcan, 1930. Pp. 559.

This large volume, which is crammed with references to old and recent works dealing in any way with the subject, *e.g.* Descartes, Spinoza, Baudelaire, Müller, Sidis, Jaensch, Urbantschitsch, Rouhier, Rothschild, and Wheeler and Cutsforth, could have been condensed into about half the present compass with profit to the author's thesis. The thesis is that genuine hallucinations exist, meaning by hallucination a full sensory experience in the absence of any external object to arouse it. His proof (aside from affirmations of hallucinated persons—from common dreamers to St. Theresa) consists in trying to show, in separate chapters, that illusions of proof-reading, synesthesis, sensitivity in an amputated limb, after-images, eidetic images, virtual images, dream experiences, alcoholic hallucinations, peyotl experiences, and the classical auditory hallucinations are all links in a chain, which, in spite of analysis, in spite of large concessions made by those who claim to be hallucinated, has in every link an irreducible sensory content even without any object to arouse a sensation. This sensory content "without object," being an integral part of all perception, makes hallucinations out of all perceptions. Quercy thus turns the tables on those who seek to outlaw hallucinations as mere images, beliefs, projections, or actions "as if hallucinated."

How is an hallucination possible? His answer is "in the absence of the object, the movement which has made an image out of the perception can reproduce itself in the inverse direction, and resuscitate the perception, identical with what it has been under the shock of the object, with what it would be if the object were present" (p. 12). Another correlative explanation is based on the theory of primary and secondary qualities, or elements. The *primary* sensory elements demand the presence and the action of the object, while the secondary sensory elements, "constitute the essence of *bona fide* perception," and "are already hallucinatory, if they detach themselves from the primary sensory core." They constitute "the classical hallucination." "Detached by sickness or by a dream from their normal, primitive, sensory element, any secondary group might associate itself with any primitive group, sometimes with unconscious groups; a lesion of the pharynx, of the ear, of the eye, of the viscera, any of these could give the unknown primitive sensory core of an hallucination. As it associates itself with another primary core, with a convenient and adequate primary group, and without having changed, it would be a legitimate perception" (pp. 35-36). "Our perceptions of red, of the third dimension, of ambiguous perspectives, and of the insults of a non-existent persecutor all possess, in spite of the growing charge of interpretation, the same rights to be called perceptions, sensations, *esthésias*, and the same rights to appear as such to us when we undergo them" (p. 494).

As seen above, although he bases his argument upon them, the author misinterprets and misapplies the processes involved in the perception of color, of ambiguous figures (*e.g.* cubes), and virtual images. Contrary to his contention, there *is* something objective, and something different for different colors, when one sees red or green; there *is* a perfectly clear physical explanation, as well as a real physical cause, when one sees a virtual image in a mirror; and far from the ambiguous cube's giving "more than it has, being this incomplete and poor object" (p. 510), it is richer by just two lines than the same sort of skeleton drawing of a *bona fide* cube.

As one might expect, the line the author has taken leads close to, or into, obscurantism. "The visionary claims to see and to hear sights and sounds as legitimately as we see tonight the multi-colored and changing after-image of a flame. . . . To what degree of absence, and why to this degree of least presence ought one to say that the object is too much absent to be perceived? When is the perception too indirect to be possible? When is the perception that has no immediately present object so unusual that one can contest it and cry to the subject: 'You have only the behavior of perception'? When on the contrary do the perceptions without present objects, if their victims persuade us, become sufficiently veridical—(esthésique)—and consequently formidable, in dreams and hallucinations, to counterbalance the ordinary perception and to render the universe doubtful?" (Pp. 26-27.)

This book is mainly a philosophical treatment of the subject. Its value for psychologists lies in its mass of references to and quotations from others' observations, in its reports of experiments on eidetic imagery (negative results with 200 children) and in its details of self-observations during alcoholic hallucinations, peyotl intoxication, and self-study of after-images. A good many clinical histories of hallucinations are also given. There is no index.

Louisiana State University PAUL CAMPBELL YOUNG

Nouveau traité de psychologie, Vol. 1. By GEORGE DUMAS. With the collaboration of C. Champy, A. Lalande, L. Lapicque, R. Perrier, P. Rivet, A. Tourney, H. Wallon. Paris, Félix Alcan, 1930. Pp. 425.

The nine-volume revision and enlargement of the two-volume *Traité de Psychologie*, edited by Dumas and published in 1923 and 1924, promises to be a valuable source book. The volumes are to cover most of the major fields of contemporary psychology. The present (first) volume comprises several chapters of introductory material from allied sciences and two chapters dealing with the general problems and methods of psychology. The following six volumes are designed to present the main facts of normal psychology, while the final two will be devoted to the approach to the science through the medium of the pathological. Since a good deal of traditional French psychology has been biased in the direction of the philosophical disciplines, it is interesting to note that the present work is ruggedly empirical in its approach. The general point of view as stated in the introductory chapter, written by Dumas himself, is catholic throughout, with the emphasis rather upon the statement of ascertained fact than upon the discussion of theory. It is evident too that the editor is opposed to the restriction of the subject matter of psychology to those facts which fall within the limits of what has been conventionally called experimental psychology. Psychology for him is "a science in which introspection plays an essential preliminary rôle in the description of facts and the analysis of mental mechanisms, in which biology and sociology study the biological roots and the social evolution of the mental functions, and in which pathological psychology, psychopathology and neuropathology make their invaluable contribution through the analysis of sensory-motor and mental disturbances and the cerebro-organic conditions which underly them." In keeping with this point of view, fully one-fourth of the work is to deal with the biological bases of psychological phenomena. In general one gathers that the order of the topics will follow approximately that of the older *Traité*.

Of the eight chapters in Volume I six are non-psychological. The titles of these are: The Place of Man in the Animal Series, The Data of Anthropology, The Physiology of Age and Sex, The General Physiology of the Nervous System, The Special Physiology of the Nervous System, and The Biological Problem of Consciousness. Of these, the chapters on anthropology and the physiology of age and sex are new, while the others, although present in the older work, have been either revised or entirely rewritten. The various articles are without exception clearly written and well illustrated. Of special interest to an increasing number of American psychologists is an excellent summary by Lapicque of most of the important work on chronaxy.

Cornell University R. B. MacLeod

L'hallucination: Philosophes et mystiques. By Pierre Quercy. Paris, Félix Alcan, 1930. Pp. xxvii, 381.

In his introduction Quercy contends that a number of speculative philosopher from Plato to Bergson have been considerably more concerned with hallucinations than with perceptions or images. The treatise proper is divided into two parts, the first a review of the writings of Spinoza, Leibniz, Taine, and Bergson, pertinent to imagery, hallucination, and perception; and the second, a psychological interpretation of the hallucinatory experiences of Sainte Thérèse. The hallucination according to philosophers and mystics is thereby contrasted with an appended note as to the view of a theologian (Gerson).

Part two, an attempt to clarify our concept of the hallucination by reference to a life full of them, started with the citation of a dispute as to whether Sainte Thérèse owed her fame to her neuropathological state, or whether her occasional lapses had nothing to do with her visions and writings. The general conclusion to a discussion of her symptoms of hysteria, sexual frigidity, and other alleged psychopathological conditions seemed to be that she was but slightly if any different from what might be expected of a woman with her interests and ability.

While the first part of *L'hallucination* summarized well the contributions of certain philsosphers to our understanding of the word, part two impressed the reviewer as being fruitless. It does not appear reasonable that any psychological phenomenon can be made clearer by reference to its manifestations in a life that ended almost 400 years ago. Likewise, the practice of establishing the reality of an individual's experience by reference to his own writings is hardly to be commended. Quercy's book is interesting chiefly as an historical approach to what has been at best a cloudlike concept.

DePauw University Stephen M. Corey

Brain and Mind. By R. J. A. Berry. New York, The Macmillan Co., 1928. Pp. 608.

The major portion of the book is concerned with (1) the phylogenetic development of the human nervous system considered first as a whole and then later with reference to its well defined divisions; (2) the gross and minute anatomy of the major portions of the human brain; and (3) the available knowledge as to the functions of these portions. A systematic presentation of the materials is accompanied by 131 figures. The treatment of the autonomic nervous system both in the text and in the illustrations is exceptionally clear. The exposition of the phylogenetic and anatomical material is simple, direct, and admirably suited for beginning students in neurology.

The minor portion of the book is avowedly psychological in nature, presenting two concepts in particular; (1) the familiar thesis that the neuron is the unit of mind, and (2) a technique for mental examinations in which physical and mental measurements are combined. The psychologist will find little new in the arguments supporting the first concept. These arguments would support no theory that any mental disorder exists for which there is not a cerebral insufficiency of some sort.

The clinical psychologist would be skeptical of the relationship assumed to exist between the anthropometric scores and mental ability. A review of the clinical cases given in a concluding chapter would appear to show that when the physical measurements support the mental tests (Binet and Porteus) they are pertinent; when they do not, they are meaningless.

Excellent percentile tables for ages 7-19 yr. and for adults are given for the following measurements: brain capacity, standing stature, sitting stature, bodily weight, grip, and vital capacity. Percentiles are given separately for the two sexes.

The references cited are lacking significantly in those of a psychological nature.

University of Utah DOROTHY NYSWANDER

Essentials of Psychology. By W. B. PILLSBURY. Third Edition. New York, The Macmillan Co., 1930. Pp. ix, 466.

In this second revision the greater part of the book has been rewritten, but the general outlines of content are preserved as far as compatible with recent developments in psychology. Psychology is now defined as "the science of behavior and of the knowing functions of man" (p. 2). This definition frankly avoids the controversies concerning mind and consciousness and recognizes that psychology now covers a broader field than mere conscious behavior. Pillsbury finds introspective as well as objective methods possible because one does not actually observe one's own act while it is going on, "but examines it a moment later in memory" (p. 3). The use of the test method in practical applications of psychology is now recognized. Modern work on cerebral functions, on the refractory phase, the all-or-none law, as well as that by the *Gestaltists*, etc., is briefly indicated, and some of the recent experimental results on the senses, the emotions, etc., are given. Helmholtz's resonance theory of hearing, though favored, is erroneously stated in regard to several details, and much of the best recent objective work by several authors is omitted. The old dimensions of the basilar membrane remain as well as the earlier view of Helmholtz, that the fibers vibrate individually, and objections to the theory on these erroneous data and views are stated as of old. The treatment of instincts has been revised and brought forward to constitute chapter 4, under the caption, The Native and Acquired Factors in Behavior. Here there is some admission of innately determined structures, but the specific developments in activity trends are due to environmental factors, except for the effects of 'selection' in the terminal movements, *i.e.* types of satisfaction or dissatisfaction which accept or reject. On this basis we still have collection, workmanship, fears, laughter, racial, parental, and social instincts, and sympathy ('suffering with others'). Instinctive acts and acquired acts are only relative divisions; 'instinct' is limited to 'drive' and to selection; but learning, as referred to in this chapter and discussed in the next two chapters, is still the result of repetition of acts 'stamped in' by pleasure or native selectiveness. The book continues to emphasize results and theories rather than scientific methods of investigation, and references are only to books. A final brief chapter sketches the general outlines of the genesis of psychological 'schools.'

The author's clear, direct style is a real joy to readers, but the method of the book is rather to impart information than to arouse problems and stimulate investigations. The new edition has several merits as an elementary textbook.

J. P.

Handwörterbuch der psychischen Hygiene und der psychiatrischen Fürsorge. By O. Bumke, G. Kolb, H. Roemer, E. Kahn. Berlin, Walter de Gruyter & Co., 1931. Pp. vi, 400.

This work ought to prove to be a very serviceable handbook on mental hygiene, especially from the psychiatric point of view. It is alphabetically arranged and contains both a table of contents which conveniently lists the author of each article and a detailed index of authors cited and items. At the end of each article comprehensive bibliographies are appended which cover such subjects as alcohol and its effect, the hygiene of industry, marriage and eugenics, the care and therapeutics of various types of mental and nervous diseases, psychoanalytic treatments, the biology and psychology of criminals, the criminally insane, and a very able summary of the whole subject of psychotherapy. The questions of suicide and of a wholesome philosophy of life are conveniently and comprehensively treated. The bearing of spiritualism and mystic approaches on mental health are also given. There is helpful material on statistics, various laws concerning insanity and the traffic in opium together with a rational exposition of methods of training in nutrition, and clothing and its relation to a proper bodily development. These subjects are samples of many others which are likewise treated with a minimum of emphasis on theory and hypothesis and a maximum of experimental orientation. Numerous tables are available but outside of a few diagrams and graphic figures there are no illustrations. The reviewer hopes that this clearly written and conveniently arranged handbook will soon be translated for the use of English-reading psychologists and psychiatrists. It certainly should be included in the libraries of our psychopathic institutions and be individually available to social workers and practitioners in the field.

University of Iowa — Christian A. Ruckmick

Culture and Progress. By Wilson D. Wallis. New York, McGraw-Hill Book Co., Inc., 1930. Pp. xii, 503.

This large volume is devoted to a critical survey of culture and progress from a broad historical approach and with a view to throwing light on contemporary society. Some of the leading problems considered are: culture areas and traits, their origin, nature of development and spread, theories of progress, and the criteria of progress. The author attacks with vigor and confidence many generalizations put forward by different 'schools' of older anthropologists, several of whom are still important contributors to anthropology. The conception of 'culture areas,' as is well known, has spread widely of late not only in anthropology but also in sociology and biology. "While much of the aboriginal world can be divided into culture areas, there is little information with regard to some large ethnological areas." Several conclusions which are justified by present information are given on page 10 ff. We can indicate some of them only briefly: A culture is unique; but it does not travel *in toto* into other culture areas. It becomes modified, each culture influencing and becoming changed by other cultures with which it comes into conflict. One culture does not supplant another, but certain culture traits even flourish better in new than in their native soil. The traits comprising any culture are so interrelated that innovations affect the entire culture complex. Individuals participate to different extents in any present culture, and when "allowance is made for the more limited range of tribal culture the individualities of men in primitive life are perhaps as marked as those of civilized men" (p. 12). Illustrations, but no adequate statistical data, are given to support even this last statement; because, apparently, such data in "numerical results" are mistrusted (p. 45) as being often used to justify foregone conclusions. Sapir's principle of 'necessary connection' or 'necessary presupposition' as to priority of culture techniques and the principle used by certain archeologists in developing an historical perspective of the development of art; namely, that the extent of distribution of any culture is positively correlated with its age, are challenged. Often (*e.g.* in modern times) the reverse is what actually occurs. The view that culture is centrifugally diffused is questioned on the basis of history. In general, the historical reconstructions of cultural developments when based on 'principles' of priority of different cultural traits as inferred by ethnologists rather than from close knowledge of actual history is strongly condemned as the construction of mere 'gardens of Eden.'

This definition of progress is finally arrived at: "Progress consists in performing a function with ease or with greater proficiency or efficiency, and in doing something desirable" (p. 428). The measure of the degree of desirability is admitted to be an especially difficult one, and the author's suggested criteria are not free from subjectivity, but, at least, broad circumstances must be considered in evaluating desirability. When Wallis argues for the uniqueness of the culture of any civilization or of all historical induction as merely the summary of what has taken place—as opposed to considering different changes as due to 'inexorable laws of nature'—he presents an argument that can as well be applied to the development of any *individual;* and he seems to be regarding mind, in the development of cultures, as an arbitrary factor. Certainly, *some* generalizations are helpful, even though they do not seem to apply universally in our modern world. Several such generalizations on progress, as Wallis indicates, have evidently been based on insufficient and old data, and have disregarded concurrences of numerous elements of progress in our period. 'Uniqueness,' however, is a conception that needs careful examination. The uniqueness of a civilization, say of Greek civilization, certainly does not make the entire complex so unrelated to all other events as to make it non-predictable. Uniqueness is not a scientific conception. The question is always one of *degree* of predictability. Uniqueness does not imply total discreteness; it should probably not be used in an absolute sense. The author's collection of data on the details of the phenomena of progress is valuable, but one wonders what degree of reliability and of selectiveness is possessed by his numerous citations and quotations. Certainly a number of the references, well-known to the reviewer, could not stand without question in this regard.

J. P.

Abstracts of Theses, The University of Chicago: Humanistic Series, Vol. 7. Chicago, The University of Chicago Press, 1930. Pp. xiii, 712.

This volume includes the abstracts of theses submitted in the year September 1928, to June 1929, in the Graduate School of Arts and Literature, the Graduate Divinity School, the Graduate School of Commerce and Administration, and the Graduate School of Social Service Administration. The departments in these schools which are probably of most interest to psychologists are represented by abstracts as follows: philosophy, 2; psychology, 10; education, 17; economics, 4; political science, 2; history, 14; sociology, 6; anthropology, 4. In all other departments there are 50. There are thus in this volume alone, disregarding those in the *Science Series, Vol. 7*, for the same period, abstracts of 109 Ph.D. theses,—quite an interesting and illuminating condensation of researches in many lines of work bearing directly on human behavior. Six of the psychology theses have already been published—either in full or in abridged form—in well-known periodicals.

As to the others, one, by C. W. Brown, found on rather small groups of graduate and college students (1) that retention of paired nouns is a function of their (estimated) frequency of occurrence in past experience; (2) that the retention of any given words arranged in meaningful relation with other words is a function of possible words conforming to this relation and varies inversely with the numbers of these possible words; and that (3) conceptual systems markedly strengthen the retention of elements in the different classes of objects, even against the influence of other, older principles of learning. Marion Monroe found, from 1612 drawings produced under controlled conditions by 134 children (ages 2 to 5 years), a correlation of 0.66 between assigned drawing scores and chronological age, and of 0.68 between the drawing scores and Stanford-Binet mental age. When mental age was held constant statistically in the first case and age in the second, the correlations were 0.24 and 0.31, respectively. Color preferences of the children were also obtained by different rather natural means in their play. Yellow was consistently preferred by the 2-yr.-old children and red by the 3- and 4-yr.-olds. Yellow decreased and red increased in preference with age; green and blue likewise increased, the green more consistently. Brightness and saturation differences were, however, also effective factors in influencing preferences. The order in which the names were assigned correctly to the colors used was blue, red, green, yellow. The difference between the first two of these was slight. Loh Seng Tsai found the gradual withdrawal of guidance in maze learning by rats to have no advantage over abrupt withdrawal. Several different methods of attack on the problem were used and blind-alleys were closed for 'guidance.' Georgiana Elnora Wylie found on 162 college students, by a variety of procedures, that the learning of "a series of twenty-four lists of items" was more effective by the whole than by the part method when both methods were equally practiced. No reliable differences in retention were obtained. Practice effects, differences in ability in the sort of memory involved, etc., were controlled.

Data for accurate statistical comparisons are not available in these abstracts but are probably obtainable by anyone desiring them. Several abstracts in other departments deal with aspects of psychology. The entire volume is well worth going over with care by readers interested in psychology. A new volume is to appear every year.

J. P.

Types of Men: The Psychology and Ethics of Personality. By EDUARD SPRANGER. Translated by Paul J. W. Pigors. Halle (Saale), Max Niemeyer, 1928. Pp. xii, 402.

This book lies in the borderland between systematic psychology and ethics. Spranger is best known for his theory of personality types, but the present volume deals with this problem and the application of his categories of personality to problems of ethics and history.

The book is divided into four main sections. The first deals with the philosophical basis. For Spranger psychology must be more than a study of physiological reflexes. The individual can only be understood in terms of meanings and values which rest in a configuration of culture and universal psychic elements. His primary interest is to work out the more general forms or types of personality. Historical backgrounds are for his purpose incidental. As he puts it, "The constant complication of historical relations obstructs our vision of the fundamental pure structures."

The second section deals with these ideal basic personality types or "attitudes." There are six of these: the theoretic, the economic, the aesthetic, the social, the political, and the religious.

The pure theorist is an intellectualistic egoist. He is coldly objective, logical, thoroughly scientific. He tends to escape the world of facts in his interest in the purely conceptual structures which he builds. The economic type, on the other hand, is controlled by utilitarian values, by an intense desire for self-preservation. Work, steadiness and the virtues of material success give the key to his values.

In the aesthetic type art furnishes the fundamental "meaning-direction" in life. Feelings, sensations, and concrete perceptual experience constitute the foundation of his values. "The aesthetic formation of existence is only possible if one loves the human, all too human, if one also loves the 'veil of Maya' and finds in one's passions, joys." Form and beauty constitute his basic values.

The social type is dominated by "the value affirming interest in another being and the taking-the-place-of-another" or sympathy. Love of others rather than love of self is the fundamental motivation. In contrast stands the political type which is characterized by the craving for power, for egocentric control of others. "Power is thus the capacity and (usually) the will, to posit one's own value direction in the other, either as a permanent or a transitory motive." Leadership, however, demands self-control if it is to be most effective.

For Spranger, however, the religious type has the greatest merit. This attitude finds its deepest meaning in the totality of life. "God is the objective principle which is thought of as the object of the highest personal value experience." And "a religious man is he whose mental structure is permanently directed to the creation of the highest and absolutely satisfying value experience." Morality then becomes the "harmony of the individual with the highest world-meaning which he is capable of grasping." This means, of course, that the religious type goes beyond perceptual experience and, moreover, must infuse his conceptual world with divine meaning, for "anyone who can no longer call a God his own gives himself over to the devil."

The third part of the volume treats the consequences of these six-fold types for ethics. There are many one-sided systems of ethics which must give way to a morality in which the collective duties are correlated with personal or individual aims in a hierarchy of values in which the religious attitude stands supreme. The latter rests upon the highest development of personal idealism in which all other attitudes are subordinate to the absolute standard that transcends time and circumstance.

At the outset of the final section the author points out various complexities which arise in actual life. These ideal types never appear in pure form. In treating actuality he is led naturally to discuss the relation of history and culture to the individual. The individual is not merely the product of his cultural *milieu* but possesses also a strain of originality and uniqueness which rests upon the very nature of his "soul" structure. Thus understanding and participating in the historical process does not rest on mere imitation but on a combination of historical factors with the self-determined unique interpretation. "In the result we approach the conception that mind as a great mutual medium includes the individual and the objectivations which grew out of the summation, reciprocal effects and stratification of innumerable individuals."

The last chapter presents briefly a sketch of biographical analysis as one possible method of understanding personality, not merely a statistical enumeration of traits but a sympathetic insight into the inner life and meaning-values of historical personalities. In conclusion Spranger re-states his belief that at all points life is made up of meanings in which the "totality of the mind exists in every mental act" but with varying degrees of subordination of other meaning-directions to the "dominant content" at the moment. And the highest life will always find this "dominant" feature in the "divine" soul-structure expressed in the religious attitude.

It is difficult to evaluate so stimulating a book within the limits of a review. For the American psychologist interested in the practical analysis of personality traits the volume may seem a strangely philosophic and religious rather than a "strictly" psychological presentation. But for those of us who are interested in the underlying conceptual formulations about personality, the book is highly provocative and is bound, in time, to influence not only our ethical interpretations but our psychological investigations as well.

University of Wisconsin KIMBALL YOUNG

Mental Radio. By UPTON SINCLAIR. Introduction by William McDougall. Pasadena, U. Sinclair, 1930. Pp. ix, 239.

This book seems to call for notice among psychological reviews both because it is about things "mental" and because it is introduced and endorsed by Professor William McDougall, who is, according to the author, "probably the 'dean' of

American Psychologists" (p. 205). The introduction vouches for the author as "an able and sincere man with a strong sense of right and wrong" and it suggests that Mrs. Sinclair, the receiving station of the book, is "one of the rare persons who have telepathic power in a marked degree and perhaps other supernormal powers" (vii). Since the 'dean' hints that the Sinclair evidence for telepathy and other unusual powers is "conclusive," it had better be left at that in the present review. May we, however, promise the prospective reader interesting glimpses of the life of the Sinclairs while Upton "was busy trying to reform America," of Jan the Psychic, who "astonished physicians of Southern California" (p. 27), and of many intimate happenings in and around Long Beach, Pasadena and Los Angeles? We may at least assure him that he will discover what Mr. Sinclair and his sponsor dean understand by a scientific experiment. He will soon observe for himself that the simple logic supporting the conviction of the author is that his alleged successes or *hits* could not be due to chance or fraud and they must therefore be due to telepathy.

Among the most entertaining tales in which the psychic powers of the "witch-wife" come to light are those connected with the heroine's childhood in the "black belt" of the "far south." The eldest daughter of a Mississippi judge, planter, and bank president, little Mary Craig Kimbrough *got* the feelings of people by "intuition." She also intuitively knew when her mother wanted her, that Mr. B was waiting at the house, and that poor Jack London was in trouble. She could forecast, too, by dreams; and she was terrified by snakes, fire, and the fact of telepathy. In more mature years, she exchanges "by the laying on of hands" her own headaches for her husband's indigestions, and she learns how, by mere willing, to make the stubborn Jan "do whatever she pleases."

Anyone familiar with the writings of a half-century on Psychic Phenomena (usually initialed in caps!) will recognize the character of the evidence, most of it from the comparison of drawings mind-read from husband to wife. Sometimes the exact physical—not to say anatomical—conditions of the mental radio are nicely observed by wrapping the husband's sketch in green paper and placing it upon the "solar plexus" of the receiving station. Some of the author's interpretations of the telepathed drawings are worthy of comment; *e.g.* the snake that became a crushed kitten, the wild animal that suggested a chorus girl's legs, the caricature of a bald head which caused the wife to see "Upton's face," the hemorrhage which was clairvoyant for hemorrhoid, and the boy's foot which suffered "a strange telepathic transformation" into a lion, dog, or pig snout. Those alienists who discern (as they think) the type of a patient's deviation in his transformation of the Rorschach ink-blots would find food for thought in Sinclair's 'telepathed' figures. Since the author of *Mental Radio* ignores his failures and since he has no adequate method either for recognizing or accounting for his successes, it may be superfluous to remind him of Lord Bacon's shrewd remark that "men mark where they hit not where they miss." The hostile critic of *Mental Radio* will be disarmed by the foreknowledge that "those who throw out these results will not be scientists" (p. 176), and by the discovery that it was only "loyalty to the nature of the universe" (p. 220) that compelled the author to publish the book in spite of the sane advice of relatives and friends in Chapter XXV. M.B.

An Experimental Investigation of Social Behavior Patterns in Young Children. By ESTHER VAN CLEAVE BERNE. University of Iowa Studies: Studies in Child Welfare, Vol. IV, No. 3. Iowa City, The University Press, 1930. Pp. 93.

For all the present interest in social behavior, we must remember that careful factual and experimental analysis of this behavior is just beginning. Hence we should perhaps not be surprised to find a student setting out, as the author did, with the ambitious purpose of "describing and measuring the patterns of social behavior in children of several pre-school groups," as if there were a definite number of particular "patterns" which with a little intelligent industry could easily be found and measured. We find no attempt either to define "patterns of behavior," or to deal with the difficulties involved in isolating definite items of such behavior and working out reliable methods for their investigation.

Three methods of measuring social traits were used in this study: a rating scale, a series of experiments dealing with selected traits, and finally observational studies yielding "observational scores" of the various traits. Thirty traits believed to include all the social behavior patterns of pre-school children were arranged in a rating scale, with 5 degrees of each trait to be distinguished. The traits are indicated by such headings as (trait 6) "Coöperates; works or plays with others; works or plays jointly," and for trait 12: "Kind: is disposed to do good and confer happiness; is benevolent; well-disposed." Instructions to the raters were carefully designed to avoid the "halo effect," by requiring them to rate the entire group on one trait before taking up another; and "good" and "bad" ends of the scale were alternated to counteract a tendency to check at one end of the blank. Tables show the average ratings by 3 persons (sometimes 2) of 104 children. The average reliability of 3 persons in rating 30 children in common is 0.74 ± .06.

Tables also show correlations of the ratings with M.A. when C.A. is held constant, and *vice versa*. Numerous detailed conclusions are drawn (pp. 41 and 44) some of which do not seem to agree with the data. For example: "When the influence of mental age is ruled out, . . . in the Kuhlmann group, chronological age gives a reliable positive correlation with rivalry; a low positive correlation with ascendance; and a rather high positive correlation with socially controlled behavior." Yet the corresponding correlations in the tables are −0.11 ±.12; +0.01 ±.12; and +0.44 ±.10. Clearly the tables are badly confused. The reader has no way of knowing what the results actually are, or whether they really furnish any ground for the conclusions given.

The tables as printed reveal a peculiar situation in that the relationships between ratings on the various traits and mental or chronological age are very different for the Stanford-Binet and for the Kuhlmann groups. For example, with chronological age constant there is in the Stanford group a negative correlation (−0.57 ± .08) of mental age with "socially controlled behavior," while in the Kuhlmann group mental age shows a high positive correlation (+0.70±.06) with the same trait. This sort of contradiction is not even mentioned as such by the author.

Some ingenious experiments were devised to measure some of the leading traits, such as obedience and respect for property rights. In general, the scores on the experiments for the various traits showed high correlations with the ratings. These experiments seem suggestive and well worth the attention of other investigators. Only in general do observational scores "parallel the ratings."

As a piece of pioneer work, this study is interesting and suggestive. Along with other pieces of work it helps in its way to provide a rough preliminary chart of the field of early social behavior. Now perhaps we are ready for the more analytical and more rigidly critical work which is needed.

Smith College MARGARET WOOSTER CURTI

James H. Hyslop—X, His Book: A Cross Reference Record. Collated and annotated by GERTRUDE OGDEN TUBBY, formerly Secretary of the American Society for Psychical Research. Preface by Weston D. Bayley. York, Pa., The York Printing Co., 1929. Pp. 424.

Although few psychologists will be interested in this book, the author and Dr. Bayley (a neurologist of Philadelphia) think it is from the study of such material as it contains that "the only possible science of psychology must grow, as it inevitably will." Here is presented annotated transcripts of stenographic records of 29 séances attended in 1924-25, in New York, London, and Paris, by Miss Tubby for the purpose of providing opportunity for evidential communications of the cross-correspondence type from James H. Hyslop, deceased in 1920. Miss Tubby was trained in the natural sciences and spent 13 years in association with Hyslop. Although her conviction to belief in "communication" occurred as early as 1907, she says it could rest upon the evidence in this book alone. Bayley deplores "the status of contemporaneous psychology," since, "as related to its possible future, [it] bears the same general relation that alchemy once did to modern chemistry." His criterion for evaluating psychology seems to be its adequacy to explain the philosophical problem of the relation between the 'mind' and the 'body'.

Stanford University J. E. COOVER

A Volume-Year Check List of Psychological and Allied Journals. By EDMUND S. CONKLIN. University of Oregon Publications, Vol. 1, No. 2. Eugene, Ore., University Press, 1931. Pp. 22.

This very useful little pamphlet contains, for 92 journals in psychology and some allied fields, the name of each journal, its proper abbreviation in bibliographical work, the names and addresses of its editor and publisher, a numerical list of the volumes published, and the date or dates for every volume. It should be a great aid to bibliographical precision for editors, for psychologists engaged in bibliographical work, and for those many authors with limited library facilities who would like to avoid the wrath of editors by making their citations of the literature exact and complete. How often one finds himself with all of a reference but the date! And, while no secondary source can be so reliable as the original volume itself, the chance of undetected typographical errors must be small in a list of consecutive volume numbers paralleled by a list of consecutive dates.

E. G. B.

The Mighty Medicine, Superstition and its Antidote: A New Liberal Education. By FRANKLIN HENRY GIDDINGS. New York, The Macmillan Co., 1929. Pp. 147.

The author diagnoses the ills of education, locates their source, and prescribes a remedy His vigorous treatment displays a remarkable breadth of accurate knowledge, draws heavily upon the sciences of anthropology, biology, psychology, and sociology, and frequently demonstrates his thesis in dramatic hyperbole. Over thirty years ago Giddings warned "against optimistic confidence that the long struggle for liberty of teaching was then practically ended." He saw "an increasing control of education by unenlightened political masses, carrying with it a submission of questions about the validity and worth of knowledge to a majority vote under universal suffrage" and he expected "that this subserviency of the high to the low will increase as the years go by." More recently scientific medicine is availing itself of chemistry and biology, and demonstrating the possibility of ridding the world of epidemic and other diseases; "but we scorn its teachings to follow after ignoramuses and quacks." "We have churches and schools . . . young men's and young women's Christian associations, colleges and universities; but we remain superstitious, afraid of knowledge, bigoted and intolerant, truculent, gun-toting and murderous." The supreme function of education in human society—"to apply intelligence and knowledge to the improvement of life"—has become more and more inhibited.

Education is the victim of the conflict between two irreconcilable conceptions of it: the sacrosanct and the experimental. It suffers from a persistent occultism that makes "Sunday more sacred than the personal example of the Lord, the flag more sacred than the country that flies it, and a written constitution more sacred than the rights and liberties it defines and guarantees." The virus of its disease is The Mighty Medicine, made in tribal antiquity by the medicine man, and perpetuated in increasingly virulent forms by practitioners of pow wow to the present day. The remedy is The New Liberal Education, with its mystery-dispelling knowledge, its rational goals, and its scientific methods.

The author sets up attainable goals, not neglecting the realm of values, and proposes means for their attainment, appropriate, respectively, to the lower schools and college, and to dull and bright or oblique and sterling pupils. He frequently draws heavily upon psychology, both directly and indirectly, and, in the opinion of the reviewer, with good judgment. Psychologists will find interest in this book.

Stanford University J. E. COOVER

Strategy in Handling People. By E. T. WEBB and JOHN J. B. MORGAN. Chicago, Boulton Pierce & Company, 1930. Pp. 260.

This book presents earmarks of strategy in its presentation as well as in its contents. Under the caption "Thanks are due to these scientists," the authors explain the "unusual plan of presenting this material," and "acknowledge a debt of gratitude to the following members of the American Psychological Association who gave timely encouragement in the early stages of this work by expressing their approval of the method used." Then follow the names (each with title or degree—Professor, Dean, or Dr.) of 75 members of the American Psychological Association. "The men and women listed above," we read in a note below the list, "are among the 159 scientists who approved of the basic plan of this book: to illustrate psychological principles with incidents from the careers of successful men. Their approval was given to the authors in response to a letter asking their estimate of various methods of presentation. There was, in some cases, a difference of opinion as to what additional methods should be used, but all agreed on the essential point." Psychologists of the Association will recall the letter in question. Was their 'approval' an indication that they would approve whatever the authors chose to call "psychological principles," and whatever illustrations were chosen? Nothing in the statements of the book indicates that it was not. Who were the other 84 approving scientists, and how many or what percentage of the members of the American Psychological Association did not respond favorably to a plan which the note of dedication shows had been in preparation "over a period of several years" during which bibliographical data had been gathered and sifted? Those members, who failed to reply or who openly expressed doubts as to the validity of such a compilation, are not referred to at all.

The book presents in a popular style, enlivened with photographs of and quotations from leading executives in American life, the principles of strategy reported to have been utilized by those who have become successful. While such selected quotations and opinions are, of course, not to be confounded with psychology, or with any science, the general principles, 'secrets,' etc. (usually printed in italics) deduced by the authors are probably in the main useful to readers. An example of such principles is this: "When you want people to adopt one of your ideas and act on it with whole-hearted interest, it is often wise to let them think it is their own, and to give them credit for originating it" (p. 47). The reviewer has tried the book on some young men and has found that it has considerable interest-value. There is much repetition of materials on similar principles. It would be instructive for some one to attempt a determination of the reliability and validity of the dicta of the book. J.P.

Character through Creative Experience. By WILLIAM C. BOWER. Chicago, University of Chicago Press, 1930. Pp. 276.

This book represents an effort on the part of the author to use a more or less psychological technique for character education. It is interesting to the psychologist as illustrating how one who evidently is familiar with modern psychology would go about using this knowledge for the desired end: character through creative experience. Religion he considers an intrinsic desire for an end. There is some confusion, however, when he designates it as a "pull from in front," and tries to differentiate this type of impulsion from *vis a tergo.* There is a happy discussion of such topics as: integration and personality, motives in learning, and behavior patterns. Under proper guidance learning becomes to the growing personality a creative experience. The author states that one of the primary functions of religion is integration of personality. Though this, in the opinion of the reviewer, is a statement one would desire to make, there may be some doubt about its being more than felicitous theorizing as yet.

University of Denver T. R. GARTH

The Pattern of Life. By A. ADLER. Ed. by W. B. Wolfe. New York, Cosmopolitan Book Corporation, 1930. Pp. 273.

The 'pattern' is exemplified by a dozen case-studies of children displaying defects in conduct. By means of these Adler exhibits his method of studying the individual history, conversing with parents and child, drawing conclusions, and giving advice. The method shows common sense and an apprehension of the state and needs of badly adjusted children. It involves fewer Latin labels for diseases, hypothetical interpretations, and cultish peculiarities than we have come to expect from those medical people who indulge in the speculative excesses of psychoanalysts and some others. The editor of the volume has prefaced the cases with a plain statement of Adler's theory of conduct, which is ambiguously and pretentiously called *Individual Psychology*. The "great principles" of the theory are set down as the unitary personality, the dynamic whole, the life pattern, and the individual strife for a goal under an apprehension of weakness and limitation. These principles strike the reviewer as more or less common property of all those who sensibly regard the conduct of youthful deviates. As Adler recedes from Freud, he seems to reduce his dependence upon such bold and speculative concepts as made impressive the teachings of his uncompromising master.

In his introductory essay the editor well says that "Individual Psychology is an art more than a science" (p. 43); but no one can read through Adler's sensible comments upon the problems of childhood without observing that, quite apart from its claim to be a 'psychology,' the method may be, in the hands of a wise and informed person, a useful way of helping children toward a more healthful development and parents toward a less stupid conception of their young.

M. B.

The Psychology of the Common Branches. By William Henry Pyle. Baltimore, Warwick & York, 1930. Pp. vii, 381.

The author has limited himself largely to four school subjects,—reading, spelling, handwriting, and arithmetic. Incorporated with the usual data are more recent experiments and opinions relating to measurements in these subjects and the methods and hygiene of instruction. Every chapter is followed by exercises, numbered statements of all the essential principles, brief abstracts of articles and books in the field, and blank pages for entering of references and abstracts of articles still unpublished.

Howard University F. C. Sumner

An Introduction to Human Problems. By Harold Benjamin. Cambridge, Houghton Mifflin Co., 1930. Pp. xiii, 472.

This book's value to psychology lies almost exclusively in the field of methodology: approach, description, and analysis. The first four chapters—I. Introducing the Problem Solver; II. The Search for Order; III. The Description of Order; and IV. Science as Generalization—merit the perusal of all elementary students in psychology if not of all psychologists. Chapter III merits especial attention, not because of newness but because of its clarity and completeness. One sees in these early sections the influences of Kelley, the author's former teacher. Other sections impress one more with the breadth of their erudition and charm of style—often almost pedantic—than with depth of psychological understanding. The simplicity and finality of this treatment leave one with a feeling that the writer is unaware of works like Clark's *The New Evolution*. Intended as an orientation for college freshmen the book begins magnificently but presents such contradictions as the discerning and aggressive freshman will resent.

Michigan Central State Teachers College Milton B. Jensen

The Child's Emotions. Proceedings of the Mid-West Conference on Character Development, February, 1930. Chicago, University of Chicago Press, 1930. Pp. x, 406.

This symposium, comprising the contributions of seven psychologists, four physiologists, four physicians, three sociologists, two authors and lecturers, two school teachers, a public school superintendent, a religionist, a philosopher, and a university president, "dared to be more scientific and less general than ever before" (p. 5). Despite this temerity, the treatment was obviously and frankly popular, including, as antipodes apparently, "both the theories of psychologists, and the experience of those actually engaged in research work with children" (p.5). Very few factual or numerical data are given; most of the contributions being interpretative. Such unsubstantiated statements as, "There were slightly higher scores for the women than for the men," "there was little correlation," "the emotionally maladjusted do better than," etc., are frequent, and they make tiresome reading. The limits of childhood are stretched considerably, some of the contributions dealing with the babe at birth, and others with college seniors. There was little agreement as to the nature of emotion.

The book contains nothing that is new, unless it be a renewed emphasis upon psychoanalytic interpretations, there being no less than twenty references to the work of Freud and his followers.

DePauw University STEPHEN M. COREY

Psychology, Normal and Abnormal. By. J. W. BRIDGES. D. Appleton & Co., 1930. Pp. xxii, 552.

This book of twenty-nine chapters applies the fairly unique principles first proposed by Störring and now well-grounded in psychiatrical practice of making abnormal conditions the outgrowth or exaggeration of normal conditions. It proceeds by defining in rather simple terms the subject of psychology together with some of the main points of view. The reviewer does not agree, however, that "structuralism is thus a static point of view" or that "functional psychology is more concerned with the sequence of mental events than with their analyses." "Mental processes" rather than "mental states" found indeed a more worthy exponent in Titchener's structural psychology than with functional psychology. The date of the foundation of the first laboratory at Leipzig is given as 1870, which must be a misprint. The relation of psychology to other sciences, including the pseudo-sciences, is rather competently given in limpid language. The subject of the abnormal is introduced through a discussion of the norm as statistically derived. After discussing other points of view with regard to mind and body, the author swings over to the double aspect theory which is today finding more and more adherence. Then follows a discussion of the neural mechanisms of behavior including such well-known concepts as the neurograms and the more recent attitude with regard to the integrating alliances in the localization of function.

The author pays his respects to the unconscious in a very conservative fashion and then returns to the usual sectional headings of many of our text-books in psychology. In this case, however, he describes briefly first the normal situation and then under appropriate paragraph headings the abnormal conditions of sensitivity, affective response, reflex connections, and instinct through the acquisition of skilled movements and linguistic habits. This is followed by a treatment of the higher mental processes of memory, imagination, thought, and belief.

With the eighteenth chapter, he turns to more intimately related psychopathic situations with an introductory review of suggestion, hypnosis, play, work, and fatigue. The subject of mental conflict follows that of motivation which leads us into the various interpretations of sleep and dreams where considerable space is given to the Freudian theory.

At the end we are led through chapters on temperament, character, personality and similar larger integrations and disintegrations of the mental life. Finally there is a summary of psychopathology with a succinct description of the main psychoses together with their subdivisions. The book ends with a summary of underlying theories in applied psychology, especially in the realm of psychotherapeutic methods. A comprehensive bibliography topically arranged is appended.

Although a number of books in this general field have lately been issued, it is the reviewer's impression that no other book has been so well systematized as this one. There is always a great temptation to press into service a long list of case studies. To a certain extent such studies furnish interesting and valuable material but the present book reduces this approach to a vanishing minimum and thus furnishes a fairly new experiment in treatises of this type.

University of Iowa CHRISTIAN A. RUCKMICK

Experiments and Exercises in Educational Psychology. By DANIEL STARCH. 3rd ed., by the author and Harry F. Latshaw. New York, The Macmillan Company, 1930. Pp. ix, 254.

Chapters are added on grade placement, personality analysis, study methods, methods of evaluating text-books, and intelligence and achievement testing. Certain chapters are considerably revised and two chapters are omitted. The added exercises are rather elementary and practical in nature. The experiments on certain phases of learning contain some good material, but many problems are omitted entirely.

Vanderbilt University LYLE H. LANIER

The Character Value of the Old Testament. By GEORGE W. BEISWANGER. University of Iowa Studies: Studies in Character, Vol. 3, No. 3. Iowa City, University of Iowa Studies, 1st Ser., no. 187, 1930. Pp. 63.

For generations the literature of the Bible has been regarded as "the chief staple of moral and religious instruction." The author questions the soundness of this evaluation and has sought to determine just what value for character building the Old Testament stories do possess. The techniques and procedures for rating and assigning school grades to 63 Old Testament stories and 65 entire books retelling such stories were slightly modified from those used by the staff of the Institute of Character Research in judging children's literature. The same staff of seven, who had worked together in judging fairy tales, assigned the ratings and grades. The conclusions reached indicate that nine-tenths of these Old Testament stories and three-fourths of the books of retold stories are not worth the child's attention, that, in any case, only three of the Old Testament stories can be recommended for use below the sixth school grade and that there are few good Old Testament story-books below that grade.

In evaluating the ethical content of the stories, restrictions were set up which seem to the reviewer to discount too completely the setting of the story, its traditional accretions and its historical perspective. It is hard to escape the impression that although the ratings and grades are made with meticulous care, they are the rather unconvincing pronouncements of a small staff of workers.

Lehigh University JAMES L. GRAHAM

The Growth of Ability. By RAYMOND O. FILTER and OMAR C. HELD. Baltimore, Warwick & York, 1930. Pp. vii, 174.

Ability is defined as a demonstrated achievement; capacity, as a surmised quality, possibly the foundation of ability. Spearman's theory of the nature of ability is dismissed, not so much on the basis of the defects of the theory as on account of his misapplication of the term ability.

Conditioning comprises the major portion of perceptual learning. An excellent theoretical account of the growth of perception is given. The process of learning is described completely in terms of conditioning. Redintegration is viewed as but a specialization of the principle of conditioning. Movement-produced stimuli (including both exteroceptive and proprioceptive) are given a justly prominent position in the formation of serial responses. Such responses are, however, shown to be far from 'automatic.' The account of the elimination of errors includes no extra principle beyond that of acquiring new behavior. A 'wrong' response is automatically eliminated by the fixing of an antagonistic one. A relatively large section of the book is given to application and other practical considerations.

One of the chief virtues of the book is the fact that it gives a consistent account of the process of learning without recourse to the invention of new theories. The reviewer has seen no more successful effort to reduce the many facts of learning to a simple system. This does not imply, however, that the discussion is reduced to a too elementary level.

Duke University HOWARD EASLEY

The Child's Conception of Physical Causality. By JEAN PIAGET. Tr. by Marjorie Gabain. New York, Harcourt, Brace & Co., 1930. Pp. viii, 309.

The French edition of which this book is a translation has already been reviewed in the JOURNAL (41, 1929, 481-483). The translator has rendered a valuable service to English readers who do not know French. The clearness and simplicity of the original version has been matched in the translation, but, unfortunately, the rather free translation occasionally does not give the exact meaning of the original work, especially where technical terms are employed. For instance, *concrète* is translated as "practical" (p. 1); *l'esprit parcourt un certain nombre de stades* is rendered "the human *spirit* goes through a certain number of stages" (p. 237, italics mine). Here *mind* would certainly have been better for English readers. With this important work by Piaget now more generally available to American readers, it is to be hoped that his experiment will be repeated in different quarters with better technique (including the use of adults as controls) than the author has employed.

J. P.

The Measurement of Man. By J. A. Harris, C. M. Jackson, D. G. Paterson, and R. E. Scammon. Foreword by C. H. Bailey. Minneapolis, The University of Minnesota Press, 1930. Pp. vii, 215.

This book is a compilation of four Sigma Xi lectures delivered by the authors at the University of Minnesota. It is semi-popular in nature and appears to have been designed for the educated layman. Part I, by Harris, deals with The Measurement of Man in the Mass; Parts II, by Jackson, with Normal and Abnormal Human Types; Part III, by Paterson, with Personality and Physique; and Part IV, by Scammon, with The Measurement of the Body in Childhood. Paterson made a thorough though brief review of the materials presented in greater detail in his book *Physique and Intellect.* Scammon presented, from the endocrinological point of view, some significant curves.

Peabody College Paul L. Boynton

The Revolt Against Dualism: An Inquiry Concerning the Existence of Ideas. By Arthur O. Lovejoy. New York, W. M. Norton & Co., Inc., 1930. Pp. xii, 325.

The author defends the hypothesis of representative ideas, which is today probably no less popular among philosophers than among American psychologists. While it is fortunate to have so able a representative of dualism the psychologist can only wish that the treatment of dualism versus some sort of monism could have been more directly and clearly drawn, and that the real issues could have been evaluated by means of more definite criteria. Few experimental psychologists will have the patience to go through this big volume for the various qualitive estimates, pro and con, of questions discussed from the standpoint of adults after years of habituation to certain views and terms and without resort to scientific methods and to genetic approaches. J. P.

Am Ende der Philosophie: Ein kritisches Wort an die Zeit und ihre Philosophen. By Ludwig Freund. Munich, Ernst Reinhardt, 1930. Pp. 178.

This is a critical examination into the limitations of philosophy. The author foresees the termination of any kind of philosophy which does not see its own limitations. On the basis of what he regards as rigid logical procedures he concludes that apodictic modality judgments are out of place in philosophy as well as in the empirical sciences. There is, of course, nothing new in such conclusions.

J. P.

Voices from Beyond. By Henry Hardwicke. Niagara Falls, Harkell Co., 1930. Pp. 125.

Although this book purports "to put into readable form certain well authenticated facts concerning psychical phenomena" established by persons "whose personal integrity it is impossible to question," there is little convincing evidence of 'mediums' and their work. The book cannot be recommended as anything like a scientific treatise. The author has presented his material in the form of a story, consequently the critical reader is unable to separate fact from fiction.

Peabody College Wilson L. Newman

Philosophy of Value: An Essay in Constructive Criticism. By LEO R. WARD. New York, The Macmillan Co., 1930. Pp. x, 263.

This is a philosophical (or probably better theological) discussion from the Scholastic point of view of the illusive problem of 'values.' The author has attempted a critical appraisal of a wide range of modern writers from the standpoint of Thomas Aquinas' *Summa Theologiae.* The book is of little interest to the modern psychologist, except, possibly, to illustrate the influence of a person's cultural setting and social contacts on his (intended) rationalistic position. The work is heavily documented with references and there is a bibliography of 26 pages.

J. P.

Discovering Ourselves: A View of the Human Mind and How It Works. By EDWARD A. STRECKER and KENNETH E. APPEL. New York, The Macmillan Co., 1931. Pp. xiii, 306.

The authors say: "This book is meant to be entirely practical. It is not written for psychologists or psychiatrists." The book presents the usual Freudian concepts in a popular non-critical manner.

Illinois Military School J. D. DODSON

Die Religion in ihrer dogmatischen und ihrer reinen Form: Versuch einer Grundlegung der Religionsphilosophie. By LEO HAMBURGER. Munich, Ernst Reinhardt, 1930. Pp. 170.

This book is not concerned with religious behavior as actually found in man, nor with individual differences in this regard, but is theoretical and somewhat absolutistic. It fails to adhere to present psychological conceptions of determination and will, and it does not employ scientific methods.

J. P.

Solution of the World-Riddle. Book One: Epistemology, the Natural Science of the Universe. By INNOMINATUS. San Francisco, Harr Wagner Publishing Co., 1931. Pp. xxxii, 197.

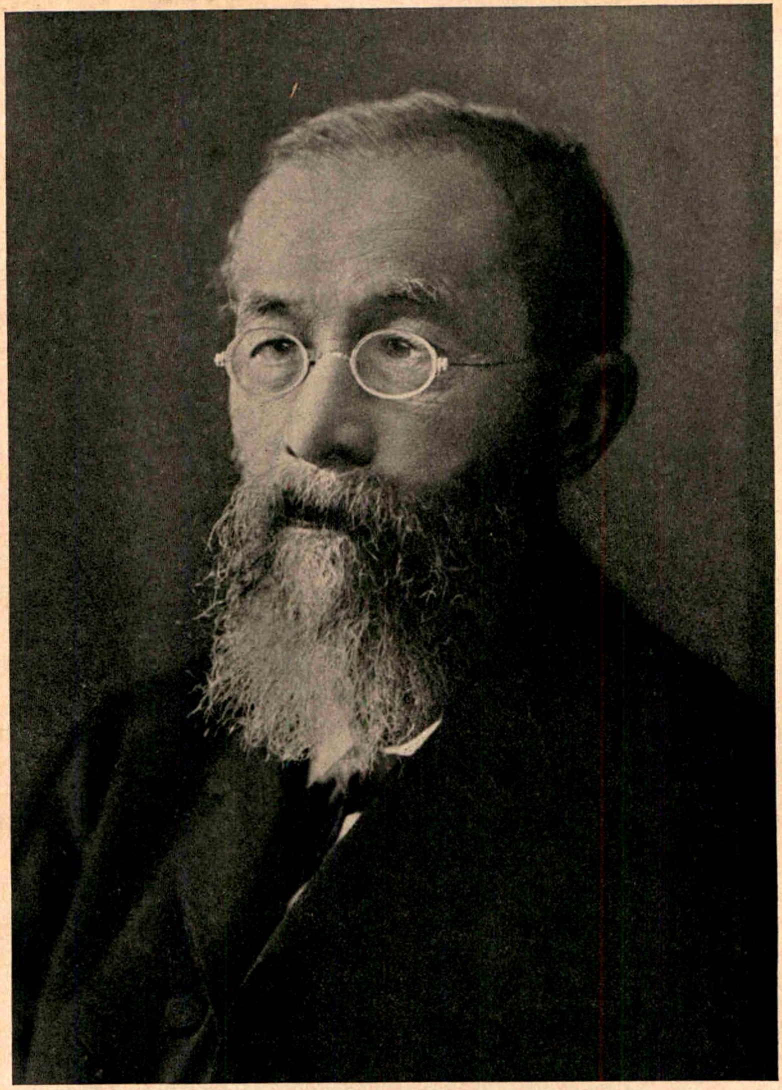
As indicated in the title and anonymous authorship, this book has no scientific value for the psychologist. It can be recommended only as a curious example of disordered elaboration.

Cornell University CHARLES WARREN FOX

Le génie des religions. Second Edition. By BARON DESCAMPS. Paris, Félix Alcan, 1930. Pp. xxii, 539.

Nothing of importance to psychology has been added in this edition. The book is not written from a scientific point of view. There is no excuse for the size of this volume: its contents could be put into relatively few pages if stated in a direct, matter-of-fact style. Its verbosity impresses one as a compensation for lack of real content.

J. P.



W. Wundt.

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WUNDT'S PSYCHOLOGY

By S. Feldman, Cornell University

This year signalizes the hundredth anniversary of Wundt's birth. Yet it is probably still true that, as Titchener wrote soon after Wundt's death in 1920, "we stand too near to Wundt to see him in a just perspective."[1] Clear vision, however, requires more than a proper perspective; with increasing distance, outlines are likely to blur. While, then, the time for a just appraisal of Wundt is not yet, certain preliminary studies can and need to be carried out now.

Wundt's was a long life, and his works were many. In his formative period, moreover, which reached to his forty-second year when he published the first edition of the *Grundzüge der physiologischen Psychologie* (1874), he bore the marks of many influences: Leibnitz, Kant, and Herbart; Helmholtz, Fechner, and Darwin. From 1874 to 1920, on the other hand, while he added greatly to the fields of experimental and social psychology, his psychological outlook persisted, through its many restatements, fairly unchanged. To see Wundt justly, therefore, we may best begin by going back more than half a century and examining the first of the six editions of the *Grundzüge*, the first rough draft of his permanent psychological system. Then we may go back still further to the earlier, discarded drafts, the *Beiträge zur Theorie der Sinneswahrnehmung*, 1858–1862, and the *Vorlesungen über die Menschen- und Thierseele*, 1863. Finally, we shall have to note how Wundt was influenced by contemporaries and predecessors.

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*Accepted for publication September 29, 1931. From the Department of Psychology, Cornell University. This study was begun under the direction of Professor E. B. Titchener.

[1] E. B. Titchener, Wilhelm Wundt, this Journal, 32, 1921, 176.

The writing of the *Grundzüge* in 1874[2] was one of Wundt's two achievements which marked the launching of psychology as an autono-

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[2] The first ten chapters were issued in 1873. On the changes in the plan of the *Grundzüge*, first outlined in a letter written by Wundt to his publisher, cf. E. B. Titchener, On the plan of the *Physiologische Psychologie*, this JOURNAL, 32, 1921, 596 f. The letter, a facsimile of which is inserted after p.90 in Wilhelm Engelmann, *Jubiläumskatalog*, 1911, reads, in translation, as follows:

Heidelberg, December 8, 1872.

My dear Sir,

For some time past I have been busy with putting into shape a book whose preparation has occupied me for several years and which I now design to publish under the title "Grundzüge der physiologischen Psychologie."

The field which in this book is to receive its first complete survey owes its demarcation and present importance almost wholly to investigations of the last two decades. It grew originally out of the physiology of sense-perception; and partly by way of the physiological investigations that bear upon that topic, partly by way of a series of independent psychophysical studies, it has gradually attained dimensions that make a summary survey of its whole extent and a development of the general scientific results to which these paths have led possible and, I believe, desirable. Although the new discipline has grown out of experimental physiology, the questions that interest it cannot in the nature of the case receive in the physiological texts the consideration that their importance demands—just as they lie also beyond the purview of current psychology. It therefore seems to me that physiological psychology is destined to form a middle term between physiology and psychology, in much the same way as at a somewhat earlier period physiological chemistry intervened between physiology and chemistry.

I have laid out the plan of my book as follows. I felt bound, first of all, to include in a physiological psychology the whole range of the interrelations between our outer life and our inner, psychological experience. But I have also felt that I could not avoid the question of the relation obtaining in the last resort between the worlds of inner and of outer occurrence, and of the explanation of the whole interconnection of psychological phenomena suggested by the survey of their borderland. I accordingly treat my subject in five sections. The first, introductory part deals with the physiological characters of the nervous system. The second contains the doctrine of sensation and apprehension; the third, the doctrine of organic movements. These two sections comprise the empirical material of physiological psychology proper; the one, in discussing sensation and apprehension, investigates the inner or psychological—the other, in discussing movement, investigates the outer or physiological consequence of the manifold interactions between our outer and inner experience. A fourth section then presents a criticism of psychological doctrines, directed and supported by the results obtained in these two; and a fifth and final section adds, as outcome of the whole enquiry, a general theory of psychophysical occurrence.

The complete book will fill some forty or fifty forms. Woodcuts are provided, especially in the first two sections, to illustrate the text. The work of preparation is so far advanced that the first steps toward publication can be taken in February of next year and the printing can proceed thereafter without interruption.

I now wish to ask, my dear Sir, whether, and if so upon what terms, you are disposed to undertake the publication of the book. Hoping to receive an early reply to this question, I am,

Yours faithfully,

W. Wundt

mous experimental discipline.[3] Affording us, as it does, the most comprehensive survey of the state of psychology in that momentous period, the book has a permanent value. We therefore begin our serial study of Wundt's psychology with a brief summary, chapter by chapter, of these first "Principles of Physiological Psychology."[4]

Introduction. While human life is the primary concern both of physiology and of psychology, the two sciences have so far, in dealing with their common subject-matter, been following different paths. Physiology has informed us of the phenomena of life as they may be perceived through our external senses; in psychology, man looked at himself, as it were, from within, and dwelled upon the results of this internal observation. Yet there is a wide range of vital processes that calls for coöperative exploring; sensation and centrally initiated movement are outstanding examples. A psychological fact, sensation is nevertheless referable in the main to conditions outside of the organism; and, while the various stages of sensory complication may be followed through psychological observation, such factors as order and grouping of impressions, interrelations of sensory and motor apparatus, and central processes of the nervous system need also to be considered. In the case of action, again, one must rely in large measure upon psychological observation in order to fill the gap between impression and movement.

So long, however, as physiology and psychology stand apart, the investigation of this common territory may be properly assigned to an intermediate discipline.[5] Such a border science would take over the problems which have, till fairly recently, been prosecuted exclusively by psychologists; and it would borrow its equipment from both parent-sciences, combining with psychological observation the methods of experimental physiology. It would single out actual instances of apprehension, feeling, and impulse for analysis; and it would analyze them, not directly, but through experimental variation of the relevant physiological factors. In time, it might provide the *Geisteswissenschaften*—history, law, politics, art, and religion—with a basis in natural science.

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[3] The other was the founding of his laboratory in 1879.

[4] Only a part (the physiological section) of the fifth edition of the book has been translated into English: *Principles of Physiological Psychology*, translated by E. B. Titchener, 1904. Cf. E. G. Boring, Edward Bradford Titchener, this JOURNAL, 38, 1927, 494.

[5] In his preface, Wundt writes: "The work which I here present to the public is an attempt to mark out a new domain of science. I am well aware that the question may be raised, whether the time is yet ripe for such an undertaking. . . . At the same time the best means of discovering the gaps in the subject-matter of a developing science is, as we all know, to take a general survey of its present status." The last sentence echoes a statement made ten years earlier by the Herbartian, M. W. Drobisch. Referring to Wundt's *Vorlesungen*, in a review entitled "Ueber den neuesten Versuch die Psychologie naturwissenschaftlich zu begründen" (*Zeitschrift für exacte Philosophie*, 1864, 313-348), Drobisch remarks: "He [Wundt] has preferred the freer form of lectures because of his conviction that text-books and systems abound in a science in proportion to lack of knowledge, and that psychology will never begin to progress unless an end is made to the writing of text-books and systems. On this score we should like to ask Mr. Wundt . . . . Is not a systematic arrangement best designed for calling attention to gaps within a science?" In his autobiography (*Erlebtes und Erkanntes*, 1920, 206), Wundt recalls Drobisch's scathing criticisms ruefully. Cf. *Vorlesungen*, I, ix.

A suitable name for this novel enterprise would be *physiological psychology*,[6] or *experimental psychology*.[7] Either designation would announce the advent of a natural, explanatory science, and would set it off from the current empirical psychology as well as from the newly launched *Völkerpsychologie*, both of which might well be termed descriptive sciences, or natural histories of mind.[8]

Sensation. Sensation is, unquestionably, the simplest psychological phenomenon; but it is never observed in isolation. To determine its underived properties, one must, in the manner of Condillac, first abstract from it all that it owes to its connections of the moment or to its history: relations in space and in time, feeling-tone, attribution to specific source. The remainder will consist of intensity and quality.[9]

Sensory intensity. Sensory intensity is commonly accepted as a simple indicator of external energies. To quote Herbart, two lights shine twice as brightly as one light, and three strings sound thrice as loud as one string. Yet it is quite obvious that such a relationship between sensory intensity and stimulus energy does not hold at the limits of sensitivity.

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[6] The term *physiological psychology* occurs as a title in F. W. Hagen, *Studien im Gebiete der physiologischen Psychologie*, 1847, and again in Leidesdorf and Meynert's *Vierteljahrsschrift für Psychiatrie in ihren Beziehungen zur Morphologie und Pathologie des Centralnervensystems, zur physiologischen Psychologie, Statistik, und gerichtlichen Medicin*, which began publication in 1867. Wundt himself employs the term in an article, *Neue Leistungen auf dem Gebiete der physiologischen Psychologie*, which he contributed to the first issue of the quarterly.

[7] Wundt seems to have coined the phrase *experimental psychology* in 1862 (*Beiträge*, vi). As we have not seen Bautain's *Psychologie expérimentale*, 1839, we cannot tell whether *expérimentale* in that title means experimental or empirical.

[8] We have not thought it worth while to analyze in detail the six chapters dealing with the "physiological properties of the nervous system," especially since a complete translation of the corresponding portion of the fifth edition is available. The most important item in these chapters is the formulation of four "general laws of the central functions" (231): (1) the principle of connection of elementary parts: every nerve-element is connected with other nerve-elements, and only when so connected does it become capable of physiological function; (2) the principle of indifference of function: no element performs specific functions; the form of its function depends upon its connections and relations; (3) the principle of vicarious function: when the function of any element is inhibited or destroyed, it may be taken over by other elements, provided that they have the necessary connections; (4) the principle of localized function: every distinct function has a distinct place in the central organ from which it issues; *i.e.* a place in which the elements have the connections required for carrying out the function. The last three principles, we are told, are interrelated: vicarious function presupposes indifference, and qualifies localization. Wundt takes sharp issue, on the one hand, with Flourens' "principle of the indivisible function of the cerebral lobes" (226 and 234) and, on the other hand, with the "hypothesis of the specific function of the neural elementary parts" implied both by phrenology and by the Müllerian doctrine of the specific energy of nerves (226 and 232).

[9] Cf. H. Helmholtz, *Handbuch der physiologischen Optik*, 1867, 438: "I conclude that, in perception, whatever can be eliminated or reversed by causes of demonstrably empirical origin is not properly sensation, but the product of experience and habituation. It will be shown that, if we follow this rule, we must refer to sensation nothing but quality, and to experience and habituation most of the spatial perceptions."

Only the lower limits have been investigated in detail, and even there only such approximate values as the following are available. In an illumination 1/300 as bright as moonlight, we can just see a white paper strip at a distance of 5.5 meters; if a cork weighing 1 m grm. drops 1 mm., we can hear it from a distance of 91 mm.; we can feel a weight of 2 m grm.; we can sense a temperature 1/10°C. higher than that of the skin. The stimulus-threshold varies, moreover, with quality, place, and extent of stimulation: it goes up to 50 m grm. on certain parts of the skin; and it goes down for light and for temperature as area of stimulation is increased.

Between the lower and the upper intensive limits,[10] discrimination is measured in terms of the differential threshold, which varies somewhat with method of experimentation (just noticeable differences, average errors, right and wrong cases). All these methods, however, have yielded, at the hands of Weber and Fechner, the same generalization: sensory intensity is proportional to the logarithm of the intensity of stimulus. Thus it is that a shadow cast by moonlight disappears in lamplight, while a shadow cast by lamplight disappears in sunlight; for the same reason, stars are invisible in the daytime. On the other hand, slight differences in brightness between two clouds may be seen by the naked eye and through dark glasses with equal distinctness. The logarithmic relationship is also exemplified in the case of apparent stellar magnitude; and it has been experimentally confirmed in vision and in hearing, for pressure and for warmth (but not for cold), and for lifted weights.

The generalization of the facts of intensive discrimination that bears Weber's name is thus sufficiently assured. Exceptions to it, where any have been demonstrated, rest upon specific physiological causes. For Weber's Law itself, however, an immediate physiological ground is lacking. Observations made upon motor nerves, stimulated electrically at various stages of fatigue, suggest by inference that between stimulus-intensity and intensity of process in sensory nerve a simple proportionality prevails. Weber's logarithmic law may, consequently, be regarded as a psychological law, expressing the relationship of sensation to neural process. It will be shown later that the relationship holds for quality as well as for intensity.

Sensory quality. Each of the four special senses—hearing, sight, smell, and taste—provides a distinct continuum of highly differentiated qualities, although in taste and in smell the form of the continua is not apparent. The skin and all the remaining sensitive areas of the body may be regarded, with Johannes Müller, as one general sense-organ, the organ of the less differentiated sense of feeling.

Inter-modal differences are sufficiently accounted for by differences in the constitution of the sense-organs. Thus, the more primitive sense of feeling functions through undifferentiated and directly stimulated nerve-endings, some of which are rendered more sensitive by accessory bulbular (Krause) and corpuscular (Meissner, Pacini) structures. That temperature and touch rest upon different modes of stimulation of identical fibers is suggested by the confusion of the two at low intensities. The ear reacts first as a whole, but the cochlea with its basilar membrane superimposes an analytic mechanism. In the eye are the rods and the cones, every one of which is the seat of many complex chemical reactions. A lesser variety of chemical reaction-modes will allow for the discrimination—by way of smell and taste—of acids, bases, salts, ether oils, etc.

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[10] Wundt invents the term *Reizhöhe* for the terminal limen.

Such extraordinary functional specialization at the receptor removes any theoretic need for ascribing, with Müller and Helmholtz,[11] specific energies to peripheral or central nerves—an assumption that is, in any case, incompatible with evolutionary tenets, the principle of indifference of neural elements, and other established facts. Qualitative variety may, instead, be referred to heterogeneous molecular processes induced through receptor-stimulation and transmitted in their specificity to the brain. Repeated stimulation of the same sort will naturally predispose the adaptable nerve-substance to a set type of molecular movement. In hearing and in touch the molecular processes repeat the periodicity of the stimulus; in the case of sight, smell and taste, the relationship is not so simple. An account of auditory and visual quality will illustrate these two modes of receptor-function.

In general, auditory quality gives a faithful rendering of stimulus-form. The simple sine-wave is heard as a pure tone; the complex wave yields an analyzable sound; irregularity of wave-form is reflected in the noise. When one considers the continuity of tone and the unanalyzability of noise, the parallelism, it is true, seems to lose its precision; but these exceptions may be respectively attributed to the temporal limits of perception and to the "narrowness of consciousness." The parallel is further modified in the case of pitch-discrimination, where a logarithmic relationship obtains between sensation and stimulus. Here, moreover, the logarithmic function is not extracted from liminal determinations, but is plainly exhibited in the comparison of finite values.

The simple—or saturated—colors lie, like the simple tones, in a linear series. The color-line, however, returns on itself and—with the purples—forms a closed figure, which becomes a triangle when degrees of color-saturation are added. Since we regard color-quality with its neural correlate as a continuous but complex function of stimulus, the color-triangle may be taken as a graphic representation of that function. Color-differences are measured in liminal terms; and the colors coinciding with maxima and minima of differential sensitivity—red, yellow, green, and blue—have a more pronounced quality than the other colors, which are sensed as transitions.

Tonal quality, being independent of intensity, is not influenced by fatigue; in the case of color, however, where quality changes with intensity, fatigue affects both. Color passes over into gray or black as the stimulus is continued; and, unless contrast intervenes, the complementary after-image—itself an indication of altered retinal sensitivity—is negative (less bright than the original impression). The same-colored after-image is always positive (as bright as, or brighter than, the original impression) and implies lag of excitation; while the flight of colors may be explained by differences in the temporal course of retinal excitation.

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[11] Wundt had earlier followed Helmholtz's views concerning specific energy (cf. *Grundzüge*, 346 f. with *Vorlesungen*, I, 182), theory of vision (cf. *Grundzüge*, 332 with *Vorlesungen*, I, 158), and theory of hearing (cf. *Grundzüge*, 348 with *Vorlesungen*, I, 184).

Upon these issues, and others to be noted later, Wundt remained in agreement with Helmholtz down to the third edition of his *Lehrbuch der Physiologie*, which he published the same year (1873) in which he sent the *Grundzüge* to press. Cf. *Lehrbuch*, 494, 611, 671 f.

The facts of color-blindness are compatible with the hypothesis that, in vision, receptor-activity is a continuous function of wave-length. Color-blindness means, as a rule, low sensitivity or total insensitivity for red, combined with low sensitivity for violet. Insensitivity for red occurs also in the normal retinal periphery, in the initial stage of atrophy of the optic nerve, and in many cases of simple amblyopia. The occurrence of green-blindness, blue-blindness, and achromatopsy lacks proof.

The most favorable conditions for contrast, as demonstrated in the Meyer and Ragoni-Scina experiments, are: maximal difference of color-tone; an optimal brightness-difference (white on black is no brighter than light-gray on the same black);[12] an optimal saturation of inducing-surface; zero saturation of induced surface; and absence of contours (in Fechner's experiment the contrast-shadow persists when viewed through a tube). While, then, the last factor displays the influence of reproduction, favored by the perception of discrete objects, the others suggest that discrimination is relative in the case of visual quality, as it is in the case of intensity and auditory quality. Direct proof of this relativity may be found in the fact that brightness-contrast remains undisturbed under wide changes of illumination; and no facts are contradicted by generalizing from this special case to the whole group of contrast-phenomena. To dispose of the phenomena of contrast as illusions of judgment (Helmholtz)[13] is to ignore their palpable character and to exalt the secondary factor of reproduction; while to refer them to a supposititious interaction of retinal areas is to disregard the reproduction-factor altogether. Rather do the facts of sensation, taken together, require the positing of a general psychological law of relation, valid for quality as well as for intensity. Sensation, in both of its aspects, is a relative, and not an absolute measure of neural process.

Sense-feelings. Sheer pleasantness and unpleasantness depend, in the first instance, upon degree of sensory intensity. Hence, these feeling-tones are most noticeable in the lower senses, where qualitative distinctions are few; and there they contribute to the apprehension of self and to mood and temperament. In the higher senses, qualitative variety is matched by a corresponding diversity of feelings, which enter into the esthetic appreciation of external objects. High tones are gleeful, low tones have dignity; loud tones give the impression of power and exaltation, soft tones are low-spirited; there is rejoicing in a fast, moodiness in a slow tempo; and clashing, discordant feelings arise from the interplay of fundamental with overtones and the intrusion of beats. Similarly, cheer is found in white, gravity in black; excitement in yellow, depression in blue; and between these poles stand the neutral grays, the tranquil greens, and the instable reds. Affective analogies—such as obtain between deep tones and dark colors, and are implied in terms like 'color-tone,' 'tone-color,' 'cold and warm colors,' 'sharp tones,' and 'saturated colors'—contribute further to esthetic appreciation.

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[12] Cf. 408.

[13] Wundt writes, "I then [*Vorlesungen*, I, 198; *Lehrbuch*, 626], adhered in detail to Helmholtz's theory of contrast; but the facts have convinced me of its untenability (*Grundzüge* 423)."

Wundt also recedes from his previous concurrence with Helmholtz in the explanation of color-mixture (cf. *Grundzüge*, 387 with *Lehrbuch*, 609 f.), of after-images (cf. *Grundzüge*, 398 f. with *Lehrbuch*, 620, 624) and of the flight of colors (cf. *Grundzüge*, 401 with *Lehrbuch*, 622 f.).

While feeling is thus originally determined by intensity and quality, it may be modified through associations. The feeling-tone of black, for example, is enhanced by its association with mourning; but the association, in turn, rests upon the original feeling-tone of black. Other affective connections, such as between green and the peaceful meadow, or between yellow and the cheering sunlight—instances of a remarkable harmony between sensation and external nature—illustrate the principle of adaptation to environment which has governed the development of the human eye.

The inconstancy of feeling is explained, in part, by sensory adaptation and summation. Yet such instances as the euphoria of convalescence and the thrill found in games of chance, in dramatic performances, and in other forms of play suggest that it is of the nature of feeling to rebound from one of its poles to the opposite pole. As Kant said, pleasure must be preceded by pain. The basis for this affective movement, and in fact for feeling itself, is to be found in the relation of sensation to consciousness, which continually oscillates between the opposed states of inhibition and facilitation. Pleasantness goes with facilitation, unpleasantness with inhibition; witness the assertiveness of pain, the easy flow of ideas when one is pleased, the frustration of sight by black, the clash of sensation in beats.

The psychological status of feeling is now apparent. Every sensation has its feeling-tone just as it has its quality and its degree of intensity. But the three sensory determinations do not stand on a level; feeling is dependent upon intensity and quality, and represents a secondary modification imposed upon sensation by the general state of consciousness.[14]

Apprehension. In the apprehension of existing and non-existing objects, of external objects and of oneself, sensations—supplied by peripheral or central stimulation—are involved. But these sensations are now ordered both in space and in time Apprehension, therefore, rests upon a new, integrative activity. The two modes of integration, temporal and spatial, are found together, and in their earliest form, in the tactual perception of bodily movement; they are independently and more highly developed in hearing and in vision.

Tactual perceptions. Tactual localization without the aid of vision implies, in the first place, a local coloring of cutaneous sensations, or local signs, whereby parts of the body are discriminated. Such local differences would account for the limits of localization (expressed in terms of constant error or two-point limen, and pictured by means of overlapping 'sensory circles') as well as for the effects upon localization of practice, sensitivity, and intensity of stimulus. The correlation of size of sensory circle with nerve-supply of the skin points to a structural basis for local signature.

Before, however, the bidimensional system of qualitative local signs can be converted into a spatial continuum, some precise and dependable metric means is required; and only in the fine intensive grading of the sensations of motor innervation is a device of this sort discoverable. Localization may, then, be referred to a synthesis of motor and tactual sensations resting ultimately upon a coördination of tactual and motor impulses, which presumably takes place in the optic thalmus but is also specifically represented in the cortex. In this psychological synthesis,[15] as in a chemical synthesis, the constituents—motor sensations and local signs—lose their identity in the product, spatial order; they cannot, therefore, be observed for themselves, but must be inferred from the conditions—motor and cutaneous—of localization.

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[14] The theory of feeling is given a different turn later in the chapter on Consciousness and Attention.

The early development of the spatial discrimination of parts of the body must have been favored by the unambiguous relation between motor and tactual sensations in bodily movement. The tactual exploration of objects would then lead to the perception of surfaces of varying curvature; and, because of the predominance of rectilinear movement, the object-surfaces would be fitted into a tridimensional framework. At the same time, the possibility of reproduction would render unnecessary a constant reliance upon both local signs and movement. Since bodily movement, with its regularly recurring accents or emphases, also provides a basis for temporal apprehension, it would seem that the earliest perception of all was that of movement, a perception in which temporal and spatial order—later developing separately in hearing and in vision—are given together.

Auditory perceptions. Sounds owe their localization altogether to vision and to touch; specific factors are to be looked for in tensor tympani, pinna, and throat movement. The variety of sounds, their analyzability, and their precise attunement to stimulus-course come into account in other ways: in the apprehension of rhythm and time, of melody, and of tonal relationship.

Related sounds are those containing common or coincident partials. Where coincidence is independent of the fundamental, as in the vocal sound and in timbre, relationship is constant. In the musical interval, on the other hand, there is a variable—or harmonic[16] —relationship between the constituent notes, the degree of resemblance depending upon the relative number and intensity of the common partials. The octave, double octave, and twelfth represent imperfect unisons rather than related tones, for in each case the higher note merely repeats partials already present in the lower note. The closest degree of kinship, then, is to be found in the fifth, with its three audible common partials; next come the fourth, the major sixth, and the major third, with two such partials; and lastly the minor third and other intervals, with but one. In major chords, where the difference-tone furnishes a common fundamental, relationship is indirect. To such an indirect relationship pure tone-intervals owe their harmonic character, although associations may be contributing factors.

Suitable conditions for temporal perception and the perception of rhythm are first found in connection with bodily movement, and later in connection with the visual perception of movement.[17] Hearing, however, affords the most adequate basis for both perceptions. Temporal discrimination is present in the simplest rhythm; every intensive rise is apprehended as a repetition, and with every rise or fall a recurrence is anticipated. Duration is estimated in terms of the rhythmic unit, and is correctly estimated only within the limits of rhythmic organization.

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[15] How Wundt came upon the notion of synthesis, and the changes which it went through between 1858 and 1874, are related by E. B. Titchener, A note on Wundt's doctrine of creative synthesis, this JOURNAL, 33, 1922, 351 ff. Cf. also *Lehrbuch*, 554 f.

[16] Earlier Wundt followed Helmholtz's theory of harmony (cf. *Grundzüge*, 370 f. with *Vorlesungen*, II, 68).

[17] Wundt alters his account of the perception of time in a later chapter (cf. *Grundzüge*, 513 and 681 f.).

In melodic apprehension, qualitative variation is added to the intensive variation upon which rhythm is conditioned; hence the significance of variable tonal relationship (melodic repetition and the law of the tonic) for melody, and of constant tonal relationship (rhyme and assonance) for poetry.

Visual perceptions. The retinal mosaic is fundamental to visual localization. It provides the sensory material for spatial arrangement; and it determines the limits of the total field of vision, and of local discrimination, or visual acuity. Since the cones have more numerous fiber-connections and are more closely packed than the rods, acuity falls rapidly from fovea to periphery. Discrimination varies also with form and size of stimulus-object; and peripheral acuity in particular improves with practice.

The determining factor in visual localization, however, is ocular movement. The orientation of the eye and the direction and magnitude of its excursions are apprehended by way of sensations of innervation and tactual local signs. That these motor and cutaneous sensations enter into fairly uniform connections with retinal local signs is guaranteed by the laws of Listing and Donders; and when the eye is stationary, the impulses of innervation reproduce the appropriate tactual local signs. From the synthesis of these elements, as from the similar synthesis in touch, spatial arrangement results in the form of a surface. The distance of this surface from the eye is defined, rather poorly, by way of accommodation; more precisely, by way of convergence; and, at a later stage of development, by the reproductive effect of perspective. Specifically, the influence of eye-movement evidences itself in the orientation, form, and continuity of the visual field; in the displacement of objects owing to paralysis of eye-muscle; in the discrimination of extents; in the overestimation of vertical distances (except in the case of such familiar figures as the circle) and of distances in the upper and outer portions of the field of vision; in the apparent inclination of verticals; in the overestimation of filled extents; and in the various illusions of visual movement.

This genetic theory of visual perception presupposes sensory and motor mechanisms that are ready to function—for purposes of spatial apprehension—at birth (or, in the case of congenital blindness, as soon as vision is rendered possible); it does not derive perception from empirical inferences or from associations. Perception, which is the basis of experience, cannot itself be the result of experience; and the concept of association is quite different from that of sensory synthesis, since an association does not yield a new product.

The singleness of binocular vision rests, not upon stimulation of identical points, but in the first instance, upon a common localization made possible by the synergy of binocular movement. Through repeated movements of fixation, a paired correspondence of retinal points becomes established which favors, but does not compel, single vision. Common localization is facilitated also by eye-movement, by the presence of fixation lines (if not too long) and of secondary cues to localization, by familiarity of object, and by similarity of the binocular impressions.

The visual field is properly a plane: one cannot see two objects in the same visual direction, at least not with equal distinctness. Of two sufficiently different impressions, one impression prevails altogether (perfect mirroring, retinal rivalry and suppression), or one is more distinctly perceived than the other impression (transparency and mirroring); if the factors favoring each impression balance, luster appears.

Imaginative apprehension. The sensory materials involved in perception recur, in conformity with the rules of association, either as direct supplements to perception or as discrete formations. Thus arise various forms of apprehension: memory and imagination, hallucination, illusion, and dream—all implying a spread of excitation in the sensory areas of the cortex. Normally this secondary excitation is weak, and the reproduced elements are faint; hence, for example, pleasant incidents lose less of their body in recall than painful (and therefore more intensive) experiences do. Cortical excitation may spread further to motor areas; and, in the case of hallucinatory objects that follow the movement of the eyes and leave negative after-images, a centrifugal course of excitation is indicated. In the hallucinatory types of apprehension, moreover, cortical sensitivity is high, and for various reasons: absence of stimulation (silence is conducive to auditory, darkness to visual hallucinations); excessive stimulation (hence the frequency of visual hallucinations); or nutritive disturbances leading to an accumulation of products of decomposition in the cortex. To this last condition, a source of automatic central stimulation, dreams and pathological hallucinations owe their incongruency; yet they draw largely upon reproductions instigated by sensory stimulation and affective tone.

Complex apprehension, generalized apprehension, and perceptive forms. Apprehension is enriched through the concurrence and reproduction of impressions of disparate modalities. This is exemplified in perceiving a sharp point, a grating noise, a heavy weight, an impending blow. Movement also contributes to such complex apprehensions by way of the sensory materials involved in gesture, speech, and writing. Pantomime, onomatopoea, and pictography exemplify the complication or fusion of similar elements, while the association of sounds with visual and other disparate impressions was originally guided by analogies of feeling. For the most part, however, it is sheer habituation that has produced the intimate connection of object, word, and sign.

The change from a specific to a generalized or schematic apprehension of objects requires, not a distinct process of abstraction, but the normal operation of the laws of association. Generalization begins in perception: the features that recur in objects of the same class tend to stand out, while the variable features are neglected. In this way a schematic mode of apprehension develops, which, however, is not stable, but easily resolves itself into a serial apprehension of specific objects within a class. Even animals may attain to this level of generalization; and the child who identifies a circle first as a plate, then as a pond, and then again as the moon, is carrying out the process of resolution, and thereby beginning to comprehend a class of objects.

Schematization, however, is adequate only for certain classes of objects, such as 'circle' or 'green.' For most object-classes the schema becomes too vague, and the process of resolution too cumbersome (*e.g.* 'color'). The realization of this difficulty, at first felt but vaguely, finally leads to the postulation of a generalized object. Such a postulate, or concept, implies no new psychological mode other than schematic apprehension. With the advent of the concept the verbal element in apprehension becomes more prominent; and the verbal symbol is then also pressed into service for the comprehending of relations (such as means and end), to which schematic apprehension is altogether inadequate. The abstract concept —the theoretical as a postulate of thinking, the practical as a postulate of action— develops out of an earlier stage of exemplificatory apprehension (*e.g.* idol-worship precedes the abstract conception of god). To this derivation abstract concepts still owe their feeling-tone and their appearance in correlative pairs.

When thinking of space or time—the two forms of perception—we apprehend a specific stretch of space or of time, and not a symbol as with other concepts. Yet their mode of derivation is similar. The geometric representation of the systems of color and tone neatly illustrate the formation of the space-concept; by bracketing together color-tone, saturation, intensity, and duration, we may even understand a space of four dimensions. It is, nevertheless, inadmissible to suppose, as Zöllner does, that space is curved; for while physical theory may—as it has already done— advance far beyond the direct evidence of perception, it must conform to the general perceptive forms of space and time. On the other hand, psychology has nothing to say concerning the reality of space or of any other concept, or even of the objects of perception; it takes all these for granted, and then seeks to determine the conditions of their apprehension.

Esthetic feelings. At a level between sense-feelings and emotions, one may place the varied class of feelings that color all apprehension, and whose basal modes are liking and disliking. Often indistinct, these 'esthetic' feelings are most obvious in tonal harmony and rhythm, and in the symmetry and proportion of spatial form. But they are also to be discerned in recall and in recognition, in the simple understanding of a sentence and in the solution of a problem, in the ideas of the beautiful and the ugly, the sublime, the ridiculous and the comic. It is evident that these feelings imply evaluation. They may be said to reflect the degree to which the diversity given in apprehension is unified. They thus express the general conscious status of apprehension, just as the sensory feelings indicate the status of sensation.

Consciousness and attention. Conscious life does not begin with sensation; nor does it involve from the outset a consciousness of self; nor, finally, can it spring from unconscious inferences.[18]

Self-awareness and inference, both, appear at a fairly late period of development; they imply the use of concepts. The explicit awareness of self, moreover, presupposes, first, the perceptive discrimination of external objects from one's own body and, secondly, the differentiation of memory and imagination from perception.

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[18] In the last two clauses, Wundt negatives the fundamental ideas of the *Beiträge* and the *Vorlesungen.* Even sensation had been made out to be an unconscious inference (*Beiträge*, 445 ff.; *Vorlesungen*, I, 52 ff., 57 ff., 305.).

The single sensation, on the other hand, is an abstraction. The sound we hear has a certain duration; the color we see is localized. Synthesis operates from the beginning; and wherever synthesis, even of the most rudimentary kind, is rendered possible by conditions of central organization, some level of consciousness must therefore be assumed to exist. In animals possessing various levels of neural integration, the unitariness of consciousness is nevertheless guaranteed by the close interrelation of the various neural mechanisms. Some animals, it is true, exhibit signs of consciousness after decerebration; such consciousness, however, is not residual, but a new development.

Alongside of sensory synthesis and of association, a third psychological activity must now be recognized: attention or apperception. All objects are not apprehended on equal terms. Under instantaneous illumination, several words can be clearly read; if, however, one is intent upon the precise form of a single letter, all the other letters become less clear; while with repetition the range of the attentive 'point of regard' may increase. The same phenomena can be demonstrated with a combination of tones. The wider range of the whole conscious field of regard may be inferred from the limits of rhythmic complexity and of consecutive thinking.

While attention is favored by reproduction (as the aforementionede ffect of repetition indicates), and also by stimulus-intensity and visual fixation and contours, various facts bespeak for it a single primary subjective condition. Under attention, apprehension of disparate objects (for example, the sound of the bell and the sight of the pointer in the complication experiment) is unitary. Then there is the peculiar feeling that may be observed during the accommodation of attention. It bears a personal reference; it becomes fairly tense in expectation and surprise, and in effortful thought, imagination, and recollection; it may be accompanied by innervation of the muscles about the eyes, ears and head; it suggests, finally, innervational functioning. One may, therefore, conceive of the cortical innervation-areas as operating in two ways: they shunt the course of excitation back to the sensory spheres, whose activity is thus reënforced; and they also innervate the bodily musculature. It follows that between attention and voluntary action there is simply a difference in the relative emphasis of these two innervational modes. The central determination of all feelings, which we have earlier derived from the total state of consciousness, may now, also, be more specifically referred to innervation.

The course of apprehension; association. Thec ourse of apprehension is regulated by the accommodation of attention. The shortest 'physiological time'[19] is obtained under conditions permitting adequate accommodation: foreknowledge of the object reacted to and of the time of its appearance, and absence of distraction. Accommodation also governs the time-relations both of perceived objects (cf. the 'personal equation')[20] and of memorial objects (cf. the experiments on the comparison of time-intervals).

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[19] Wundt here (*Grundzüge*, 733) reports experiments performed by himself and his wife.

[20] Wundt brought the 'personal equation' into the laboratory in 1860, when he devised the complication experiment. In 1862, in the course of a general attack on Herbartianism, he made the bold announcement that this experiment disproved the metaphysical axiom upon which the whole Herbartian psychology had been

The possibility of reproduction rests upon a facilitating after-effect of cortical excitation. The after-effect is of a functional sort, analogous to the effect of practice upon muscular movement or upon visual acuity rather than to the after-effect of visual sensation. Such a dispositional after-effect underlies the laws of association by spatial and temporal contiguity. These two laws, both of which imply a tendency of excitation to recur in the manner to which it has been habituated, may be combined into a single law of associative habituation. The law of similarity presupposes, in addition, a transfer-effect of associative practice. Since the sensory areas of the cortex are dominated by the frontal areas of innervation, the laws of association cannot operate automatically. They are always subject to the accommodation of attention, although this may sometimes, as in dreams and in the pathological flight of ideas, be fairly slack; and the guiding influence of accommodation grows stronger with proper training. What appears to be association by contrast really points to a peculiarity of attention: when accommodation continues for a long time in the same direction, fatigue sets in, encouraging a maximally different direction of accommodation.

Emotion, impulse, and temperament. Emotion and impulse are both expressions of innervational activity. In emotion, attention is, as it were, taken by surprise; in impulse, it is in a state of expectation.

In all pronounced instances of emotion, the affective situation inhibits the flow of ideas; if the emotion is pleasant, a sudden release of inhibition may follow; all emotions gradually subside, and are replaced by moods. At the same time, the changing state of innervation and of feeling is reflected in sthenic and asthenic tonicity and in alteration of visceral function; this bodily reverberation is the same in all emotions of sufficient intensity.

The impulse is characterized by a forward reference to an affective situation, with appropriate motor consequences. Its two opposed modes of desire and resistance meet in the neutral mode of expectation. The mere accommodation of attention may thus be regarded as an elementary impulse. The more primitive impulses, like the early perceptions, command ready-made sensory and motor mechanisms; yet they are not mere reflexes. Their direction, at first vague, is defined only as apprehension develops. Nor does psychological development con-

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erected. Herbart, by the use of his mathematical method, had demonstrated that the sum of ideas held simultaneously in consciousness was never less than two, while the relative displacement of bell and pointer in the complication-experiment —so ran Wundt's argument—meant that we can apprehend only one thing at a time (*die Einheit der Vorstellung*). Cf. *Beiträge*, xxi (footnote), xxvii f., 335, 382 f.; *Vorlesungen*, I, Lectures III and XXIII, and 469; *Grundzüge*, v, 757; J. F. Herbart, *Psychologie als Wissenschaft*, sec. 44; E. B. Titchener, Wundt's address at Speyer, 1861, this JOURNAL, 34, 1923, 311; E. G. Boring, *A History of Experimental Psychology*, 1929, 247 ff., 341. Drobisch replied vehemently for the Herbartians in the review mentioned above. In the *Grundzüge*, Wundt tacitly drops the argument.

Boring writes (*op. cit.*, 144) that "Müller's belief in the instantaneousness of nerve action was overthrown by Helmholtz's measurement of the velocity of the nervous impulse, but his view nevertheless persisted as the basis of Wundt's psychology of the 'complication.'" Cf., however, *Vorlesungen*, I, Lecture III, where Wundt writes "Just a few years ago, a German physiologist, Helmholtz, determined with precision the true rate of conduction of sensory and motor process in nerves" (33), and then uses Helmholtz's determination to support his own explanation of the complication experiment.

sist in the elimination or repression of the primitive impulses, but in the appearance of new and more varied forms, which, of course, bring with them the possibility of conflict.

Emotional and impulsive dispositions, or temperaments, vary more or less with individual, nation, race, and species. One may discriminate, with Galen, four temperamental types. Intense emotion and dark mood distinguish the choleric and the melancholic from the phlegmatic and the sanguine. While the sanguine and the choleric change their mood with the occasion, the brooding and the conservative types are not so easily distracted. Lastly, one may set off the affectivity of the melancholic and the sanguine against the activity of the choleric and the phlegmatic. Each temperament has its good and its bad points; the wise man will, therefore, cultivate all of them.

Action. There is no sharp cleavage between voluntary action, or action initiated by impulse, and non-voluntary action. The impulse usually indicates the general direction of the action, without prescribing its specific course. Under repetition, intention may lapse altogether or may show itself only in starting the actional sequence and in bringing it to a close. Such facts cannot be accounted for by invoking now the agency of the will and now unconscious or mechanical determination. All actions are sustained by the operation of organic devices. In voluntary action, essentially the same mechanisms are involved as in non-voluntary action; but initiation is cortical, since the impulse implies innervational activity. The course of apprehension resembles that of action: once started by apperceptive activity, it may be carried on by way of associative linkage with only occasional interference on the part of apperception. The two modes of innervational function thus behave in a like manner.

Expressive movements. Expressive movements have various origins. The tonic and visceral indices of emotion are, for the most part, direct consequences of innervation; expressions of the mouth and nose are specific sensory reactions that have become integrated with diverse situations by virtue of a common affective coloring; and gestures can be traced to early perceptive situations. Speech began as vocal gesture; as such it may still be observed in the deaf-mute. Both speech and gesture presuppose a discreteness of perceived objects that is lacking in animals and in human infants. The further development of speech was aided by onomatopoea and affective analogy. The comprehension of speech was facilitated by gestural comment, just as facial expression contributed to the understanding of gesture.

Concluding remarks. All psychological processes—sensation, synthesis, association and apperception—are physiologically grounded; and just as the organism is a unity of many parts, so mind, its inner counterpart, is a unitary connection of multiple processes. These principles suggest the monistic conception that the world consists of simple beings variously integrated, and undergoing parallel changes externally and internally. When integration becomes sufficiently persistent and coherent—a level attained in animal life—the internal processes take the form of sensation and apprehension. In this sense, mind may be regarded, with Leibnitz, as a mirror of the world.

WUNDT'S DOCTORATE STUDENTS AND THEIR THESES

1875–1920

By Miles A. Tinker, University of Minnesota

As an historical figure, Wundt is rapidly achieving the eminent position earned by his contributions to the advancement of scientific psychology. The centennial of the birth of this "senior psychologist in the history of psychology" was August 16, 1932. A survey and list of the theses done under his guidance during his professorship at Leipzig may be fittingly given at this time.

Since 1885 Leipzig theses and their authors are listed in the successive volumes of *Jahresverzeichnis der an den deutschen Universitäten erschienenen Schriften.* There is no published list prior to that time. The titles and names listed below were obtained for the period ending 1884 from the records filed at the University of Leipzig and since 1885 from the *Jahresverzeichnis.*

From 1875 to 1919 Wundt directed 186 theses. The frequencies by quinquennia follow: 1875–80 (6 years), 12 degrees; 1881–85, 23 degrees; 1886–90, 26 degrees; 1891–95, 28 degrees; 1896–1900, 23 degrees; 1901–05, 25 degrees; 1906–10, 25 degrees; 1911–15, 22 degrees; 1916–20, 2 degrees. While during the first six-year period less than two degrees per year were granted by Wundt, from 1880 to 1915 he sponsored about five per year and they were spread quite uniformly through the seven quinquennia. This is an impressive record, especially impressive when one remembers that Wundt was 83 years old in 1915. Although he retired in 1917 Wundt was major adviser on two theses completed in 1919. The fact that Wirth was minor adviser on several of the theses granted by Wundt between 1911 and 1920 indicates that the latter was probably transferring some of his responsibilities to his colleagues during this period. The first Leipzig thesis sponsored by Wirth as major adviser is dated April, 1919 (1921) in the 1921 *Jahresverzeichnis.*

In considering the Wundtian doctorates the reader should remember that many students (not listed here), who did not take their degrees at Leipzig, studied in Wundt's laboratory. Wundt was, of course, minor adviser on numerous theses (not tabulated here) in philosophy, pedagogy, ethics, and anthropology. The list of the 186 men obtaining degrees and their thesis titles follows:

1876

Steffen, Robert. Kants Lehre vom Dinge an sich.

Swientochowski, Alexander. Ein Versuch die Entstehung der Moralgesetze zu erklären, eine ethische Analyse.

1877

Dimitresco, Constantin. Der Schönheitsbegriff, eine ästhetischpsychologische Studie.

1878

Bolliger, Adolf. Das Problem der Causalität.

Fischer, Wilhelm. Herder's Erkenntnislehre und Metaphysik.

Hartung, Ernst Bruno. Grundlinien einer Ethik bei Giordano Bruno, besonders nach dessen Schrift Lo spaccio de la bestia trionfante.

Soboyo, Peter. Das Pythagoreische System in seinen Grundgedanken.

1879

Gude, Wilhelm. Die Gesetze der Physiologie und Psychologie über Entstehung der Bewegungen und der Articulationsunterricht der Taubstummen.

Tzerteleff, Dimitrij. Schopenhauer's Erkenntnistheorie: Eine kritische Darstellung.

Vujic (Wuitsch), Michael. Über Substanz und Causalität.

1880

Radestock, Paul. Schlaf und Traum. Eine physiologisch-psychologische Untersuchung.

Tischler, Otto. Ostpreussische Gräberfelder.

1881

Borries, Gottfried. Über den Pessimismus als Durchgangspunkt zu universaler Weltanschauung.

Donadt, Alfred. Das mathematische Raumproblem und die geometrischen Axiome.

Friedrich, Max. Über die Apperceptionsdauer bei einfachen und zusammengesetzten Vorstellungen.

Horny, Friedrich Wilhelm. Kurze kritische Darstellung der Anfänge und Entwickelung der utilitarischen Moralphilosophie in England.

König, Edmund. Über den Substanzbegriff bei Locke und Hume.

1882

Schmerler, G. B. Th. Untersuchungen über den Farbencontrast vermittelst rotirender Scheiben.

Tischer, Ernst Th. Über die Unterscheidung von Schallstärken.

Trautscholdt, Martin. Experimentelle Untersuchungen über die Association der Vorstellungen.

1883

Estel, Volkmar. Neue Versuche über den Zeitsinn.

Korner, Christian Reinhold. Die logischen Grundlagen der Systematik der Organismen.

Merkel, Julius. Die zeitlichen Verhältnisse der Willenstätigkeit.

Schneider, Georg. Der menschliche Wille vom Standpunkte der neueren Entwickelungstheorien.

1884

Rosenstein, Alfred. Die psychologischen Bedingungen des Bedeutungswechsels der Wörter.

Stern, Albert. Über die Beziehungen Chr. Garve's zu Kant, nebst mehreren bisher ungedruckten Briefen Kant's, Feder's und Garve's.

1885

- Berger, Oscar. Über den Einfluss der Reizstärke auf die Dauer einfacher psychischer Vorgänge mit besonderer Rücksicht auf Lichtreize.
- Bixby, James Thompson. Herbert Spencer's Data of Ethics and Transmutational Ethics.
- Dietze, Georg W. Untersuchungen über den Umfang des Bewusstseins bei regelmässig aufeinanderfolgenden Schalleindrücken.
- Knabe, Carl. Die Formen des indirecten Beweises mit besonderer Rücksicht auf ihre Anwendung in der Mathematik.
- Lorenz, Gustav. Die Methode der richtigen und falschen Fälle in ihrer Anwendung bei Schallempfindungen.
- Mehner, Max. Die Lehre vom Zeitsinn.
- Münsterberg, Hugo. Die Lehre von der natürlichen Anpassung in ihrer Entwickelung, Anwendung und Bedeutung.
- Nedich, Ljubomir. Die Lehre von der Quantification des Prädicats in der neueren englischen Logik.
- Selver, David. Der Entwicklungsgang der Leibniz'schen Monadenlehre bis 1695.

1886

- Cattell, James McKeen. Psychometrische Untersuchungen.
- Gühne, Herm. Bernh. Über Hobbes naturwissenschaftliche Ansichten und ihren Zusammenhang mit seiner Zeit.
- Köhler, Alfred. Über die hauptsächlichsten Versuche einer mathematischen Formulirung des psychophysischen Gesetzes von Weber.
- Lange, Ludwig. Der Bewegungsbegriff während der Reformation der Himmelskunde von Copernicus bis zu Newton (1543 bis 1687).
- Starke, Paul. Die Messung von Schallstärken.
- Wolfe, Harry Kirke. Untersuchungen über das Tongedächtniss.

1887

- Epstein, Joseph. Die logischen Principien der Zeitmessung.
- Glass, Richard. Kritisches und Experimentelles über den Zeitsinn.
- Külpe, Oswald. Zur Theorie der sinnlichen Gefühle.
- Schanz, Johannes. Das Freiheitsproblem bei Kant und bei Schopenhauer, eine kritische Untersuchung.

1888

- Bobtscheff, (W.), Nicolaus. Die Gefühlslehre in ihren hauptsächlichsten Gestaltungen von Kant bis auf unsere Zeit.
- Gloeckner, Gottfried. Der Gottesbegriff bei Leibniz.
- Lipps, Gottlob Friedrich. Die logischen Grundlagen des mathematischen Functionsbegriffs.
- Luft, Eduard. Über die Unterschiedsempfindlichkeit für Tonhöhen.
- Thieme, Karl. Der Primat der praktischen Vernunft bei Lotze.

1889

- Brix, Walter. Der mathematische Zahlbegriff und seine Entwicklungsform.
- Schischmanow, Iwan. Untersuchungen über die Empfindlichkeit des Intervallsinnes.
- Zeitschel, Hermann Richard. Die Erkenntnislehre Spinoza's.

1890

- Fischer, Ludwig. Cogito ergo sum.
- Gimkiewicz, Benno. Über Herbarts Methode der Beziehungen.
- Kirschmann, August. Über die quantitativen Verhältnisse des simultanen Helligkeits- und Farbencontrastes.
- Krestoff, Kresto. Lotzes metaphysischer Seelenbegriff.
- Lorenz, Carl. Untersuchungen über die Auffassung von Tondistanzen.
- Schmid, Albert. Zu Kants Lehre vom Raum.
- Schubert, Johannes. Adam Smith's Moralphilosophie.
- Türk, Hermann. Das psychologische Problem in der Hamlet-Tragödie.

1891
Angell, Frank. Untersuchungen über die Schätzung von Schallintensitäten.
Benecke, Max. Vom Takt in Tanz, Gesang und Dichtung.
Bondi, Georg. Das Verhältnis von Hallers philosophischen Gedichten zur Philosophie.
Page, Eduard. Das Relativitätsprincip in Spencer's psychologischer Entwickelung.
Schleimer, Alexis. Der Positivismus, eine kritische Studie.
Scripture, Edward. Über den associativen Verlauf der Vorstellungen.

1892
Rees, Daniel. Contemporary English Ethics.
Titchener, Edward. Über binoculare Wirkungen monocularer Reize.
Uhlitzsch, Richard. Anthropometrische Messungen und deren praktischer Werth.

1893
Eckener, Hugo. Untersuchungen über die Schwankungen der Auffassung minimaler Sinnesreize.
Gruber, Eduard. Experimentelle Untersuchungen über die Helligkeit der Farben.
Kämpfe, Bruno. Beiträge zur experimentellen Prüfung der Methode der richtigen und falschen Fälle.
Paulins, Johannes Martin. Die Sittenlehre Geulincx.
Radulescu-Motru, Constantin. Zur Entwicklung von Kant's Theorie der Naturcausalität.
Richter, Raoul. Schopenhauers Verhältnis zu Kant in seinen Grundzügen. I. Theil.
Stier, Theodor. Analyse und Kritik der Berkeley'schen Erkenntnistheorie und Metaphysik.
Wenzel, Alfred. Beiträge zur Logik der Sozialwirtschaftslehre.
Weygandt, Wilhelm. Entstehung der Träume.
Witmer, Lightner. Zur experimentellen Ästhetik einfacher räumlicher Formenverhältnisse.

1894
Eisler, Rudolf. Die Weiterbildung der Kant'schen Aprioritätslehre bis zur Gegenwart.
Kiesow, Friedrich. Beiträge zur physiologischen Psychologie des Geschmacksinnes.
Kühn, Victor. Kurze Darstellung und Kritik der praktischen Ideen Herbarts von Standpunkt religiöser Heteronomie.
Schmid, Sebastian. Schopenhauers Willensmetaphysik in ihrem Verhältnis zu neueren Ansichten über den Willen.
Smith, William G. Zur Frage der mittelbaren Association.
Turban, Theodor. Das Wesen des Christentums von Ludwig Feuerbach.

1895
Heller, Theodor. Studien zur Blinden-Psychologie.
Mentz, Paul. Die Wirkung akustischer Sinnesreize auf Puls und Athmung.
Thiéry, Armand. Über geometrisch-optische Täuschungen.

1896
Beck, Paul. Die Substanzbegriffe in der Naturwissenschaft.
Bon, Siegfried. Grundzüge der wissenschaftlichen und technischen Ethik.
Friedrichs, Max. Der Substanzbegriff Spinozas neu und gegen die herrschenden Ansichten zu Gunsten des Philosophen erläutert.
Grieve, Alexander. Das geistige Princip in der Philosophie Thomas Hill Greens.
Jovanovitsch, Milevoje. Die Impersonalien: Eine logische Untersuchung.
Judd, Charles H. Über Raumwahrnehmungen im Gebiet des Tastsinns.
Stratton, George. Über die Wahrnehmung von Druckänderungen bei verschiedenen Geschwindigkeiten.

1897
- Arrer, Max. Über die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung.
- Rostosky, Paul. Über funktionelle Beziehungen beider Gehörorgane.
- Sherman, Frederic David. Über das Purkinje'sche Phänomen im Centrum der Netzhaut.
- Tawney, Guy Allan. Über die Wahrnehmung zweier Punkte mittelst des Tastsinnes, mit Rücksicht auf die Frage der Übung und die Entstehung der Vexirfehler.

1898
- Müller, Robert. Über Raumwahrnehmung beim monocularen indirecten Sehen.
- Weyer, Edward Moffat. Die Zeitschwellen gleichartiger und disparater Sinneseindrücke.
- Wintzer, Wilhelm. Die ethischen Untersuchungen Ludwig Feuerbachs.

1899
- Jasper, Joseph. Leibniz und die Scholastik.
- Mosch, Erich. Zur Methode der richtigen und falschen Fälle im Gebiete der Schallempfindungen.
- Radoslawow-Hadjidenkow, Zwetan. Untersuchungen über das Gedächtnis für räumliche Instanzen des Gesichtssinns.
- Schwabe, Paul. Michel de Montaigne als philosophischer Charakter.

1900
- Alechsieff, Nicolaus. Reactionszeiten bei Durchgangsbeobachtungen.
- Hellpach, Willy. Die Farbenwahrnehmung im indirecten Sehen.
- Savescu, Michael. Die Gefühlslehre in der neuesten französischen Psychologie.
- Scott, Walter. Die Psychologie der Triebe historisch-kritisch betrachtet.
- Zeitler, Julius. Tachistoskopische Untersuchungen über das Lesen.

1901
- Hänig, Pauli. Zur Psychophysik des Geschmackssinnes.
- Linke, Paul. D. Hume's Lehre vom Wissen.
- Von Voss, Robert. Über den Begriff der Erkenntnis insbesondere der intuitiven bei Spinoza.

1902
- Bargmann, Hermann Christian. Der Formalismus in Kants Rechtsphilosophie.
- Bauch, A. P. J. Die Psychologie Arthur Schopenhauers, dargestellt als Lehre von den Motiven.
- Dürr, Ernst. Über das Ansteigen der Netzhauterregungen.
- Endler, Richard. Kants physische Monadologie im Verhältnis zur Philosophie und Naturwissenschaft der Zeit.
- Hennig, Max. A. E. Biedermanns Psychologie der religiösen Erkenntnis.
- Seyfert, Richard Hermann. Über die Auffassung einfachster Raumformen.
- Squires, Wilhelm Harder. Jonathan Edwards und seine Willenslehre.

1903
- Gent, Werner. Volumenkurven bei Gefühlen und Affekten.
- Hoffman, Heinrich. Die Leibniz'sche Religionsphilosophie in ihrer geschichtlichen Stellung.
- Köhler, Johannes. Der simultane Farben- und Helligkeitskontrast mit besonderer Berücksichtigung des sog. Florkontrastes.

1904
- Freudenreich, Hans. Fechners psychologische Anschauungen.
- Gusti, Demetrius. Egoismus und Altruismus. Zur soziologischen Motivation des praktischen Wollens.
- Krumbholz, Friedrich. Schleiermachers Weltanschauung in den Monologen und die literarischen-philosophischen Voraussetzungen dazu.

Peters, Wilhelm. Die Farbenempfindung der Netzhautperipherie bei Dunkeladaptation und konstanter subjektiver Helligkeit.

1905

Bergemann, Robert. Reaktionen auf Schalleindrücke nach der Methode der Häufigkeitskurven bearbeitet.

Kobilecki, Stanislaus. Über die Wahrnehmbarkeit plötzlicher Druckveränderungen.

Mitzscherling, M. A. F. Die Farbenkurve bei Reduktion auf gleiche Helligkeiten.

Olshausen, Waldemar. Friedrich von Hardenbergs (Novalis) Beziehungen zur Naturwissenschaft seiner Zeit.

Quandt, Johs. Wilhelm. Bewusstseinsumfang für regelmässig gegliederte Gesamtvorstellungen.

Reuther, Fritz. Beiträge zur Gedächtnisforschung.

Richter, Otto. Kants Auffassung des Verhältnisses von Glauben und Wissen und ihre Nachwirkung besonders in der neueren Theologie.

Spearman, Charles. Die Normaltäuschungen in der Lagewahrnehmung.

1906

Büchner, Max. Über das Ansteigen der Helligkeitserregung.

Bode, Rudolf. Die Zeitschwellen für Stimmgabeltöne mittlerer und leiser Intensität.

Kafka, Gustav. Über das Ansteigen der Tonnerregung.

Klemm, Otto. G. B. Vico als Geschichtsphilosoph und Völkerpsycholog.

Pfeifer, Rich. Arwed. Über Tiefenlokalisation von Doppelbildern.

Reiss, Josef. Kritik der Willensphänomene des Privatrechts.

1907

Berliner, Bernhard. Der Anstieg der reinen Farbenerregung im Sehorgan.

Katzenellenbogen, Edwin. Die zentrale und periphere Sehschärfe des hell- und dunkeladaptierten Auges.

Keller, Johannes. Die Methode der mehrfachen Fälle im Gebiete der Schallempfindungen und ihre Beziehung zur Methode der Minimaländerungen.

Mittenzwey, Cuno. Über abstrahierende Apperzeption.

Salow, Paul. Der Gefühlscharakter einiger rhythmischer Schallformen in seiner respiratorischen Äusserung.

Wiże, Kasimir Filip. Friedrich Justus Riedel und seine Ästhetik.

1908

Arps, George. Über den Anstieg der Druckempfindung.

Kästner, Georg. Untersuchungen über den Gefuhlseindruck unanalysierter Zweiklänge.

Menderer, Otto. Raumtäuschungen des Tastsinns bei annormaler Lage der tastenden Organe.

Schwarze, Karl. Die Ethik Herbert Spencers, eine kritische Studie.

1909

Schilling, Roland. Die realistischen Elemente der Lotzeschen Ontologie.

1910

Berlage, Franz. Der Einfluss von Artikulation und Gehör beim Nachsingen von Stimmklangen.

Deuchler, Gustav. Über Reaktionsversuche mit unbestimmter Erwartung.

Goldschmidt, Rich. Quantitative Untersuchungen über positive Nachbilder.

Heyde, Curt. Über gleichzeitige Durchgangsbeobachtungen bei verteilter Aufmerksamkeit.

MacEachran, John. Pragmatismus.

Poschoga, Nicolai. Die successive und simultane Raumschwelle im indirekten Sehen.

Ristitsch, Swetonia. Die indirekten Beweise des transzendentalen Idealismus. Ein kritischer Beitrag zur Kantforschung.

Sander, Paul. Das Ansteigen der Schallerregung bei Tönen verschiedener Höhe.

1911

Drozynski, Leon. Atmungs- und Pulssymptome rhythmischer Gefühle.

Günther, Friedrich. Reaktionsversuche bei Durchgangsbeobachtungen.

Rehwoldt, Friedrich. Über respiratorische Affecktsymptome.

Stefanescu-Goanga, Florian. Experimentelle Untersuchungen zur Gefühlsbetonung der Farben.

1912

Jesinghaus, Carl. Beiträge zur Methodologie der Gedächtnis-Untersuchung.

Lorenz, Johannes. Unterschiedsschwellen im Sehfelde bei wechselnder Aufmerksamkeitsverteilung.

Sartorius, Hermann. Der Gefühlscharakter einiger Akkordfolgen und sein respiratorischer Ausdruck.

Stephanowitsch, Johann. Untersuchung der Herstellung der subjektiven Gleichheit bei der Methode der mittleren Fehler unter Anwendung der Registriermethode.

Volkelt, Hans. Über die Vorstellungen der Tiere: Ein Beitr. z. Entwicklungspsychol.

Westphal, Willy. Untersuchung der sphygmographischen und pneumographischen Symptome bei Wahlreaktionen.

1913

Herfurth, Curt. Die Konstanz des mittleren Schätzungswertes bei Umkehrung der Lage des Normal- und Vergleichsreizes.

Kramers, Lambertus W. Experimentelle Analyse eines einfachen Reaktionvorganges unter gleichzeitiger Registrierung der pneumographischen und sphygmographischen Symptome.

Kraskowski, Clemens. Die Abhängigkeit des Umfanges der Aufmerksamkeit von ihrem Spannungszustande.

Lohnert, Karl. Untersuchungen über die Auffassung von Rechtecken.

Sander, Friedrich. Elementarästhetische Wirkungen zusammengesetzter geometrischer Figuren.

1914

Hammer, Albert. Untersuchungen der Hemmung einer vorbereiteten Willenshandlung.

Kahnt, Otto. Über den Gang des Schatzungsfehlers bei der Vergleichung von Zeitstrecken.

Kobelt, Werner. Untersuchung über die Farbenunterschiedsempfindung bei Schulkindern.

1915

Buchholz, Gerhard. Über die Beeinflussung tachistoskopischer Auffassung durch vorangehende Eindrücke.

Burger, Hermann. Über den Einfluss der Rhythmisierung von Eindrücken auf den Umfang des Bewusstseins.

Giese, Fritz. Untersuchungen über die Zöllnersche Täuschung.

Topciu, Trajan. Aufeinanderfolge zweier einfacher Reaktionen mit rechter und linker Hand bei Variation des Reizintervalls.

1919

Müller, Paul. Verlauf einer vorbereiteten Willensbewegung.

Paulssen, Berta. Einfache Reaktionen bei Variation und rhythmischer Gliederung der Vorperiode.

An attempt has been made by 'inspection' of the titles of the theses and their authors' names roughly to classify the subject-matter investigated and the nationality of the students. While such classification can not be done without errors, the approximation is sufficiently accurate to show major trends. Many of the theses were concerned mainly with philosophy. The frequencies of theses concerned with psychological and philosophical subjects for successive periods follow: 1875–80, two psychological and 10 philosophical; 1881–90, 27 and 22 respectively; 1891–1900, 32 and 19; 1901–10, 31 and 19; 1911–20, 24 and 0. From these data it is seen that the psychological work did not get under way until the early eighties and that from then on psychology predominated over philosophy. The ratio between psychological and philosophical theses is approximately 3 to 2 for each period after 1880 except the last, in which all theses were psychological.

Further analysis of the psychological theses reveals that about 70% can be classified as *sensation and perception* in subject-matter, 9% as *method*, 10% as *attention and feeling*, and most of the rest as scattered between *action*, *association*, and *memory*. Sub-groupings under *sensation and perception* include *vision*, 28% (of total psychological theses); *audition*, 23%; *touch*, 5%; *time-sense*, 8%; and the rest divided between *taste* and *method*.

Inspection of the names led to the following classification of nationalities: 136 German (or Austrian) students; 14 American; 10 English; 13 Balkan (from Bulgaria, Roumania, etc.); 6 Polish; 2 French; 2 Danish; 3 Russian. For the first 7 years and from 1896–1900 the Germans were fewer in numbers, but during each of the remaining quinquennia to 1916 there were about 20 names. Both the Balkan and the English students were fairly uniformly distributed through the eight periods, but nearly all (12 of the 14) Americans took their degrees between 1886 and 1900.

A CRITICAL EXAMINATION OF THE INVESTIGATIONS OF AUDITORY ACTION CURRENTS

By George Kreezer, Cornell University

For many years a controversy has existed between the adherents of the 'place' and of the 'frequency' theories of hearing. In the 'place' theories it is supposed that the neural correlate of the various distinguishable frequencies of sound consists of impulses in different nerve fibers (terminating at different places on the basilar membrane); in the 'frequency' theories the various stimulus frequencies are thought of as retained in the neural response. With the development of techniques for detecting action-currents in nerves, attempts were, naturally, early made to get a direct decision of the question by the application of this technique to the auditory nerve. But the earlier experiments were inconclusive, apparently on account of the lack of recording instruments that could respond at relatively high frequencies and still be sensitive enough to detect the action-currents. The development of electrical amplification technique with the advent of radio made it possible, however, to use instruments which though in themselves less sensitive were capable of responding at higher frequencies. So the telephone, which had early been in common use for determining the frequency of tetanic muscular contractions, was turned to again, but it now served, in conjunction with powerful amplifiers, as an indicator of the frequency of auditory action currents. As a result of such experiments with telephone and amplifier, it has been found that currents led off from the auditory nerve of the cat in response to sound stimuli, and conducted through amplifier and telephone, gave rise in the telephone to tones of the same pitch as the sounds serving as stimuli. This result, in conjunction with certain experimental checks, has led many of the recent investigators to the conclusion that the fluctuating currents detected were auditory action currents, and therefore, that impulses in the acoustic nerve correspond in frequency with the sound stimulus.

This conclusion is of such great theoretical and practical importance that it is obvious that the grounds upon which it rests must be

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very carefully scrutinized. The various investigations have not all been made under identical conditions; and the logic underlying the interpretations of the results has not always been made explicit. It seems desirable, therefore, to bring together the results of all of these investigations, along with certain other relevant facts, in order to determine just how substantial is the logical and experimental groundwork upon which rests the conclusion mentioned above. That is the purpose of the present paper. The general questions which we wish to answer are: (1) Does any single investigation provide conclusive evidence that the microphonic effect which was detected is due to action currents of the acoustic nerve? (2) To what extent do the different investigations consider the same problems and obtain corroborative results? Our procedure will be first, to review some of the fundamental facts concerning the electrical properties of tissues, then to itemize and analyze various microphonic phenomena which might be involved in investigations of the type being considered, and finally, with these considerations in mind, to pass to an examination of the various experimental investigations that have been made.

The Electrical Properties of Tissues

As we shall later see, whether certain microphonic phenomena may be expected to occur will depend on whether the tissues possess certain essential electrical properties. We wish, therefore, to recall some of the basic facts concerning the electrical properties of tissues and the way these may change under various conditions.[1]

Almost all of the electrical properties found in non-living materials are exhibited under some circumstances by living tissues. Experiments show that living tissues conduct direct currents and that they must, therefore, be designated as having a certain conductance (or resistance). Experiments with alternating currents of various frequencies show that the tissues also possess capacity. These properties may be measured by methods like those used with non-living materials. The capacity and resistance are found to vary for different kinds of tissue and for the same tissues under different conditions. Experiments show an apparently close relation between the functional and electrical properties of the tissues; consequently much of the experimental work upon the changes in the electrical properties under different conditions represents attempts to use such measures as indicators of the functional state of the tissues. The capacity and resistance are also found to vary with changes in the frequency of the alternating current being conducted. Different conventions are current for representing the manner of combination of the resistance and capacity possessed by the tissues. For representing the passage of direct currents through them, it is customary to regard the tissues as equivalent to a particular resistance and capacity in parallel, as for a leaky condenser. For representing the properties of a tissue in transmitting alternating currents, it is often more convenient to regard the tissue as equivalent to a resistance and capacity in series (and to specify the changes that occur in this capacity and resistance at different frequencies). It is of course always possible, where the passage of alternating current is involved, to determine, by means of an appropriate formula, the values for a capacity and resistance in parallel which will be equivalent to a particular resistance and capacity in series. In terms of the actual structures and phenomena in the tissues responsible for its capacity, a given stretch of tissue is more appropriately represented as a group of condensers in series than as a single condenser with the measured capacity. Every place where there occurs a limiting membrane which is responsible for the piling up of ions may be regarded as corresponding to a new dielectric layer between two condenser plates. In terms of this view, the actual capacity possessed by the 'individual condensers' would be far greater than the measured value obtained for a whole stretch of tissue which is equivalent to a number of such 'unit-condensers' in series.[2]

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[1] M. Gildermeister, Passiv elektrische Erscheinungen, *Handb. d. normalen u. pathol. Physiol.*, 8 (2), 1928, 657-681.

The tissues also possess characteristics which may be likened to the electromotive force generated by batteries, though the nature of the fundamental phenomena responsible for them is still a matter of theory. If two places on a particular kind of tissue or on different tissues are connected in a circuit containing a galvanometer, a current may pass through the instrument between the two points. Such currents are called demarcation or injury currents, and the potential differences upon which they depend may be designated as injury or resting potential differences.[3] The term injury is used since it has been believed that if each of two places on the same tissue are completely uninjured, there will be no potential difference between them, although it is often difficult experimentally to prepare a given tissue so that two places on it will show no such resting potential difference. Hoffman,[4] for example, reports that in experiments made for measuring the injury currents of muscle tissues, marked differences in the magnitudes of the currents were found even though extreme care was used to prepare the specimens examined in exactly the same way. The maximum injury potential differences obtainable are different for different tissues and for the same tissues under different conditions. The maximum potential difference occurs when one point has been maximally injured (killed) and the other is completely uninjured. If both are somewhat injured the potential difference between them will naturally be less. In nerve tissue the maximum injury potential difference obtainable is of the order of 0.026 v., though its magnitude varies somewhat for different nerves and in different animals. The injured part of the tissue is negative relative to the uninjured part.

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[2] *Ibid.*, 692 f.

[3] L. Hermann, *Lehrbuch der Physiologie*, 13 ed., 1905, 119; *Handb. d. Physiol.* 2 (1), 1879, 144-154; P. Hoffman, *Handb. d. normalen u. pathol. Physiol.*, 8 (2), 1928, 703-708, 738-741; R. S. Lillie, *Protoplasmic Action and Nervous Action*, 1923, 299-310; W. J. V. Osterhout, Electrical phenomena in the living cell, *The Harvey Lectures*, 25, 1931, 169-181.

[4] Hoffman, *op. cit.*, 704 f.

These resting potential differences may be produced between two places on a tissue by differences in temperature,[5] or of pressure,[6] as well as by differences in injury.

Probably related to the injury currents are the action currents or potentials.[7] As a wave of excitation passes along a tissue, it is accompanied by a wave of decreased potential, so that the active place on the tissue becomes negative relative to other points. That there is some basic connection between the two phenomena is indicated by the fact that the action and the injury potential differences are of about the same magnitude. It is by means of these action potentials that so much valuable information about the functioning of nerves and muscles has been obtained, and it is the hope of obtaining similar information about the auditory nerve that has led to the various investigations to be considered in this paper.

MICROPHONIC PHENOMENA

The essential procedure in the various investigations of auditory action currents consists in exposing the auditory nerve in an appropriate experimental animal, in most investigations the cat, and in placing the active electrode on or near the auditory nerve and the other electrode on the nerve or, more generally, on nearby muscle or brain tissue. The animal is of course previously placed under an anaesthetic and usually decerebrated so that there will be no movements to interfere with subsequent experiments. The electrodes are connected to an amplifier either over a transformer or directly to grid and filament of the first tube, and any currents which may be led off in this way, when sound stimuli are sent into the cat's ear, are amplified and finally passed through a telephone or a galvanometer. In the investigations to be considered here, these currents were found to be of the frequency of the stimulating sound waves. So we shall refer to this phenomenon as the 'bio-microphonic effect' or simply as the 'microphonic effect.' A number of possible ways in which this microphonic effect may have been produced will be discussed in the present section. They will be referred to as 'microphonic phenomena.' Following is a list of the ones that we shall consider here: (*1*) Microphonic effects dependent on induction; (*2*) Microphonic effect of the tubes; (*3*) Vibration of electrodes by sound waves from the air to produce: (*a*) variation of the resistance in the input circuit, (*b*) variation of the capacity in the input circuit; (4) Vibration of the electrodes by sound waves transmitted from the labyrinth: (a) and (b) as under (3); (5) Fluctuating potentials (which produce a fluctuating potential difference between the electrodes): (a) generated in the tissues by the passage of sound waves, (b) generated in the labyrinth by the passage of sound waves, (c) generated in the auditory nerve by the passage of nerve impulses.

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[5] *Ibid.*, 708; cf. also W. Pauli and J. Matula, Der Thermostrom des Muskels, *Arch. f. d. ges. Physiol.*, 163, 1916, 355-383.

[6] Hoffmann, *op. cit.*, 708; cf. also J. de Meyer, Sur la dualité de la réaction électrique des systèmes musculaire, *C. r. de la Soc. de Biol.*, 83, 1920, 301; De Meyer, De la superposition des courants d'action et de déformation dans les muscles, *Arch. internat. de physiol.*, 16, 1921, 193-227; H. Lehmann, Über den Willkürversuch du Bois-Reymonds, *Arch. f. d. ges. Physiol.*, 207, 1925, 316-319.

[7] Hoffmann, *op. cit.*, 709-737; 741-758.

(1) *Induction.* An obvious artifact to be avoided in the investigations being examined here is that due to induction effects. Fluctuating electric or magnetic fields which vary synchronously with the stimulating sound waves may induce corresponding fluctuations of current or potential in the amplifier and so give rise to telephone tones of the same pitch as the stimuli. This danger is the greater, the more sensitive the amplifying system; for the fluctuating currents or potentials induced may then be very minute and yet be detected. Hence all of the experimenters who have reported upon the 'biomicrophonic effect' have avoided the use of electrical sources of sound stimuli and have relied upon air sources alone.

But the presence of fluctuating electric and magnetic fields of like frequency with the sound-stimulus is not limited to those that may be produced by electrical sources of sound. If anywhere in the region that may be reached by the stimulus-waves there is electrical apparatus which permits the sound waves to produce vibration of a conducting wire in a magnetic field, or the variation of the reluctance of part of a magnetic circuit (as by a vibrating membrane near the ends of telephone magnets), or the production of fluctuating currents synchronized with the sound waves, or the vibration of a charged body, then fluctuating electric and magnetic fields may be produced.

Amplifiers are quite generally provided with shielding to exclude disturbances from the surroundings. But even where present, such shielding designed for general purposes cannot be *assumed* to have been adequate to prevent fluctuating fields, such as those mentioned above, from inducing microphonic effects in the amplifier. Shielding is a relative matter. Whether it is adequate in a given case depends upon the strength and frequency of the fluctuating fields, the characteristics of the shielding system (nature of the material, its thickness and form, presence of slits and joints, all of which determine the ease with which eddy currents may flow), the degree of amplification, and the characteristics of the circuit which is being protected, as, for example, the input circuit of the amplifier.[8]

To avoid the effects of fluctuating fields produced in the ways mentioned above, it is of course desirable that all electric apparatus that might give rise to such fields be removed from the vicinity of the stimulus-room, and that appropriate shielding against fluctuating electric and magnetic fields be provided. But even with such precautions, the exclusion of induction effects cannot be considered assured until appropriate tests have indicated this to be true. Such tests are as necessary as is testing the soundproofing of the room containing the amplifier to assure exclusion of the microphonic effect of the tubes. Such tests, to be adequate, must also take account of the fact that the characteristics of the input circuit may determine the intensity of the effects that might be produced in it by induction. During the tests, therefore, the input circuit must have magnitudes of resistance, capacity and inductance comparable with those existing in the circuit during the experiments, when the animal tissues between the electrodes provide certain of these magnitudes.

(2) *Microphonic action of the tubes.* The vibration of the amplifier tubes by the sound waves may produce movements of the grid and filament relative to each other, altering the grid-filament capacity, and thus produce what is called the microphonic effect of the tubes. This phenomenon, which is relatively easy to detect, due to its favoring of certain frequencies and its independence of the characteristics of the input circuit, seems not to have been a factor in any of the experiments and so need not be further discussed.

(3) *Vibration of the electrode—sound waves from the air.* Microphonic phenomena due to the vibration of the electrode by air-waves (air vibration phenomena) may be of two kinds: (*a*) those dependent upon the variation of the resistance in the input circuit (resistance-microphone phenomena); and (*b*) those dependent upon the variation of the capacity of the input circuit (condenser-microphone phenomena).

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[8] J. H. Morecroft and A. Turner, The shielding of electric and magnetic fields, *Proc. Inst. Radio Engineers*, 13, 1925, 477-505. The significance of the characteristics of the circuit is indicated by the formulae for the flow of alternating currents in various kinds of circuit: see, *e.g.*, V. Karapetoff, *The Electric Circuit*, 2nd ed., 1912, 60-80.

(*a*) *Variation of resistance of input circuit.* The model for resistance-microphone phenomena is furnished by the carbon microphone.[9] Direct current from a battery flows through a resistance provided by carbon granules lying loosely behind the telephone transmitter membrane, then through the line to a telephone or transformer and back again to the battery. Vibration of the transmitter membrane by sound waves changes the contact among the carbon granules and thus the resistance of the circuit. Corresponding fluctuations in the direct current through the circuit results. For such a microphonic effect to be produced, it is apparent that there must be a source of direct current, a complete circuit around which the direct current may flow, and a means by which pressure variations of the sound waves may produce corresponding variations in the resistance of the circuit.

When these considerations are applied to the experiments being reviewed here, it is evident that such a 'resistance-microphone' phenomenon might occur in the experiments in which the electrodes are connected to the amplifier over a transformer. The source of direct current might be provided by an injury potential difference of the tissues in contact with the electrodes; the closed circuit by the series path from one electrode through the tissues to the other electrode, through the primary of the transformer and back again to the first electrode. The circuit would contain an inductance and capacity in series due to the inductance of the transformer primary and the capacity of the tissues. The tissues must be regarded here as a resistance and capacity in parallel, the continuous current passing by way of the resistance branch. The variations in resistance might be produced by the vibration of the active electrode by sound waves reaching it either directly from the air, or coming through the tissues from the cochlea or other parts of the labyrinth. The latter case we shall consider further on. In a microphonic circuit of the sort described, it is apparent that the amplitude of the current fluctuations produced by the vibrating electrode will depend on all of the electrical dimensions of the circuit: the resistance, the capacity, and induction of the total circuit, as well as the voltage of the steady electromotive force and the amplitude of the fluctuations in resistance.

(*b*) *Variation of capacity of input circuit.* In the investigations in which the electrodes are connected directly to the grid and filament and not over a transformer, a resistance-microphonic effect would not be possible, on account of the absence of one essential for such a phenomenon, a continuous circuit for direct current. But an analogous phenomenon, dependent on the variation of the capacity of the input circuit, would still be possible. The model for this phenomenon is furnished by the condenser microphone.[10] It depends on the principle that if a charged condenser connected to an external circuit is subjected to vibratory pressure changes producing corresponding variations in capacity (due to variation in the separation of the plates or in the dielectric constant of the medium between the plates), then an alternating electromotive force will be set up in the circuit. This will occur whether the external circuit consists of an inductance, resistance, or another capacity (as provided, for example, by the grid-filament capacity of a vacuum tube), though the magnitude of the alternating voltage across the terminals of the external circuit will depend upon its properties as well as upon those of the rest of the circuit. In terms of the conditions of the present experiments, the vibrating (active) electrode can be considered one plate of a condenser, and the tissues the other plate. The electromotive force for charging the 'condenser' is represented by the injury potential difference, as in the case above. Vibration of the electrode may produce fluctuations of the capacity and corresponding fluctuating currents through the transformer or a fluctuating potential difference between the grid and filament of the first tube, if no transformer is used. It is also possible to regard the condenser as provided by the entire stretch of tissue between the two electrodes. As pointed out earlier, the capacity of the tissues may be thought of as corresponding to the capacity of a group of condensers in series with each other, the occurrence of such a series being due to the successive membranes at which a piling up of ions occurs. So the fluctuations in capacity of the input circuit might be produced in the tissues themselves by the passage of the sound waves through them. In either case, the amplitude of the fluctuating electromotive force produced will depend on all of the electrical dimensions of the input circuit: the resistance, capacity, and inductance in the total circuit, the voltage of the injury potential difference and the amplitude of the capacity changes produced by vibration of the electrode or passage of the sound waves through the tissues.

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[9] A. B. Wood, *A Textbook of Sound*, 1930, 382-402; G. D. Shepardson, *Telephone Apparatus*, 1917, 89-99. For derivation of formulae expressing the dependence of microphonic currents on the characteristics of the circuit for commercial circuits, see Shepardson, *op. cit.*, 106-113.

[10] Wood, *op. cit.*, 395-398; I. B. Crandall, *Theory of Vibrating Systems and Sound*, 1926, 249 f.

(4) *Vibration of the electrode—sound waves from labyrinth.* The sound waves responsible for setting the electrode into vibration may, as mentioned above, come from two sources, either directly from the air or from the cochlea (or other parts of the labyrinth) by way of any tissues intervening between the walls of the labyrinth and the electrode. In many of the investigations, the active electrode rests directly against the wall of the labyrinth (*e.g.* against the medial surface of the petrous bone, when the electrode is around the auditory nerve). As is well known, the bones of the middle ear are adapted for transforming the sound waves striking the ear drum into vibrations of greater force, but correspondingly reduced displacement-amplitude. Some of the energy of the resultant vibrations in the liquids of the cochlea might be expected to be transmitted through the bony case of the cochlea, and from there further on through the tissues adjacent to the petrous bone. Bone is a good conductor of sound. The tissues are aqueous in nature, and water too is a good conductor of sound. Sound-conductance in the tissues would, of course, be less than in the case of the homogeneous liquid since, in the case of the tissues, the additional materials present in the form of colloids and suspensions would more quickly damp out the vibrations. But just how soon this would occur for a sound of given frequency

and energy could be determined only experimentally. Another possible path of transmission of vibrations from the cochlea that has been suggested is that by way of the auditory nerve out through the inner auditory meatus. This too is a possible path, although the path by way of the bone-walls of the cochlea (or other parts of the labyrinth) is probably of much lower acoustic impedance.

As for the transmission of sound waves in the brain tissues, it seems unlikely that this would occur with equal facility in all directions from the petrous bone. For such equality might be expected only in a homogeneous medium. The acoustic conductivity in the different directions must depend on the same factors, such as viscosity of the medium, reflection, refraction, diffraction, and interference, which determine the transmission of sound in general.[11] The marked differences in the texture and structure of the tissues in the various directions about the medial surface of the petrous bone is probably coupled, therefore, with corresponding differences in acoustic impedance. It is conceivable, for example, that the sound waves will be propagated relatively better along paths of greater homogeneity, such as along the length of nerve trunks and along the paths followed by blood vessels. At any rate, in the absence of experimental data on the distribution of acoustic impedance in the various parts of the brain, it is not possible to assume that differences do not exist. If sound vibrations are transmitted through the bony walls of the labyrinth and further along through the tissues, then such motions would be imparted to an electrode resting against the bone or further along in the tissues. Both of the microphonic phenomena discussed in the last section might then occur. The magnitude of the resultant microphonic effects would, however, depend here on two additional factors: the energy of the sound waves in the liquids of the labyrinth and the acoustic impedance of the path from these liquids to the electrode.

(6) *Fluctuating potentials: (a) Generated in the tissues by the passage of sound waves.* It is possible that the fluctuating potentials underlying the 'bio-microphonic effect' may have been produced in the tissues themselves by the passage of sound waves. In a previous section, we have mentioned the possibility that such a phenomenon may occur as a consequence of changes in the capacity of the tissues by sound waves. But we wish to suggest here the possibility of such

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[11] Wood, *op. cit.*, 229-335.

a phenomenon occurring on the basis of certain empirical evidence and independently of any hypothesis as to the mechanism underlying the phenomenon.

(1) Osterhout and Hill, as a result of experiments upon the sea-plant Nitella report that mechanical stimuli which travel down a cell are accompanied by a wave of decreased potential, similar to that which accompanies a wave of excitation. According to a summary statement of the authors: "Mechanical stimulation of Nitella often produces responses resembling propagated negative variations but traveling faster and going past a killed spot. They appear to result from a mechanical disturbance traveling along the cell and stimulating each spot it touches (*i.e.* the stimulus itself travels). They are called mechanical variations to distinguish them from propagated negative variations. . . . The all or none law does not apply to incomplete mechanical variations, for the response varies with the strength of the stimulus."[12]

(2) As mentioned earlier, De Meyer has reported the production of potential changes of considerable magnitude as a result of mechanical distortion of muscles.[13] When resting muscle is deformed from the outside in without injury, potential differences (as high as 1.5 mv.) occur between it and neighboring points. The deformation current appears without latency and is approximately proportional to the deformation. The greater the deformation, the more positive is the place.

(3) A recent report by Keller is also of interest in this connection.[14] Keller, in commenting upon the significance of the microphonic effect reported by Wever and Bray gave some reasons why he believed that it was not due to action currents. He said he had found similar phenomena in the vagus and depressor nerves which his observations indicated were not a result of true action currents. He found that when the experimental animal breathed noisily, the acoustic frequencies corresponding to the sounds would be reproduced in the curves of the oscillograph used for recording the action currents of the nerve. (Mechanical or acoustic transmission to the amplifier he considered to be excluded since the nerve hung loosely on the electrodes and the amplifier was in a soundproof chamber.) He found that the effect could also be obtained if a silk thread, soaked in Ringer's solution, was placed around the animal's trachea and the end of the thread placed on the electrodes, or if the string was tied to a monochord and the monochord struck. He suggested that the effect might be due to electrostatic displacements produced by the vibrations and that the arrangement might be designated as a thread-microphone. Such phenomena, he thought, were probably responsible for the microphonic effect that had been detected in the auditory nerve.

The results of these investigations indicate that mechanical changes in tissues may produce corresponding variations in electrical potential. It is possible, then, that in the investigations made of the 'bio-microphonic effect,' the variations in pressure involved in the transmission of sound waves through the tissues in the neighborhood of the active electrode may have given rise to corresponding fluctuations of electric potential, and so have led to a microphonic effect in the telephone.[15]

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[12] W. J. V. Osterhout and S. E. Hill, Electrical variations due to mechanical transmission of stimuli, *J. Gen. Physiol.*, 14, 1931, 485.

[13] See footnote 6.

[14] Ch. J. Keller, Über ein merkwürdiges Phänomen an Niederfrequenzverstärkern, *Z. f. Biol.*, 91, 1931, 346-348.

(*b*) *Generated in the labyrinth.* The fluctuating potentials responsible for the 'bio-microphonic effect' may have been produced in the cochlea or some other part of the labyrinth. Whatever the nature of the underlying mechanism, the intensity of the resultant microphonic effect would depend, presumably, upon the energy of the sound vibrations occuring in the liquids, the possibilities for changes in the concentration of ions, and the electrical impedance of the path between the place of origin of the fluctuating potentials and the active electrode.

(*c*) *Generated in the auditory nerve.* Fluctuating potentials generated in the auditory nerve is the phenomenon which it was the purpose of the various investigations to discover. If it can be demonstrated that the 'bio-microphonic effect' detected is in fact due to action potentials of the auditory nerve, then the fact that such an effect occurs is obviously of significance for auditory theory. A microphonic effect due to auditory action currents would depend presumably upon the energy of sound waves occuring in the cochlea, the functional condition of the auditory nerve, and the impedance of the electrical pathway between the active point on the nerve and the active electrode.

It has been apparent that every one of the various microphonic phenomena which we have examined has its own peculiar conditions. Furthermore, the technical and accidental conditions of the various investigations cannot be regarded as identical. There are, for example, differences in the degree of amplification used, differences in the way the electrodes are connected to the amplifier (and therefore differences in the characteristics of the input circuit), and, finally, differences in the electrical properties of the tissues involved. The last, *i.e.* the electrical properties of the tissues, cannot readily be controlled, and there is no indication in most of the investigations of any attempt to control or even to specify them. There is no reason, then, for expecting that the same microphonic phenomena will occur in the various investigations. Consequently every investigation must be examined as a unit in itself; and even though some particular phenomenon may have been excluded as a factor in one investigation (or experiment), it does not follow that it has been absent from another, unless this fact is specifically stated and demonstrated. It is also apparent that the microphonic effect which occurs at a given time cannot be assumed to be due to but a single one of the conceivable microphonic phenomena. It is possible in a given case that a number of microphonic phenomena may be involved and that the effect detected is a resultant of the component effects of many of them; furthermore, the relative share contributed by each of the phenomena involved to the resultant effect may shift and change as the conditions of the experiment change. With these considerations in mind, let us now examine the various investigations to see whether any of them provides conclusive evidence that the 'bio-microphonic effect' or some fraction of it is due to action currents of the auditory nerve.

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[15] Whether electrical potentials generated in or near the auditory nerve in this way would be capable of stimulating the nerve to conduct auditory-nerve impulses, that is to say, impulses that might mediate experiences of sound, is a separate question and need not be discussed here. The point involved here is that such fluctuating electrical potentials would not be action potentials in the customary sense of the term and so could not be used as indicators of the nature of the impulses in the auditory nerve.

Investigations of the Bio-Microphonic Effect

The investigations that we shall consider here are those of Wever and Bray,[16] Adrian,[17] Adrian, Bronk, and Phillips;[18] Wever;[19] Davis and Saul;[20] Hughson and Crowe;[21] Rademaker and Bergansius;[22] and Kreezer and Darge.[23] We shall take them up as nearly as possible in the order of their publication.

Wever and Bray. As a result of their experiments, Wever and Bray came to the conclusion that the microphonic effect detected by them was due to action currents in the auditory nerve. Their procedure for reaching this conclusion was by a process of elimination. Every one of a number of other possible microphonic phenomena are considered in turn and experiments performed to determine whether it could be considered responsible for the effect. Every one in turn is judged to be excluded by the results of the experiments, and a final experiment is then made to decide between the two remaining possibilities, resulting in the conclusion mentioned above. Let us now examine these experiments more closely.

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[16] E. G. Wever and C. W. Bray, The nature of acoustic response, *J. Exper. Psychol.*, 13, 1930, 373-387.

[17] E. D. Adrian, The microphonic action of the cochlea: an interpretation of Wever and Bray's experiments, *J. Physiol.*, 71, 1931, xxviii-xxix.

[18] E. D. Adrian, D. W. Bronk, and G. Phillips, The nervous origin of the Wever and Bray effect, *J. Physiol.*, 73, 1931, 2-3P.

[19] E. G. Wever, Impulses from the acoustic nerve of the guinea pig, rabbit, and rat, this Journal, 43, 1931, 457-462.

[20] H. Davis and L. J. Saul, Action currents in the auditory tracts of the midbrain of the cat, *Science*, 74, 1931, 205 f.

[21] W. Hughson and S. J. Crowe, Function of the round window, *J. Amer. Med. Assoc.*, 96, 1931, 2027-2028.

[22] G. G. J. Rademaker and F. L. Bergansius, Expériences sur la physiologie de l'ouie, *Arch. neerl. de physiol.*, 16, 1931, 346-349.

[23] G. Kreezer and H. Darge, Auditory action currents, *Science*, 75, 1932, 105.

(1) *Induction*. The authors state that "induction effects are of first importance in the use of a sensitive electrical system if the stimulus is electrical or magnetic in nature." They have therefore avoided the use of such stimulating apparatus and were content with the use of air-sources of sound. Tuning forks too were avoided since they found "that if they are placed before the animal's ear in the ordinary way the vibration of their magnetic fields—for they seem invariably magnetized—sets up induced currents in the lead-in wires." The authors state that tests show that tuning forks as well as audio-oscillators "can be employed if the animal and the electrode wires are carefully shielded." But they conclude "it has been simpler in these preliminary experiments to use wind sources of sound."[24]

It is apparent from the authors' account of the measures they adopted to avoid induction effects that they had in mind only those induction effects which might have been due to the production of fluctuating electromagnetic fields by the sources of stimuli, and that to avoid them they have limited themselves to wind sources of sound. As has been indicated earlier in this paper, and in a recent note,[25] fluctuating fields synchronized with the sound waves might have been generated in other ways than by the stimulus-sources. Wever and Bray, in a recent note, say merely that "it should be unnecessary to state" that such effects were not involved in their experiments.[26] We fail to see, however, that the absence of such induction effects can be assured except by the use of the precautions we have mentioned in a previous section, and which involve a final test of the effectiveness of any shielding that may be in use, with proper regard for the influence of the characteristics of the input circuit upon the results of such a test. There is no indication in any of Wever and Bray's reports that such measures were taken.

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[24] Wever and Bray, *op. cit., J. Exper. Psychol.*, 380 f.

[25] Kreezer and Darge, *op. cit.*, 105.

[26] E. G. Wever and C. W. Bray, Kreezer and Darge on auditory action currents, *Science*, 75: 1932, 267.

(2) *Microphonic effect of amplifier tubes.* The microphonic effect of amplifier tubes seems to have been effectively avoided by placing the amplifier in a soundproof room and the preparation in another room about 50 ft. distant.

(3) *Mechanical action of the electrodes.* In explaining what is meant by the phrase "the mechanical action of the electrodes," the authors say: "It is easy to imagine that the pressure variations of the air cause slight movements of the electrode wires in the tissues, and that these movements produce a change in resistance between the electrodes or a change of inter-electrode potential. If the electrodes are placed on dissimilar tissues an inter-electrode potential and hence a more or less steady direct current is doubtless present, and changes of the sort suggested would act on the amplifier so as to produce an audible signal corresponding to the stimulus."[27] A number of experiments were performed to test this possibility.[28] The authors found that the effect was eliminated: (1) when the electrodes were placed in other tissues, away from the acoustic region (with certain exceptions); (2) upon death of the animal; (3) as a result of the destruction of both cochleas; (4) upon restriction of the blood supply to the head; and (5) as a result of a simultaneous polarizing current sent through the tissues between the two electrodes (active electrode around the nerve and indifferent electrode on cerebral tissue). They considered this evidence "effectively to eliminate mechanical action at the electrodes, and to indicate the cochlear origin of the effects."

But in the case of no one of these checks is there any assurance that the conditions which produced loss of the microphonic effect did not also produce changes in the electrical properties of the tissues upon which the air-vibration phenomenon depends. For this microphonic phenomenon depends not only on whether sound waves from the air are available for striking against the electrodes, but also on the electrical properties of the tissues between the electrodes—the magnitude of the injury potential difference, and the capacity and resistance of the tissues. If changes occurred in these properties that would reduce *at all* the magnitude of the effect due to vibration of the electrodes, then the checks obviously would be valueless as demonstrations that air-vibration effects were not involved. In the case of all of these checks (except possibly that with the polarizing currents), a certain decrease in the total microphonic effect was to be expected from the elimination of the components dependent on energy derived from the cochlea; consequently if any additional decrease might be expected from changes in the factors underlying air-vibration phenomena, then the checks would lose any validity they might otherwise have had. Unfortunately, Wever and Bray present no evidence to show that such changes in the electrical properties of the tissues did not occur, an omission which deprives the checks of any claims to conclusiveness. Below is presented some evidence which indicates that the checks performed would, in fact, have been likely to alter the electrical properties of the tissues between the electrodes. To just what extent such changes occurred in the experiments of Wever and Bray, and the extent to which they determined their results could have been determined only under the specific conditions of their experiments.

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[27] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 381.
[28] *Ibid.*, 381-384.

In experiments of Kreezer and Darge,[29] marked differences were found between the electrical impedance of tissues in the neighborhood of the acoustic nerve and the impedance of tissues in other parts of the body such as were selected by Wever and Bray for their checks. The experimental object was a living cat which had been prepared for the experiment in the manner followed in the experiments of Wever and Bray; that is to say, the cat was placed under ether, the carotid arteries were tied, and the cat decerebrated. Measurements of impedance were made by means of Wheatstone bridge and telephone; an audio-oscillator served as a source of alternating current. To measure the impedance of the brain tissues in the neighborhood of the acoustic nerve, one electrode was placed besides the petrous bone, near the acoustic nerve, and the other electrode on the opposite side of the brain-stem, just caudal and dorsal to the ventral edge of the tentorium. Following are some illustrative measurements. At an alternating frequency of 450 cycles per sec., the impedance was 1340 ohms; at a frequency of about 800 cycles, impedance was 1310 ohms. Then one electrode was placed around the saphenous nerve in the leg, and the other under near-by skin tissue. At 450 cycles impedance was 2500 ohms, at 800 cycles it was 2450 ohms. With one electrode around the nerve, and the other in muscle tissue 1 cm. away, the impedance at

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[29] The experiments upon the electrical impedance of the tissues that are referred to in the present section were all performed in collaboration with Mr. Hans Darge and will soon be published. The impedance (Z) of the tissues measured here is a function both of the effective resistance and of the capacity of the tissues. The influence of particular changes in capacity and resistance of the tissues upon the total impedance of an input circuit containing a transformer would depend, of course, upon the magnitude of the self inductance of the primary of the transformer as well as upon the capacity and resistance of the tissues. Calculations of the impedance of the total circuit could be made only if the individual values of the capacity, resistance, and self-inductance were known. It will be recalled that impedance takes the place in alternating current formulae of resistance in Ohm's law for direct currents. That is to say, the impedance of a circuit to alternating currents equals the effective voltage of the alternating electromotive force divided by the effective value of the total current in the circuit.

450 cycles was 2120 ohms; at 800 cycles, 2030 ohms. With both electrodes in muscle at a distance apart of 1 cm., the impedance at 450 cycles was 2310 ohms; at 800 cycles, 2200 ohms. With one electrode in muscle and the other in fatty tissue 4 cm. away, the impedance at 450 cycles was 3400 ohms; at 800 cycles, 3250 ohms. In these experiments Kreezer and Darge did not expect to duplicate the exact positions which the electrodes had had in the checks; for that, the descriptions are not precise enough. They wished merely to see whether, when the electrodes are placed first in the acoustic region and then in other parts of the body such as were used for the checks, any marked differences in the impedance of the tissues would be found. That such differences were found is clear from the figures reported.

The experiments of Kreezer and Darge also showed that marked changes in the impedance of the brain occur upon the death of the cat. The experimental animal was prepared as in the experiments just described, and the electrodes in the brain were located as before—one near the auditory nerve and the other on the opposite side of the brain-stem near the tentorium. The following figures, obtained in one of the experiments, are indicative of the changes occurring with death. Measurements of the electrical impedance of the tissues were made at 5-min. intervals for a period of 2½ hr. The cat breathed regularly without the aid of artificial respiration since the centers in the medulla had not been injured. The rubber tube leading to the trachea was then clamped and the cat permitted to die. The impedance to a frequency of 450 cycles just before death was 1260 ohms, at about which level it had been during the previous period of measurements. After death, the impedance rose steadily: ½ hr. after the heart stopped beating it was 1660 ohms; 1 hr. later it was 1820 ohms. A parallel rise in the impedance occurred for a frequency of alternating current of 800 cycles, at a slightly lower level of impedance.

That failure of the circulation is accompanied by changes in the electrical properties of the tissues is indicated by the results which have just been cited upon changes in impedance after the heart stopped beating. The investigations of Galleotti[30] and Von Bud[31] on the changes in the resistance and capacity of tissues with the onset of death are also relevant here. Hermann reports too a decrease in the injury current of nerves with the onset of death (more rapid for warm-blooded animals), and with fall in temperature.[32] In general, the close correlation that has been found to exist between even slight changes in the functional condition of an animal and the electrical properties of the tissues indicates that changes in the blood supply to the tissues would be likely to alter their electrical properties.[33]

The changes in these properties which might be produced by cochlear destructions would probably be dependent on any disturbances that these destructions might produce in the blood supply to the tissues of the head. Such disturbance of the blood supply becomes not unlikely when we consider the situation of the experimental animal as a result of the technique that had been followed in making it ready for the experiments. The tying of the carotid arteries and decerebration would mean that the sole blood supply to the tissues of the head and, therefore, to those included in the input circuit, would now be by way of vessels supplied by the basilar artery and the two vertebral arteries which supply it. Destruction of the cochleas could scarcely avoid rupturing the vessels supplying them, the two auditory arteries which branch off from the basilar artery. Such a rupture would drain away blood coming through the basilar and vertebral arteries and consequently disturb the distribution of blood to the regions of the head supplied by branches of the basilar artery.

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[30] G. Galleotti, Über die elektrische Leitfähigkeit der tierischen Gewebe, *Zsch. f. Biol.*, 43, 1902, 301-316; 45, 1904, 65-78.

[31] G. von Bud, Permeabilität und Autolyse, *Zsch. f. Biol.*, 86, 1927, 108-114.

[32] L. Hermann, *Handbuch der Physiologie*, 2 (1), 1879. 148.

[33] G. W. Crile, H. R. Hosmer and A. F. Rowland, The electrical conductivity of animal tissues under normal and pathological conditions, *Amer. J. Physiol.*, 60, 1922, 59-106; W. J. V. Osterhout, *Injury, Recovery, and Death in Relation to Conductivity and Permeability*, 1922, 15-20; Gildermeister, *op. cit.*, 657-681; Lillie, *op. cit.*, 299-336.

Kreezer and Darge also performed experiments upon the effect of a simultaneous polarizing current on the impedance of the tissues to alternating currents. The previous arrangement used for measuring the impedance of the tissues was combined with a battery and a potentiometer to serve as a source of direct current. The two electrodes were placed in the brain-tissues as in the previous experiments described. Following are figures which represent typical results. In this list the first quantity, in volts, refers always to the voltage of the direct current supply; the second figure to the impedance of the tissues to an alternating current of 450 cycles sent simultaneously through the same tissues: 0 volts, 1360 ohms; 1 volt, 1360 ohms; 2 volts, 1420 ohms; 3 volts, 1420 ohms; 4 volts, 1600 ohms; 5 volts, 1880 ohms; 6 volts, 2070 ohms; 7 volts, 2180 ohms; 8 volts, 1930 ohms. For a setting of 0 volts made immediately after the previous measurements, the impedance was 1420 ohms. About an hour after the polarizing current had been discontinued the impedance had dropped back again to a relatively steady value of 1340 ohms. A curve of the same form was obtained for an alternating current of 800 cycles, at a slightly different level of impedance.[34] These results show clearly the marked change that is produced in the electrical properties of the tissues by a simultaneous polarizing current. In Wever and Bray's experiment it had been found that polarizing currents of from 4-8 volts were sufficient to eliminate the microphonic effect.

This account is enough to show that a constancy in the electrical properties of the tissues under the conditions of Wever and Bray's checks cannot be assumed. These checks, however, have failed to take account of the possible changes and cannot, therefore, be regarded as conclusive.

(4) *Mechanical action of the cochlea.* By the mechanical action of the cochlea Wever and Bray refer to the microphonic phenomena dependent upon vibration of the electrodes by sound waves which may be transmitted from the cochlea, rather than directly from the air. As expressed by the author, "it may be supposed that the cochlea, being especially sensitive to aerial pressure variations, picks

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[34] The direct current voltage between the electrodes in these experiments is about three fourths of the applied electromotive force of the total circuit, as drawn from the potentiometer, on account of the potential difference drop in the circuit before the animal. The figures given above for the voltage are for the total circuit.

up and transmits these variations to the substance of the nerve in a mechanical fashion, and the resulting movements of the nerve against the electrode vary the inter-electrode potential and thereby produce the response."[35] Against this possibility the authors argue "that nerve substance is a poor mechanical conductor" and that "this would be especially true of the auditory nerve, since it originates in small filaments which later unite into the larger trunk emerging from the internal meatus." But as we have already pointed out, the auditory nerve is not the only path for sound waves propagated from the cochlea, or indeed, the best conducting one. Another and probably better path is provided by the bony case enclosing the liquids of the cochlea. The authors add as a further argument that "the cessation of the response on the death of the animal and on the introduction of a polarizing current into the nerve argues against mechanical action of this kind, as it seems unlikely that such action should be seriously impaired by these changes." But as has been also noted above, such changes in the animal modify certain of the electrical properties of the tissues, which are also an essential factor in microphonic phenomena dependent upon "mechanical action of the cochlea."

Three additional experiments are cited as furnishing more positive evidence against the possibility that the microphonic effect is due to "mechanical action of the cochlea:" (*a*) the persistence of the effect when an attempt was made to eliminate the inter-electrode potential; (*b*) the fact that the response is fairly well localized in certain tracts of the nervous system; and (*c*) "the cotton and film" experiment.

(*a*) *Experiments to eliminate the inter-electrode potential.* Wever and Bray point out that the microphonic phenomenon under consideration would require the presence of an inter-electrode potential and they state that "under ordinary conditions, where the two electrodes are on dissimilar tissues, such a potential is the rule. An effort was made, therefore, to reduce or eliminate such a potential as a check against the artifact suggested. In certain experiments non-polarizable electrodes of the silver-chloride type were used, one on the nerve and the other in the cerebrum. The response continued as before. Two silver-chloride electrodes were then imbedded in an insulating cement at a distance of 2 mm., and both placed in contact with the nerve." "By this means," they continue, "we sought to avoid an inter-electrode potential by having the electrodes on similar and adjacent tissue, and by the embedding cement to avoid the possibility of alterations of the distance between the electrodes through any movements in their neighborhood. The responses continued as strongly as before, but perhaps with a certain amount of distortion. It occurred to us that such distortion might be the result of the reception of diphasic action currents (cat 10)."[36]

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[35] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 384 f.

As has been noted in an earlier section, the inter-electrode potential may depend not only on the polarization of the electrodes and their presence in different tissues, but on *differences in injury, pressure, or temperature* of two places on the *same tissue* in contact with the electrodes. The means adopted by Wever and Bray for eliminating the inter-electrode potential cannot, therefore, be regarded as adequate. Its elimination must be demonstrated, not assumed. Nor does the mere imbedding of the electrodes in insulating cement guarantee the elimination of relative movement between the tissues and the electrodes. But even if these measures had sufficed to eliminate the possibility of microphonic effects due to vibration of the electrodes, the results would not demonstrate that this phenomenon was not involved in *any* of the experiments (which, we take it, is the point of the checks). The persistence of the effect after the measures taken to eliminate the inter-electrode potential, etc. could, at most, mean that other causes for the microphonic effect exist, that microphonic effects dependent upon vibration of the electrodes are not the only possible ones. Of these other possibilities there are, as shown above, a considerable number. But it would not follow that the microphonic phenomena dependent on vibration of the electrodes could then be regarded as absent from all of the experiments. For if a given phenomenon should be eliminated in certain experiments, by elimination of some of its essential conditions, it obviously does not mean that this phenomenon would also be absent when the essential conditions are not eliminated.

(*b*) *Path of transmission.* Wever and Bray cite as additional evidence against "any hypothesis of mechanical transmission" the fact that the response is fairly well localized in certain tracts of the nervous system and that "the currents can be picked up across the medulla and from the brain stem, at distances which would seem too great for mechanical action to be effective. Under the same conditions, other tissues equally distant from the nerve do not give the effects."[37] These results are of significance as checks against "mechanical action of the cochlea" only if it is true that the distances *are* "too great for mechanical action to be effective," and if the transmission of sound waves is as good to places in the tissues where the effect is not picked up as to those where it is detected. Without proof of these statements, the results reported cannot be regarded as adequate checks.

(*c*) *The 'cotton and film' experiment.* This experiment is designated by the authors as one of the best checks they have and so it merits particular consideration. It was made possible by the exceptional cases in which the response was obtainable from tissues off the nervous system. "It was found that the active electrode placed on a lump of wet cotton resting on the temporal bone, and the inactive electrode on distant muscular tissue, picked up the responses readily. But if a piece of dry collodion film were interposed between the cotton and the bone the responses disappeared. Yet if a small strand of the cotton was picked up with the forceps and carried over the edge of the film so as to make contact with the moist bone or muscular tissue beneath, the response returned (cats 11, 8). The experiment yielded the same results if both electrodes were on the cotton

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[36] *Ibid.*, 385. [37] *Ibid.*, 385.

(cat 11). As the manipulation of the strand of cotton did not alter the mechanical features of the situation in any essential manner, we may conclude that the transmission concerned is electrical rather than mechanical in nature."[18]

Let us consider first the case in which the active electrode rests on wet cotton and the indifferent electrode on distant muscular tissue. The collodion film between the cotton and the tissues obviously breaks the circuit between the electrode and the tissues. The authors forget that this connection is as necessary for the microphonic phenomenon under discussion as it is for those dependent on electrical changes coming from the tissues or the cochlea. For how can variations in resistance, produced by vibration of the electrode, lead to microphonic fluctuations of current unless there is a closed circuit so that currents may flow? The provision of the moist cotton switch makes it possible, not only for electrical changes in the tissues to be detected by the electrode, but also for 'mechanical action of the cochlea' to become capable of producing a microphonic effect. To use this experiment to prove that the microphonic effect does not depend on vibration of the electrodes would be like trying to prove that the microphonic effect produced by a carbon or condenser microphone does not depend on mechanical vibrations since the effect disappears when the part of the circuit leading to the battery is broken.

When the electrodes are both on cotton, the situation is somewhat different. This variety of the check is like removing the battery from a carbon or condenser microphone circuit and connecting together the two leads (by a bit of moist cotton). Obviously, under such conditions, the microphonic effect produced by the sound vibrations will disappear. With both electrodes on the cotton there is no way by which injury currents can provide the inter-electrode potential which is necessary in order for 'mechanical action of the cochlea' to be effective. The effect which appears when the connection is made with the tissues beneath, by the moist cotton switch, might be due to any or all of a number of causes—electrical changes in the tissues produced by the passage of sound waves, electrical changes originating in the cochlea, or acoustic action currents. The occurrence of a microphonic effect under these conditions in no way proves, however, that the microphonic phenomena dependent upon vibration of the electrodes were not present in the other experiments and will not also appear when the conditions essential to them are provided—contact of the electrodes and the tissues.

(5) *Source of electric effects.* Assuming that all the microphonic phenomena previously considered have now been shown to be absent, the authors ask "whether the electrical effects shown to be correlated with the sound have their source in the sense organ or in the nerve." As conclusive evidence in favor of nerve transmission, the following experiment is reported: "The right cochlea was destroyed completely, and the impulses tested from both the left nerve and the brain stem. The left auditory nerve was then severed. The responses were no longer obtainable from the brain stem. They were still obtainable from the stump of the left nerve, however, and con-

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[18] *Ibid.*, 385 f.

tinued to be so for some time. Just after severing the nerve the responses from the stump were of normal intensity, but the intensity died away during the next few minutes, and finally the signals ceased. Care was taken in the transection of the nerve to leave the tissues in contact, and a quantity of salt solution was introduced before and after the transection; it seems reasonable to say that the electrical conductivity of the tissues was not appreciably altered by severing the nerve. As the responses were completely eliminated, our conclusion is that these responses are due to nerve conduction (cat 11)."[39] The logic of the authors seems to be then that the transection of the nerve, especially since a quantity of salt solution was introduced, should not have produced any change in the electrical conductivity to tissues beyond the cut, while it would stop the propagation of nerve impulses beyond this point. That the effect was eliminated on the far side of the cut is taken to indicate that the effect could not have been due to electrical spread from the cochlea; that it was still found in the stump of the nerve is regarded as in agreement with what would be expected if the effect were due to action currents.

This argument is faulty for two reasons: first, because the electrical conductivity from the cochlea to the brain stem may not be considered as good after the section of the nerve as before; and secondly, because the electrical changes detected at the stump of the nerve could not have been due to action currents generated at that point. There are a number of reasons why it may not be assumed that the electrical conductivity from the cochlea to the brain stem was as good after the nerve was severed as before. Kato, in another connection, has shown that even crushing a nerve decreases its electrical conductivity.[40] Moreover, the nerve could scarcely have been severed without severing the auditory artery that runs along with it, as has been suggested too by Adrian. Loss of blood upon collapse of the vessel might be expected to alter the conductivity of the entire 'cable' composed of the auditory and facial nerves and auditory artery. Results from the therapeutic administration of electric currents to the body indicate that the currents, to a large extent, follow the paths laid out by the blood vessels. Therefore, cutting the blood vessel would very materially alter the conductivity along the path traced by the vessel. Moreover, the injection of salt solution might be expected to hinder the transmission of electrical changes to tissues beyond the place of the cut, rather than to aid it. For a given quantity of electricity that was spreading out from the cochlea to regions of lower potential would be dissipated throughout the mass of the liquid injected. The amount of electricity that would reach the stem beyond the cut would be so much the smaller on account of much of the charge having been thus dissipated. For these reasons, it is more probable that the electrical conductivity between the cochlea and the brain stem was decreased by cutting the nerve than that it was not.

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[39] *Ibid.*, 386 f.

[40] G. Kato, *The Theory of Decrementless Conduction in Narcotized Region of Nerve*, 1924, 75.

That the effect detected at the stump of the nerve was due to action currents is improbable. If the nerve had been cut it would be showing a steady injury potential at the stump. Action potentials could not be detected at that point since action potentials disappear when the nerve impulse reaches a region of injury, or rather, the nerve impulse as well as the action potentials are extinguished on reaching a region of injury. It is this very fact that is made use of in physiological technique for obtaining monophasic action currents instead of diphasic ones, the fact, namely, that an electrode resting upon an injured place is not affected by the action potentials of nerve impulses being transmitted toward that point. Consequently, any electrical changes picked up by the electrode at the stump of the nerve must have been conducted to it by the tissues at the stump acting as an inert conductor of electrical potentials generated elsewhere.

The results of this experiment may, however, be readily accounted for without the difficulties involved in the 'action current hypothesis' given by Wever and Bray. Cutting the left nerve probably also involved the rupture of the artery running along with it. This may have had as effects: a decreased conductivity of the tissues from the cochlea to the brain stem; a decrease in the 'strength' of functioning of the cochlea, due to interference with its blood supply (Adrian reported that a disturbance of the blood supply led to a decrease in the microphonic effects obtained from the cochlea); and finally a change in the electrical properties of the tissues about the electrodes (interelectrode potential, and the capacity and resistance determining the impedance of the input circuit). The decrease in the 'strength' of functioning of the cochlea would lead in turn to a decrease in the amplitude of the electrical changes produced there, as well as to a decrease in the energy of sound vibrations in the cochlear liquids, and therefore of the vibrations propagated from these liquids to the tissues outside. All these changes may have contributed to the loss of the microphonic effect on the stem. The fact that the effect was still obtainable from the stump might be accounted for by the facts that: (1) the electrode, being pressed up against the stump, and the petrous bone, would be in a better position to respond to any sound vibrations that were being propagated through the bony wall of the cochlea than when resting on the tissues of the brain stem; (2) the inter-electrode potential might be greater now, on account of one electrode resting on the freshly cut nerve; and (3) there would be a better electrical conducting path between the electrode and the source of electrical changes in the cochlea.

This last experiment completes the checks performed by Wever and Bray to demonstrate that microphonic phenomena other than action currents of the auditory nerve were not responsible for the bio-microphonic effect. Except for the measures taken to avoid the microphonic effect of the tubes, it is apparent, however, that the experiments performed have not been adequate to show that the other phenomena considered were not factors in the effect.

Adrian. The procedure followed by Adrian to determine whether the microphonic effect was due to action currents in the auditory nerve was somewhat different from that of Wever and Bray, who, as we have seen, chose as their point of attack certain of the phenomena which may have been responsible for the effect other than action currents. Adrian concerned himself, rather, with the influence upon the effect of a number of conditions that might be expected to eliminate or seriously interfere with action currents. As a result of these experiments, he concluded that "it is almost certain that the electrical changes are generated in the cochlea and are not due to nerve impulses. The chief reasons for this view are that the effects are obtained with electrodes so far from the nerve that the usual action potentials would have little chance of reaching them, and that they are unchanged by conditions which would seriously interfere with nerve conduction."[41] He states further that "the effect led off from the nerve, the medulla, or the cerebellum can scarcely be due to impulses in the nerve fibers under the electrodes since it persists unchanged after the bare nerve has been covered with novocaine crystals or with lumps of ice or injected with 5% acetic acid. It is not then due to nervous conduction from the opposite cochlea, for this may be destroyed. Partial crushing of the nerve makes little or no difference. Crushing the entire nerve or cutting it through reduces or abolishes the effect, but the arteries to the inner ear run with the nerve and I have found it impossible to avoid damaging them as well. As Wever and Bray have shown, interference with the blood supply usually abolishes the effect, possibly owing to pressure changes in the inner ear." Adrian points out that the detection of the electric changes by an electrode on the nerve is understandable since the internal meatus is a hole "which will provide a low resistance pathway into the inner ear and an electrode on the nerve will tap this pathway." He considers it unlikely that the electric changes are generated by nervous elements in the cochlea since the reproduction of sound may persist unaltered after cooling the petrous bone with ice. He finds that "oscillograph records reproduce the wave form of the sound and the curves show no sign of being built up of nerve impulses." Adrian's stand is then that the electrical changes detected by him were probably generated in the cochlea, that it is improbable that these were produced by nerve elements in the cochlea, and that the effect which was detected by an electrode on the nerve was merely due to the nerve conducting the electrical changes as would an inert conductor, and was not produced by action currents of the nerve.

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[41] Adrian, *op. cit.*, xxviii-xxix.

Adrian, Bronk, and Phillips. The experiments of Adrian, Bronk, and Phillips were performed to determine to what is due the electrical changes that can be detected in the cochlea.[42] As a result of the experiments, they state that the objections previously raised by Adrian to Wever and Bray's explanation of the microphonic effect turn out to be not very serious, and conclude that the electrical changes in the cochlea are due to nervous structures of some sort. They add that the situation of the nerve fibers in the cochlea may favor the development of large potentials and that the intra-cranial part of the nerve may appear to act as an inert conductor because it is unfavorably placed for electrical recording and more liable to damage than the intra-cochlear part. It should be noted, however, that the experiments of Adrian, Bronk, and Phillips do not change at all the validity of the results of Adrian's previous experiments which tend to show that the electrical changes led off from *the nerve* were not due to impulses in the nerve fibers under the electrode. For the present investigation is not upon this point; it is concerned only with the conditions of the effect which can be picked up by an electrode on the wall of the cochlea.[43]

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[42] Adrian, Bronk, and Phillips, *op. cit.*, 2P-3P.

We may now examine the experiments which led these authors to the conclusion that the electrical changes picked up from the cochlea were due to nerve elements of some sort in the cochlea. To support this conclusion, three lines of evidence are presented: (*a*) final disappearance of the microphonic effect after death of the animal or after failure of the blood supply to the inner ear (after 1½ hr. for the guinea pig and about ½ hr. for the cat); (*b*) failure of the effect as a result of injections of cocaine solution through the round window or the wall of the guinea pig's cochlea; and (*c*) weakening of the effect upon cooling the cochlea of the guinea pig with ice. The authors conclude that "these results all indicate that the Wever and Bray effect is due to living cells and cease with their death, and the action of cocaine makes it likely that nerve endings, nerve cells or nerve fibers are responsible."[44]

Let us consider first the evidence that the microphonic effect is due to living cells. In order for the evidence to be conclusive there must be first a precise correlation between the changes that the experiments would be known to produce in living cells and the changes occurring in the microphonic effect; and secondly, the changes produced in the effect should be explainable in no other way.

(*a*) *Death or failure of blood supply.* In the experiments upon the effect of death or failure of the blood supply to the inner ear, there was found to be an initial fall in intensity when the circulation failed, then a continuation of the effect on about the same intensive level for from 1-1½ hr., and finally a complete disappearance of the effect. The authors attribute the initial fall in intensity to a mechanical cause such as the collapse of vessels, and not to a failure of the oxygen supply. The fact that the effect finally disappears is taken to indicate that the electrical changes are due to the activity of living cells. The necessary positive correlation of changes in the effect and in living cells is then not very complete. For there is no evidence that living cells did or would be likely to cease functioning at the particular time that the microphonic effect failed. All that can be said is that this may have occurred. Furthermore, the final failure of the effect may have been due to other causes, *e.g.* the final drying up of the liquids beyond a certain necessary level.

(*b*) *Cocaine injections.* A rapid failure of the effect was produced by injection of 10–20% cocaine solution into the round window or through the walls of the cochlea. "The injection of saline," the authors state, "usually leaves the effect unimpaired if no great damage is done," and add that "although the variable amount of damage confuses the issue the effect of cocaine seems incontestable."[45] It is difficult to regard this experiment as conclusively demonstrating the influence of cocaine, in view of the lack of precise knowledge of the damage done inside the cochlea by the various injections. That the cocaine was the responsible agent could hardly be considered conclusive without statistical data. But even though the cocaine should be regarded as a contributing factor in the failure of the effect, it need not be concluded that living cells were therefore involved. Such a conclusion would be justified only if the influence of cocaine could be regarded as limited to living cells. A significant property of cocaine, as well as of other narcotics, is that it is a non-electrolyte; and its influence upon living cells probably depends in large measure upon this property (in conjunction with other essential properties).[46] But the production of electrical fluctuations in the cochlea and their conduction to the electrodes must depend in some way upon the presence of electrolytes, upon the possibility of changes in the concentration of ions. Injection of a non-electrolyte would naturally produce changes in the concentration and state of equilibrium of any electrolytes that might be present in the cochlear liquids. There seems no need for assuming that these changes must take place inside of living cells.

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[43] In this experiment, the electrical changes from the cochlea were picked up by a moist thread electrode placed on the ventral surface of the petrous bone and it was therefore unnecessary to open the cranium, as when the auditory nerve is exposed.

[44] Adrian, Bronk, and Phillips, *op. cit.*, 3P.

(c) *Cooling the cochlea.* The weakening of the effect upon cooling the cochlea does not necessarily indicate the participation of living cells. Temperature is a noteworthy condition of many physical and physico-chemical processes not involving life, including many of the phenomena involved in the generation of electromotive force in electrolytes.[47] The effect of temperature upon such processes might have been responsible for the weakening of the microphonic effect obtained from the cochlea.

The evidence that the living cells, if they are actually involved, are *nerve cells* is still less decisive. The only evidence presented is the result of the experiments with cocaine, the inconclusiveness of which has already been indicated. Besides, the action of narcotics is not limited to nerve cells; they interfere as well with the functioning of other kinds of living cells.[48]

As for the significance of the conclusion that nerve elements of some sort are involved in the production of the electrical changes in the cochlea, even though this statement had been conclusively established, it would not follow that such electrical effects are likewise generated along the extent of the auditory nerve. The electrical changes in the cochlea might have arisen at the surfaces of contact between the nerve-endings and the liquids of the cochlea. It is well known that the surfaces of contact between two different media have properties very different from the main body of either medium alone.[49] Therefore, even if electrical changes of the frequency of the stimuli should be generated at the junctions of the nerve-endings and the cochlear liquids, it could not be assumed that like changes would occur throughout the extent of the auditory nerve, as action currents of impulses in this nerve. (Though, of course, one should expect such electrical changes in the cochlea to be conducted along the nerve in its rôle as an inert conductor.) Finally, it may be pointed out that the experiments of Adrian, Bronk, and Phillips do not demonstrate that the electrical effects detected were actually generated within the cochlea itself. They may also have been generated at the electrode by the sound waves transmitted through the walls of the cochlea.

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[45] *Ibid.*, 3P.

[46] See discussion on theory of counter forces and capacity in Gildermeister, *op. cit.*, 690-693; 688.

[47] F. H. Getman, *Outlines of Theoretical Chemistry*, 1926, 4th ed., 527-618.

[48] W. M. Bayliss, *Principles of General Physiology*, 1924, 4th ed., 140 f.

Wever. In this study, Wever reports the detection of the 'bio-microphonic effect' in the guinea pig, rabbit, and rat.[50] He reports one new check, which was designed to test the hypothesis that the bio-microphonic effect "might be due not to nerve impulses but to a condenser action of the drum and associated ossicles."[51] The check was performed on one guinea pig and on one cat. It consisted in the destruction of the right cochlea and of the left vestibule, care being taken to avoid modification of the ear drum and the ossicles. Upon the completion of these operations the microphonic effect failed to occur. This check, though it may exclude the hypothesis it was designed to test as a possible explanation of the microphonic effect, obviously does not exclude as possible causes any of the microphonic phenomena dependent on the occurrence of sound waves in the cochlea or other parts of the labyrinth, since they would all be eliminated by the operations performed.

Davis and Saul. The experiments of Davis and Saul involved certain differences in experimental technique from that of their predecessors.[52] A stethoscope was fitted to a decerebrate cat. Sound stimuli could be sent to one ear or the other by clamping the appropriate arm of the stethoscope. The stimuli consisted of taps delivered to the bell of the stethoscope and of other sounds delivered through a funnel in place of the stethoscope bell. The electrodes consisted of a diffuse silver electrode placed subcutaneously on the muscles of the neck and a fine silver needle electrode, insulated except at the tip, which was placed on the tissues where the electrical response was to be tested. The recording instrument consisted of one stage of amplification with string galvanometer or of more stages (number not stated) with telephone. The investigators were especially interested in determining the nature of the responses that could be detected in the brain stem, by pushing the needle electrode caudally down the stem, at right angles to the plane of decerebration.

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[49] *Ibid.*, 71.
[50] Wever *op. cit.*, this JOURNAL, 457-462.
[51] *Ibid.*, 459.
[52] Davis and Saul, *op. cit.*, 205 f.

The authors concluded from their results that the microphonic effects they obtained in response to acoustic stimuli were due to true action currents in the auditory pathways. These results were as follows: (*a*) the responses were much greater in the auditory pathways than in surrounding tissue; (*b*) there was immediate cessation of the response upon the death of the animal; (*c*) there was a decrease in the size of the response upon the local application of narcotics. The authors believe that the brain stem effect involved two factors: first, action currents in the particular auditory tract to which the electrode is applied; and secondly, electrical spread from distant parts of the auditory mechanism.

Let us consider the experimental evidence. Here too, in order for the results to be regarded as showing conclusively that auditory action currents were involved in the microphonic effect, it is necessary first, that there be a precise correlation between the changes in the effect due to a given condition, and the probable change in the functioning of the auditory nerve under these conditions; and secondly, that the changes in the effect be explainable in no other way.

(*a*) *Limitation of the maximum effect to the acoustic pathways.* Responses in the brain stem were found to be sharply localized. "Using the string galvanometer with one stage of amplification, responses were obtained," Davis and Saul write, "only from certain points. A movement of the electrode of 1 mm. or less suffices to pass through a maximum and usually to lose the response. With more sensitive amplifier and ear phones, this point represents a very sharp maximum many times louder than the responses from neighboring points. These active points have been shown by subsequent gross section to lie invariably upon the auditory pathways. . . . When electrodes are placed upon crossed tracts, such as the lateral lemniscus, the responses are chiefly contralateral, *i.e.* the electrode in the left midbrain is much more sensitive to stimuli applied to the right ear than to those applied to the left ear."[63] These results agree very closely with what would be expected if the microphonic effect were due to auditory action currents, and to that extent support the action-current hypothesis. They do not, however, exclude all other possible hypotheses. There is no certainty that sound waves and electrical changes propagated from the cochlea would not also find a better conducting path along the acoustic pathways. As for electrical conductivity, it was early found by Hermann that the conductivity of nerves was five times better in a lengthwise direction than in a crosswise direction.[54] That the acoustic pathways may represent pathways of higher electrical conductivity thus has some experimental support. We have mentioned above some of the reasons for supposing that the sound waves propagated from the cochlea might follow with less opposition the pathway laid out by the nerve. It is conceivable, therefore, that the microphonic effects obtained by Davis and Saul in these experiments may have been due to vibration of the active electrode produced by sound waves, potential changes generated in the tissues by the passage of sound waves, and electrical spread along the nerve from the cochlea, as well as to action currents in the acoustic pathways. Before the latter hypothesis can be regarded as necessarily true, the others must first be excluded as possibilities.

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[63] *Ibid.*, 205.

(*b*) *Death of the animal.* "When the cat dies in the course of an experiment, the responses from the brain stem are lost," Davis and Saul write, "as soon as the heart stops beating, and sometimes a few minutes before. The responses from the acoustic nerve persist after those from the brain stem are no longer detectable, and sometimes for several minutes after the heart has stopped beating."[55] This result is not in complete agreement with that found by Adrian, Bronk, and Phillips for the microphonic effects obtained from the cochlea. They found that there was an initial decrease in the intensity of the effect after death, but that the effect continued at a constant level of intensity for from 1–1½ hr. longer in the guinea pig and about ½ hr. in the cat, before finally disappearing.[56] Wever and Bray found that the effect picked up by an electrode on the nerve lasted about 20 min. after the death of the cat. Adrian, Bronk, and Phillips interpreted the initial decrease as due to a collapse of vessels of some sort, and the final disappearance of the effect as due to the death of nerve cells or fibers, which they believed were responsible for the microphonic effect.[57] If Davis and Saul wish to use the result they found as an indication that the effect was due to acoustic action currents, they must assume that loss of function of the acoustic pathways occurred immediately with death, with the failure of the circulation. If might possibly be claimed, in order to reconcile the two sets of results, that nerve cells in the brain and the nerve fibers leaving the inner auditory meatus cease functioning immediately after death (in accordance with the results of Davis and Saul) and that the nerve elements in the cochlea continue functioning for about ½ hr. after death (in accordance with the interpretation of Adrian, Bronk, and Phillips). But however one may seek to reconcile these results, it is apparent that the changes in the microphonic effect with death cannot be considered as supplying evidence that the effect is due to auditory action currents until there is more precise knowledge of just what is the effect of death upon the functioning of the auditory nerve and pathways, and how soon after death functioning there ceases. Without this information, a correlation of nerve functioning and the microphonic effect cannot be alleged. Furthermore, even on the hypothesis that the microphonic effect was due to some one or a number of the microphonic phenomena *not* involving action currents, a decrease or elimination of the effect with death might be expected. For as noted above, death might lead to an alteration in a number of the factors on which these other microphonic phenomena depend: *e.g.* decreased 'strength' of functioning of the cochlea with consequent decrease in the magnitude of the mechanical and electrical fluctuations propagated from the cochlea; increase in electrical impedance of the tissues between the active electrode and the cochlea; and changes in the injury potential difference, capacity, and resistance of the tissues.

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[54] Hermann, *Lehrbuch der Physiologie*, 239.
[55] Davis and Saul, *op. cit.*, 205.
[56] Adrian, Bronk, and Phillips, *op. cit.*, 2P.
[57] *Ibid.*, 3P.

(*c*) *Effect of narcotics.* "Narcotization of parts of the brain stem by injecting 5 or 10% novocaine in Ringer's solution near the electrode obliterates responses on the string. The responses return after 1–3 hr. If the novocaine is injected on one side of the brain stem between the electrode and acoustic nerve, responses from that ear disappear, while those from the other ear persist."[58] These results too disagree with those found by Adrian and by Wever and Bray.[59] These investigators found that the microphonic effect obtained with an electrode on the auditory nerve at its exit from the inner auditory meatus persisted despite the application of narcotics to the nerve. Such differences in results may be due to differences in the technical conditions of the various experiments; but for the results with narcotics to have a clean-cut significance such disagreements must be reconciled. But in any case, the elimination of the microphonic effect by cocaine would not compel one to attribute the effect to neural action currents. Injection of cocaine in the tissues near the active electrode might be expected to alter certain essential electrical properties of the tissues in this region.[60] As a result of changes in the capacity and resistance of the tissues, in the inter-electrode potential, and in the electrical impedance of tissues between cochlea and electrode, there might have occurred a loss of microphonic effects due to vibration of the electrode, to the propagation of electrical changes from the cochlea, or to the generation of electrical changes in the tissues by the passage of sound waves. Further, where cocaine is injected between the electrode and the auditory nerve of one side, the microphonic phenomena that might be expected to be altered would depend upon the extent to which diffusion of the narcotic could affect the tissues surrounding the active electrode. If the narcotic diffused so far, the microphonic phenomena which might have been eliminated by the narcotic would be the same as those itemized above; the persistence of the effect from the opposite ear might indicate merely that the average magnitude of the changes in the tissues on the side where the narcotic was injected were greater than on the other side—that here the changes in the electrical properties of the tissues were not great enough to eliminate the effect. So the results with cocaine cannot be considered conclusive until the disagreements of the different investigators have been reconciled and the exact influence which the cocaine had on the properties of the tissues essential to the other microphonic phenomena is known.

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[58] Davis and Saul, *op. cit.*, 205.

[59] Adrian, *op. cit.*, xxix; Wever and Bray, Action currents in the auditory nerve in response to acoustic stimulation, *Proc. Nat. Acad. Sciences*, 16, 1930, 348.

[60] Gildermeister, *op. cit.*, 678, 688; W. J. V. Osterhout, *Injury, Recovery and Death, in relation to Conductivity and Permeability*, 1922, 184-194; Crile, Hosmer, and Rowland, *op. cit.*, 98; G. W. Crile, H. F. Rowland, and G. H. Crile, The electrical capacity of animal tissues under normal and pathological conditions, *Amer. J. Physiol.*, 76, 1926, 323.

Hughson and Crowe. The purpose of this investigation was not to determine the nature of the phenomena responsible for the 'bio-microphonic effect,' but rather to use this effect as an indicator of the influence of various parts of the middle ear on the transmission of sound vibrations to the cochlear liquids.[61] The extent to which this procedure seems justifiable will later be examined; but now we wish to determine whether the results of these authors provide proof of the neural origin of the effect. The experimental procedure consisted in placing the active electrode on or near the auditory nerve (of the cat) and the indifferent, grounded electrode in the muscles of the neck. Wires connected the electrodes to a 6 tube amplifier (with telephone or loud speaker) in another part of the building. The experimental operations performed were of three kinds: (a) experimental lesions that produced *no appreciable change* in the effect—such as puncture of the ear drum, and filling the niche of the round window with wax or plaster; (b) lesions that produce a *decrease* in the effect—such as fixation of the ossicular chain, division of one of the ossicular joints, interference with the movements of the malleus, and puncture of the membrane of the round window (puncture of the round window membrane produced a great decrease in the intensity of the effect, the operations upon the ossicular chain affected especially the lower frequencies); (c) experimental lesions that produce an *increase* in the intensity of the effect, "increase the volume and clarity of the spoken voice and pure tones,"—such as an increase in pressure on the round window membrane.

(*a*) *No change.* The experiments where no change was produced in the microphonic effect obviously supply no evidence supporting the action current hypothesis. If anything, they furnish contrary evidence. Whatever reasons there are for expecting puncture of the drum or blocking of the round window to decrease sensitivity to sound would also lead one to expect a corresponding change in neural functioning and, therefore, in the microphonic effect, if it were due to action currents. The results might be interpreted, however, as showing merely that the microphonic effect is not sensitive enough to indicate changes in the neural response of the magnitude that would be produced by these lesions.

(*b*) *Decrease.* Consider now the experiments where there was a decrease in the effect. Various operations upon the ossicles led to a decrease in the microphonic effect. Consequently, there must have been at least a fraction of the original effect which was dependent upon proper functioning of the ossicular chain. This decrease in the effect might very readily be explained by the action current hypothesis. For such operations upon the bone chain might be expected to lead to a decrease in the energy of sound vibrations transmitted to the cochlea, a decreased intensity of stimulation of the nerve endings in the cochlea, and therefore decreased intensity of auditory nerve response and of the accompanying action currents. A decrease in the energy of the sound waves in the cochlea would, however, also decrease the energy of the vibrations propagated through the cochlear walls to the electrode and so might be expected to produce a decrease in the microphonic effects dependent on such transmission of sound vibrations. Furthermore, if the magnitude of the *electrical changes* generated in the cochlea depended on the energy of the sound waves in the cochlea, they too would be decreased or eliminated when the sound waves transmitted to the cochlea decreased in energy. It should be noted that the effect was not eliminated, but merely decreased in intensity by operations upon the ossicular chain. Which of the various microphonic phenomena contributed to this residual effect there is no way of deciding from the experiments. Any of the phenomena may have been involved, those not dependent upon the occurrence of vibrations in the cochlea as well as those dependent upon this source. The decrease in the microphonic effect produced by puncture of the round window membrane has the same significance as the decrease produced by the various operations on the ossicles. Such a puncture might be expected to lead to a decrease or disappearance of the microphonic effect due to *any* of the phenomena dependent on the energy of the sound waves occurring in the cochlea. Therefore the decrease that was found may have been due to a change in any or all of these microphonic phenomena, and there is no special evidence in favor of the action current hypothesis. . The residual effect here, as in the case of that remaining after operations on the ossicular chain, may have been due to all or any of the various microphonic phenomena.

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[61] Hughson and Crowe, *op. cit.*, 2027-2028.

(*c*) *Increase.* Let us consider finally the experiments in which an *increase* in the intensity of the microphonic effect was produced by an increase in the pressure on the round window membrane. There is no way of deciding the question: which of the various conceivable microphonic phenomena contributed to the effect before the increase in pressure? All or any of them may have been involved. The only part of the total effect that permits more specific conclusions is that representing the increase, the increment in the effect. This increase in the effect indicates that the increased pressure made more favorable some factor upon which at least a fraction of the total effect now depends. The question at issue here is whether the increment in the microphonic effect would be expected if the effect were due to action currents of the auditory nerve. That is to say, would pressure on the round window membrane be expected to lead to an increased magnitude of response of the auditory nerve? As Hughson and Crowe state, it has generally been believed that an increase in the intra-labyrinth pressure *interferes* with normal hearing. One should expect then that the increased pressure would be correlated with a decrease in the intensity of the neural response, and consequently, a decrease in the intensity of the microphonic effect if it were due to action currents of the nerve. The opposite result was found; this result, then, not only does not support the action current hypothesis; it is in opposition to it.

Hughson and Crowe suggest, despite the indications of other lines of evidence, that the increased pressure on the round window membrane may effect an increase in the "transmission of sounds" through the ear. If such an increase occurs, it might be expected, however, to lead to an increase in the intensity of the effect due to all or any of the microphonic phenomena dependent upon the occurrence of sound vibrations in the cochlea. So even this supposition does not lend any advantage to the hypothesis that the effect is due to action currents in the auditory nerve. We may conclude then that the experiments of Hughson and Crowe fail, as do the other investigations, to furnish the conclusive evidence in favor of the action current hypothesis which we have been seeking.

A few comments may now be added upon the attempt of Hughson and Crowe to use the *bio-microphonic effect* as an indicator of the functions of various parts of the middle ear in the transmission of sound to the inner ear. It is apparent that before the effect can be accepted as a reliable indicator of such functions, it must first be shown that there is in fact a precise correlation between the energy of the sounds transmitted to the cochlea and the magnitude of the microphonic effect. This correlation has not yet been demonstrated; consequently any conclusions upon the functions of various mechanisms in the ear, made on the basis of the microphonic effect, must be regarded as tentative only. One may, however, examine the experiments of Hughson and Crowe to see whether these supply evidence that the correlation mentioned does exist. Upon examination, the results turn out to be contradictory in nature. Puncture of the ear drum might be expected to decrease somewhat the energy of the sound vibrations transmitted to the inner ear, but no change occurs in the microphonic effect. Various operations on the ossicular chain, which might be expected to decrease the energy of the vibrations transmitted, do in fact lead to a decrease in the microphonic effect. A similar agreement occurs in the case of experiments involving the puncture of the round window membrane. Blocking the niche of the round window or increased pressure upon the membrane, either of which procedures might be expected to lead to a decrease in the energy of the sound vibrations in the cochlea, leads to no such corresponding changes in the microphonic effect. Blocking leads to no change at all, and increased pressure on the membrane leads to an increase in the effect. It is clear then that the results of this investigation as a whole may hardly be considered as furnishing support for the thesis that the correlation mentioned exists; some of the results support this thesis, other results do not, and still others may be considered contradictory to it.

Hughson and Crowe, in the paper under discussion, were primarily concerned with determining the function of the round window membrane. On the basis of the increase in the microphonic effect which was produced by pressure on the membrane they concluded: "We are convinced therefore that the round window membrane acts as a safety valve for the inner ear structures and by its mobility effects approximately a 50% absorption of all sound entering the ear. If the membrane is made rigid by pressing on it with a plug of moist cotton, the perception of spoken words and practically all tones is increased at least 50%."[62] But even if the bio-microphonic effect detected could be accepted as a reliable indicator of the energy of the sound vibrations in the cochlea, the results hardly seem to warrant the conclusion that the function of the round window membrane is to *act as a safety valve* to protect inner ear structures. For the function of a safety valve, which is incorporated in a physical system of some sort, *e.g.* a pressure system, is to allow the escape of energy *only when it exceeds* a certain dangerous level, and not at levels beneath this. A device which released energy beneath the level at which it became dangerous would not be helping in any way to protect the structures within the system, but merely serving to render the system less efficient. Such a device might better be called an attenuator than a safety valve. So even if it were true that the mobility of the round window membrane is responsible for a "50% absorption of all sound entering the ear," that is to say, if it were responsible for a reduction in the energy of *all vibrations* occurring in the cochlea, this fact would not justify the hypothesis that the function of the membrane is that of a safety valve.

Finally, we may briefly consider some of the reasons which make it improbable that increased pressure on the round window membrane does lead to an increase in the energy of the sound vibrations occurring in the cochlea, despite the reported increase in the intensity of the microphonic effect. Certain physical grounds indicate that mobility of the round window membrane serves to *increase* the energy of the vibrations that may occur in the cochlea rather than decrease it. Essentially, the cochlea is like a tube of water closed at one end by an elastic membrane (the round window membrane) and at the other by a piston which can move back and forth (the base of the stapes) and containing within the tube an elastic membrane (basilar membrane) which can be set into motion by the movements of the liquid.[63] It seems clear that if the membrane at the end of the tube is made rigid, the piston will not be able to set the liquids in the tube into motion (since the liquid is practically incompressible), unless some other part of the walls of the tube can move under the changing pressures produced by the piston. Increasing rigidity or tension of the membrane should lead then to a *decrease* in the energy of movement of the liquid in the tube. Moreover, if the walls of the tube are capable of responding at all to the changing pressures produced by the piston, then with increasing rigidity of the end-membrane of the tube, relatively more and more energy will be transmitted through the walls. That is to say, as one pathway becomes blocked, relatively more energy will flow through other pathways; just as in an electrical system, when the resistance of one of a number of parallel pathways increases, a greater proportion of the total current flows through the other paths. And such a phenomenon may account for the increase in the intensity of the microphonic effect reported by Hughson and Crowe. A larger proportion of the energy of the vibrations produced at the oval window might now be transmitted through the walls of the cochlea, and through the liquids of the vestibule and semicircular canals and their enclosing walls, since the path by way of the round window was blocked. More sound energy might thus be transmitted through the bony walls of the labyrinth even though the energy of the vibrations occurring in the liquids of the cochlea might have decreased. A corresponding increase in the intensity of the microphonic effects due to the vibration of the electrode and the electrical changes generated in the tissues by the sound waves might then be expected. An explanation of this sort makes unnecessary a conflict with clinical and physical evidence such as occurs on the assumption that increased pressure on the round window membrane leads to an increase in the energy of sound waves in the cochlea.

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[62] *Ibid.*, 2028.

[63] For a discussion of recent ideas on the dynamics of the inner ear, see H. Fletcher, *Speech and Hearing*, 1929, 111-125; R. L. Wegel and C. E. Lane, The auditory masking of one pure tone by another and its probable relation to the dynamics of the inner ear, *Phys. Rev.*, 23, 1924, 276-285; G. v. Békésy, Sur la théorie de l'audition, *Année Psychol.*, 31, 1930, 63-96.

Rademaker and Bergansius. Rademaker and Bergansius were concerned with registering the bio-microphonic effect with string galvanometer and determining how it varied with different frequencies and intensities of stimulus, rather than with examining the conditions upon which the effect depended.[64] The curves of the electrical changes produced by tonal stimuli (tuning forks, organ pipes, and voice) corresponded in all particulars with the galvanometric curves of the stimulating sound waves. The curves of the microphonic effect obtained from simple tonal stimuli were sinusoidal in form, as closely as can be judged from the reproductions of the curves; the curves of the electrical changes produced by a sung vowel showed characteristic higher partials along with the fundamental. Increases in intensity of tonal stimuli led to an increase in the amplitude of the curves of the microphonic effect.

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[64] Rademaker and Bergansius, *op. cit.*, 346-349.

Although Rademaker and Bergansius performed no experiments upon the conditions of the microphonic effect, the results which they obtained upon the form of the curves may throw some light on whether or not the effect is due to action currents in the auditory nerve. Before going on to discuss the significance of these results, we may note that they agree with what has been said by certain of the previous investigators concerning the form of the electrical changes constituting the bio-microphonic effect. In a number of places Wever and Bray indicate that the electrical effects obtained by them from simple stimuli were sinusoidal in form. In one place, they say, "a pure tone sounded into the ear of the cat is recognized as that same tone in the receiver."[65] In a later note, this sine-wave character of their microphonic effect is indicated in a statement made concerning the freedom of their apparatus from distortion effects: "our amplifier and telephone receiver system will not distort to the extent of giving a sine wave type of response to a signal that departs at all widely from it" and add later that work with a cathode ray oscillograph confirmed the indications of the telephone receiver concerning the form of the electrical response.[66] Adrian too, it will be recalled, said that "oscillograph records reproduce the wave-form of the sound and the curves show no sign of being built up of nerve impulses."[67]

These results acquire some significance in indicating whether the microphonic effects obtained were due to action currents when we consider certain facts relating to the transmission of sound waves to the cochlea by the chain of ossicles in the middle ear. As is well known, vibrations transmitted to the inner ear by the ossicles undergo the effect of so-called non-linear distortion. That is to say, equal changes in pressure on the ear drum do not result in equal displacements of the foot of the stapes at the oval window. The result is that sound waves impressed on the ear drum are distorted in their transmission to the cochlea, in such a fashion that a sine-wave stimulus will result in a wave containing not only the impressed frequency, but a series of its harmonics as well. As the intensity of the stimulating sound increases, the amplitude of the harmonics *relative* to the fundamental will likewise increase.[68] If the action current hypothesis is correct, that is to say, if the frequencies occurring in the bio-microphonic effect correspond to the frequencies present in the response of the auditory nerve, and if these in turn correspond to the frequency of the vibrations in the cochlea—then the harmonic frequencies introduced by the ossicles in the transmission of sound to the cochlea should be represented in the microphonic effect as well as the frequencies present in the original physical stimuli. The results of the various investigations which have shown that the microphonic effect obtained from simple stimuli are sinusoidal in form fail to confirm this consequence of the action current hypothesis and so indicate that the electrical changes detected were not due to auditory action currents.

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[65] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 377.

[66] E. G. Wever and C. W. Bray, A note on "A neglected possibility in frequency theories of hearing," this JOURNAL, 44, 1932, 193.

[67] Adrian, *op. cit.*, xxix.

[68] Fletcher, *op. cit.*, 124, 312 f.; L. T. Troland, *Psychophysiology*, II, 1930, 246 f.

It is desirable, however, that a more precise test of this point be made than is provided by the data so far presented. That is to say, what is required is oscillographic records (obtained with a high frequency oscillograph) of the bio-microphonic effect obtained from simple tonal stimuli which range from stimuli of low intensity to those of intensities high enough to produce harmonics of appreciable magnitude. As the intensity of the stimulus increases, harmonics should appear in the curves of the microphonic effect and should increase in magnitude relative to the fundamental component, which corresponds to the stimulating frequency. The failure of this result to appear must then be regarded as a failure to confirm the hypothesis that the microphonic effect is due to action currents of the auditory nerve, and as an indication of the incorrectness of this view.[69]

Kreezer and Darge. When the experimental conditions described by Wever and Bray were duplicated by Kreezer and Darge, a microphonic effect was obtained.[70] This effect did not appear, however, when the precautions mentioned above in this paper (p. 642 f.) were taken to exclude the possibility of induction effects. The authors concluded, therefore, that the effect which they had obtained was due to induction from outside microphonic sources that were set into vibration by the sound waves used as stimuli.

SUMMARY

Our survey is now at an end. Among the various investigations we have found a certain variety in the problems attacked and many differences in the particular conditions of the 'bio-microphonic effect' which were investigated. In most cases in which the influence of the same conditions was examined by different investigators marked differences occurred in the results. Our search, furthermore, failed to reveal in any of the investigations a decisive experimental proof that the microphonic effect is due to action currents in the auditory nerve.

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[69] But the converse of this is not true, that is to say, detection of these harmonics in the curves of the bio-microphonic effect will not prove that the effect is due to action currents, since the harmonics might appear as a result of any one of the microphonic phenomena dependent on sound waves in the cochlea, though not necessarily in the same degree.

[70] Kreezer and Darge, *op. cit.*, 105.

In certain of the investigations, the possibility was examined that certain phenomena *other than* acoustic action currents were responsible for the microphonic effect. Wever and Bray, who followed this procedure, concluded that the effect was *not* due to any of the other microphonic phenomena which they considered and that, therefore, it must have been due to auditory action currents, as the only remaining possibility. We found, however, that this conclusion was not justified, since the other microphonic phenomena were not conclusively eliminated as possible factors in the effect. Kreezer and Darge, who followed a similar procedure, concluded that the bio-microphonic effect which they obtained was due to induction, for it disappeared when adequate precautions for excluding induction effects were taken.

In other investigations, the procedure followed was to determine the influence on the microphonic effect of conditions that would be expected to influence it if it were due to auditory action currents. Adrian, whose experiments were of this sort, presented evidence opposing the 'action current hypothesis.' Adrian, Bronk, and Phillips went on to investigate the conditions of the microphonic effect which they could detect in the cochlea; theirs was *not* an investigation of the effect obtainable from the nerve. Their conclusion that the effect which they obtained was probably due to nerve elements of some sort in the cochlea is compatible with the evidence which they present but is not conclusively demonstrated by it. Davis and Saul investigated some of the conditions of the microphonic effect which they detected in the midbrain and in the auditory nerve. Their conclusion that it was due to action currents in the nerve is compatible with their results, but is not conclusively demonstrated by them, for certain other possible explanations were not excluded.

In still other investigations (Hughson and Crowe, Rademaker and Bergansius), the purpose of the authors was not to investigate whether the microphonic effect was due to auditory action currents; they were concerned, instead, with other problems. These studies were, however, examined but also found to be wanting in evidence capable of demonstrating the correctness of the 'action current hypothesis.' On the contrary, a considerable part of the results of these investigations tends rather to refute the hypothesis that the microphonic effects obtained were due to action currents in the auditory nerve.

The general result of our survey is then that the evidence thus far presented fails to demonstrate that the microphonic effect is due to action currents of the auditory nerve. Hence, it is not possible as yet to conclude. on the basis of this evidence, that the frequencies of auditory stimuli are retained in the response of the nerve. Such a conclusion is premature; the question remains open. Whether it can be readily settled by means of the action current technique will depend on whether more decisive experiments can be devised and upon how readily the gaps in our knowledge, which prevent a precise interpretation of some of the experiments already made, may be filled in.

ELECTROPHYSIOLOGY OF MENTAL ACTIVITIES

By Edmund Jacobson, University of Chicago

The road to a science of psychology evidently must follow a rigorous examination of the question, What occurs in the organism simultaneous with and indispensable to mental activities? In the years 1829 to 1834, Weber made the studies on cutaneous and kinesthetic sensations which frequently are regarded as marking the beginning of experimental psychology.[1] After adding to these observations, Fechner,[2] in 1860, sought to make of psychology an exact science by developing the "fundamental relation or relations of dependency between the body and mind." This science, as is well-known, he called "Psychophysics," and its fundamental formula was, in effect, that sensation is proportional to the logarithm of stimulus. We need not here review the criticisms aroused by Fechner's interpretation; to find a scale to measure the experiences called sensations in terms of the objects or energies outside the body called stimuli has generally been considered impossible. According to the present investigation, in contrast with the views of Fechner, the goal should be the determination and measurement of nervous and muscular changes specific to the mental activity studied. This may reasonably be expected to open the road to find quantitative relationships between the nervous and muscular changes and their appropriate stimuli.

Classical psychologists, including experimentalists, at least until recent years, have employed some form of examination by the individual of his own activities. This approach was developed, chiefly by Wundt and his many disciples, with the aid of instruments and various mechanical devices, to permit repetition of observations under standard experimental conditions.

Wundt's attempts to link mental events to their physiological correlates,[3] particularly feelings to pulse and respiration, met with no startling success. Looking backward, we can see that he approached the problem primarily as a

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[1] E. Weber, *De Tactu*, in *Annotationes anat. et phys., programma collecta*, 1851; *Die Lehre vom Tastsinne und Gemeingefühle*, 1851.

[2] G. Fechner, *Elemente der Psychophysik*, 1889.

[3] W. Wundt, *Grundzüge der physiologischen Psychologie*, 5th ed., 1902.

systematic theorist. In spite of the vast array of technical methods which he employed, the influence of philosophical training on the psychologist is apparent. More recently, subsequent to experiments by Binet and various members of the Würzburg school, introspective methods were applied to the so-called higher intellectual activities; but here instruments seemed of little aid, and while the observations proved of great interest to some psychologists, to others—particularly Wundt himself—the methods appeared so crude as to be utterly unscientific. We were confronted in the days of Titchener with controversies, frequently acrimonious, resulting in divisions into schools of belief and marked by the absence of an incisive method to determine which opinion was correct. The poverty of human psychology at this stage as compared with other biological sciences was apparent.

Introspective psychologists have contended that only the individual is in a position to give adequate descriptions of his conscious activities as he himself experiences them. Accordingly the custom has developed of employing a number of self-observers during an experimental investigation and securing reports independently from each observer. Where such reports have agreed, conclusions have been drawn bearing on the character of conscious experience. But experimenters who have employed these methods have themselves called attention to what is, from the present point of view, the limitation of introspective psychology. They have pointed out that the introspector is called upon only to give accounts of his experiences as such; he should not report on his physiological processes. If he does, he commits the 'stimulus error.' Accordingly, we shall not expect to learn from the methods of introspective psychology what in the organism is indispensable to mental activities.

Nevertheless, methods of autosensory observation form the starting-point for the present studies. When we recall the delicacy of the human senses—their use, for example, in certain chemical tests to detect as little as one part of a substance diluted a billion times—we should expect them to be of service if we are ever to solve problems of the mechanism of conscious activities. What is required is that the reports of the senses, used directly in observation, be checked by objective methods, such as electrical measurement.

Despairing that psychology, based upon introspective methods, could ever become a real science, certain students under the leadership of Watson decided to limit their studies to problems of behavior. Some of them, in their enthusiasm over this limitation, proceeded to question the validity of all observations depending upon the observer's watching his own processes. Notwithstanding this skepticism, I am informed by a prominent ex-behaviorist, conclusions frequently were evolved during prolonged conversations replete with introspections but not admitted as such.

Since Lange's valuable observations on physiological phenomena associated with emotions published in 1889,[4] many investigators have added their contributions. Of particular interest have been studies of the galvanic reflex. This change in resistance to the passage of electric current attendant upon emotion has been found to depend upon the tissues within the skin; although it is not yet certain whether constriction of the blood-vessels,[5] or changes in the epithelial cells,[6] or sweating[7] alone is responsible. It is agreed that the galvanic reaction depends upon the autonomic nervous system,[8] following emotion as an after effect, or at least as a late effect or manifestation. But we remain in ignorance as to what occurs in the neuromuscular system simultaneously with the emotional experience.

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[4] C. Lange, *Über Gemüthsbewegungen*, 1889.

The aim of the present investigation is to learn something about what takes place in the nervous or neuromuscular system specifically for various forms of mental activity, and so far as possible to measure the processes in physical terms. During a period of study of relaxation covering twenty-three years, individuals who have been trained to observe their muscle sense generally have agreed in independently reporting sensations as from slight or pronounced muscular contraction marking the occurrence of mental activity. The writer has repeatedly made this observation on himself during the above-mentioned period.[9] We desire to test the possibility thus opened that the muscle sense, if carefully and critically employed, may put us on the track of observations that may be objectively verified for the whole range of our mental experience.

Accordingly in the studies here recorded, we deal with action-potentials and not with mere resistance changes, such as the galvanic reflex. These investigations, which I have been attempting to get under way since 1911, have been described elsewhere,[10] but it is believed that a résumé including certain recent results may prove of interest to students of psychology.[11]

General methods. It has been necessary to develop extremely sensitive apparatus in order to detect and measure the slight changes in potential in the nervous or muscular system during mental activities. Thermionic amplification has been used with the string galvanometer. In the most recent studies, measurements are possible of potential differences as small as one millionth of a volt, and such small changes are recorded with negligible lag in time on photographic film moving about 1 in. per sec. Electrodes are devised to connect the tissues with wires leading to the amplifier. Apparatus and method have been described with details that need not here be repeated.

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[5] F. Aveling and R. J. S. McDowall, Electric resistance of skin, *J. Physiol.*, 60, 1925, 316; H. B. Denscham and H. M. Wells, The mechanism by which electrical resistance of the skin is altered, *Quart. J. Exper. Physiol.*, 18, 1927, 175.

[6] C. P. Richter, A study of the electrical skin resistance and the psychogalvanic reflex in a case of unilateral sweating, *Brain*, 50, 1927, 216-235.

[7] C. W. Darrow, Sensory, secretory and electrical changes in the skin following bodily excitation, *J. Exper. Psychol.*, 10, 1927, 197; The galvanic skin reflex and finger volume changes, *Amer. J. Physiol.*, 88, 1929, 219-229.

[8] E. Schilf and A. Schuberth, Über das sogenannte psychogalvanische Reflexphänomen beim Frosch und seine Beziehung zum vegetativen Nervensysten, *Arch. f. d. ges. Physiol.*, 195, 1922, 75-95.

[9] E. Jacobson, Progressive relaxation, this JOURNAL, 36, 1925, 73-87.

[10] Jacobson, Action currents from muscular contractions during conscious processes, *Science*, 66, 1927, 403; Electrical measurements of neuromuscular states during mental activities. I, *Amer. Jour. Physiol.*, 92, 1930, 567-608; II, *ibid.*, 94, 1930, 22-34; III, *ibid.*, 95, 1930, 694-702; IV, *ibid.*, 95, 1930, 703-712; V, *ibid.*, 96, 1931, 115-121; VI, *ibid.*, 96, 1931, 122-125; VII, *ibid.*, 97, 1931, 200-209.

[11] Several suggestions regarding the writing of this résumé were kindly made by Professor Harvey Carr.

For the most part, *S*s are employed who previously have been trained to relax.[12] This is generally necessary because restless movements occurring in untrained *S*s occasion action-potentials which obscure the readings concerning phenomena of mental activity. Relaxation, then, is requisite to furnish control tests and, in a sense, to isolate the mental activity investigated.

The *S* lies relaxed upon a couch in a darkened, quiet, partially sound-proof room. His eyes are closed. Distraction by *E* is carefully avoided. It is agreed between *E* and *S* that the clicks of a telegraph key are to be signals: the first, to engage in a particular mental activity; the second, occurring soon afterwards, to relax any muscle tensions present. By means of a signal magnet, the instant of occurrence of each click is recorded on the photograph.

Imagination of movement. To secure data for statistical consideration, one instruction is used many times with one *S* and repeated with others. *S* is to imagine bending the right arm. An imagined persistent or steady flexion of the right forearm is meant, rather than an imagined brief flexion and extension. One electrode is connected with the right biceps-brachial muscles and the other to a (partially) indifferent point—the skin or under the skin in the elbow pit.

During a period of general relaxation, when the galvanometer string with one lead attached to the right biceps is recording an approximately straight line, the signal is given for the subject to imagine what would in actual performance involve contractions in the above mentioned muscle; for instance, to imagine that he is steadily flexing the right forearm. Generally within a fraction of a second, the string ceases its steady course and engages continually or intermittently in relatively large swings which cease soon after the signal is given to relax any muscular tensions present (see Fig. 1). This test has been made upon about 20 *S*s and the results have been in agreement, with modifications later to be stated.

But under the above conditions, if it has been agreed that the first signal is to mean, "Imagine bending the left foot," the string shows no such onset of large swings, generally continuing unaltered in its course (Fig. 2). This type of control test will be called 'Control 1.' Similarly the string continues in a steady course even after signals have been given, "Imagine bending the left arm" (Control 2), "Actually bend the left foot one inch" (Control 3), "Actually bend the left arm one inch" (Control 4), "Imagine the right arm perfectly relaxed," or, "Imagine the right arm paralyzed" (Control 5), "Imagine extending the right arm" (Control 6), and "When the signal comes, do not bother to imagine" (Control 7).

The negative results noted in the control tests rule out two possibilities: (1) that the string deflections observed during imagination are due to effects of the sound on the S; (2) that the act of imagination involves 'action-currents all over the body.'

The period from the beginning of the first signal to the appearance of the string vibrations (reaction-time) is often as brief as 0.2 sec. in trained Ss, but averages 0.4 sec. in about ⅗ of all the tests on imagination of bending the right arm.

It seems unnecessary to present results in tabulated form here, since this has been done previously. That the electrical phenomena observed following the instruction to imagine bending the right arm are not artifacts due to training in relaxation has been tested with 6 Ss not so trained. In these instances the string as a rule is not so quiet before the first and after the second signal as in trained Ss; therefore the contrast of the vibrations occurring after the first signal with those of the foregoing and subsequent control periods is not so marked as in trained Ss. However, after the S has become accustomed to conditions and has attained a fairly quiet state, tests show a definite increase in the microvoltage of vibrations with untrained as with trained individuals, excepting in instances where the instruction leads to visualization alone, as will later be discussed.

After setting the galvanometer to record higher voltages, a record is taken while the S actually flexes his right forearm, moving the wrist about 1 cm. The photograph quite resembles those above described for imagination of flexing the right forearm; but the microvoltage during overt movement is considerably greater than during imagination.

Imagination and recollection of various muscular acts. In a further series of investigations, the S is instructed as before except that he is to imagine lifting a 10-lb. weight with the right arm. Under these conditions, electrical fluctuations are found to occur in the biceps region of that arm during such imagination, but not during the foregoing and succeeding periods when the S is instructed to relax. Likewise they are not found to occur in the right arm when S imagines that he is bending his left arm, nor in the other control tests previously specified. Again it seems unnecessary to present results in statistical form, since this has previously been done.

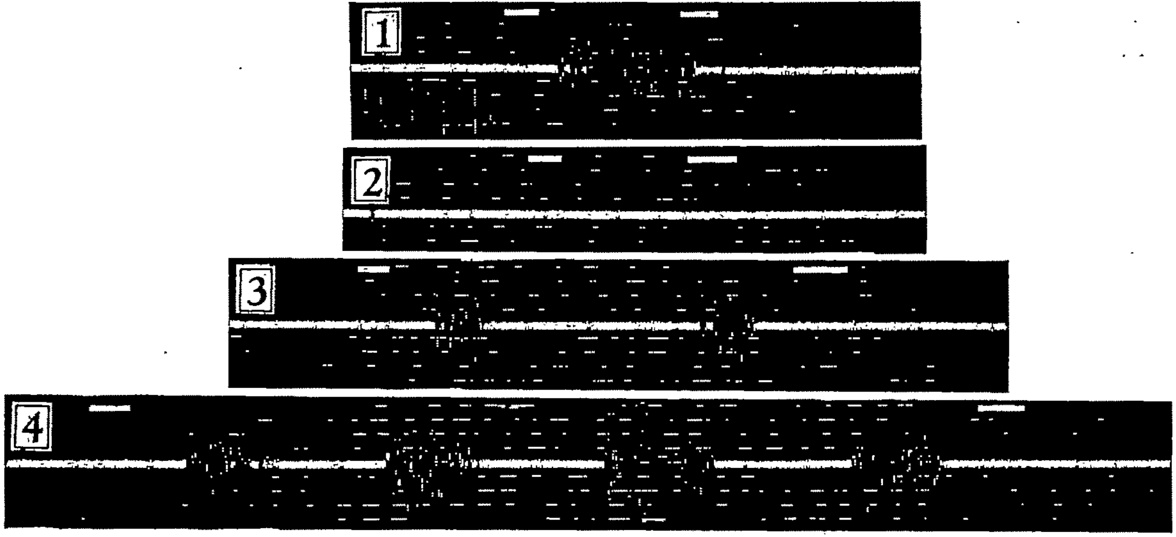
Recently this experiment has been repeated with 15 *S*s previously trained to relax and with 12 *S*s not trained. During imagination of lifting the weight with the right arm, action-potentials were recorded from the right biceps in 54 out of 63 tests with the trained *S*s and in 41 out of 63 with the untrained *S*s. During imagination of lifting the weight with the left arm, the galvanometer showed no electrical change from relaxation in the right biceps in 24 out of 24 tests with the trained *S*s and in 20 out of 25 tests with the untrained *S*s.

Imagination and recollection of various other acts commonly performed with the right arm have been studied with electrical apparatus. It is necessary to test for the presence of action-potentials when instructions are given which can be carried out by the *S* in terms of responses of one or another muscular group not always determinable in advance. Ideally, for these tests we should need a separate string and a set of electrodes for each important neuromuscular group, as well as a galvanometer circuit considerably freer from electromagnetic and electrostatic disturbances than was the circuit first employed; for there is always the possibility that results may be called negative through masking of very slight action-potentials by extraneous electrical disturbances.

In preliminary tests on imagination, the following instructions were selected by *E* (without consulting the *S* beforehand): "When the signal comes, imagine sweeping a room with a broom. Upon the second signal, relax any muscular tensions present." Other examples of instructions are: "Imagine writing your name;" "Imagine yourself rowing a boat;" "Imagine yourself boxing;" "Imagine scratching your chin;" "Imagine plucking a flower from a bush;" "Imagine combing your hair;" "Imagine playing the piano."

In half of these preliminary tests (14 out of 28) the string excursions were markedly increased during imagination as contrasted with relaxation; but in the other half the contrast was not marked. The *S*s sometimes stated that following the instruction to imagine, they carried out the imagined act with some muscle-group other than the flexors of the forearm. In other instances they stated that they visualized themselves performing the act, but failed to have arm-muscle sensations. If these statements are correct, they evidently account for the absence of action-potentials from the biceps region in some of the above-described tests.

If the instructions selected by *E* fail in some instances to induce the *S* to imagine various muscular acts as carried out with his right biceps, but lead him instead to visualize or to carry out the imagination with other muscle-groups, another approach is suggested. *S* is asked to mention various acts which he would naturally perform with the right arm and would naturally imagine as so performed. In making such selection, he will be aided if he has had experience at



FIGS. 1–4. (Legends opposite)

FIG. 1. PHOTOGRAPHIC RECORD OF STRING DEFLECTIONS DURING IMAGINATION

The *S*, previously trained to relax, has been instructed, "Upon hearing the first signal, imagine lifting a 10-lb weight in the right forearm. Upon hearing the second signal, relax any muscular tensions, if present." One electrode in the biceps muscle, about 6 cm. above the other, which is inserted under the skin in the elbow-pit. Intervals between vertical time lines shown in left lower corner = 1/5 sec. Distance between horizontal lines, approximately,

1 mm. = 1.3×10^{-6} volts.

This photograph shows between signal marks (appearing at the top) groups of vertical lines, evidently due to action-potentials, which are absent in the periods of relaxation before the first signal and which subside soon after the onset of the second signal. We conclude that in the present test, muscle-fibers of the right biceps contract as the *S* imagines lifting with the right forearm.

FIG. 2. PHOTOGRAPHIC RECORD OF CONTROL TEST

Conditions the same as in Fig. 1, excepting that here the instruction has been, "Upon hearing the first signal, imagine lifting the weight with the left forearm." No evidence of action-potentials from the right biceps here appear. Results from the right biceps are negative similarly in tests where the instruction has been, "Do not bother to imagine," or "Imagine bending the right leg" (see text). The latter instruction is characteristically followed by action-potentials in the right leg.

FIG. 3. PHOTOGRAPHIC RECORD OF STRING DEFLECTIONS DURING IMAGINATION OF A REPEATED ACT

Conditions the same as for Fig. 1. The instruction has been, "Imagine hitting a nail twice with a hammer held in your right hand." Between signal marks, two series of long vertical lines, indicating action-potentials, are seen, separated by an interval of practically horizontal line, indicating relaxation.

FIG. 4. PHOTOGRAPHIC RECORD OF STRING DEFLECTIONS DURING IMAGINATION OF A RHYTHMICAL ACT

This photograph shows between signal marks four groups of vertical lines, indicating action-potentials from the flexor muscles of the right forearm, separated by approximately equal lengths of practically horizontal line, indicating relaxation.

observation of the muscle-sense. Four *S*s were used in the following tests who had such experience. In addition, all except one (*G*) had been trained to relax.

The instructions selected by them were as follows: "Imagine lifting a cigarette to your mouth;" "Imagine pulling a microscope toward yourself;" "Imagine pulling up your socks;" "Imagine grinding coffee;" "Imagine yourself chinning on a horizontal bar;" "Imagine hugging;" "Imagine shifting the gear of your automobile to first speed;" "Imagine pulling a door open;" "Imagine lifting a glass of milk to your mouth;" "Imagine climbing a rope;" "Imagine throwing a ball;" "Imagine pulling up weeds;" "Imagine pumping a bicycle tire."

Results for these 4 *S*s are positive for action-potentials from the right biceps-brachial region in 97.5% of the tests (159 out of 163). All of the control tests prove negative.

The microvoltage for such imagination, as averaged for all *S*s, is about 26. This is much less than the average value found for imagining lifting a 10-lb. weight (41 microvolts). No attempt is made to measure the microvoltages as they occur in the tissues, but only in the attached electrodes. Corrections need to be made for various errors, as previously discussed; therefore the figures presented are not final.

Beautiful records are secured when the *S* is requested to imagine some act performed rhythmically, such as "climbing a rope," "pumping a tire;" or "turning an ice-cream freezer" (Figs. 3 and 4). Following the signal to imagine, the string shows a brief period of long vibrations, then an interval of rest for a fraction of a second or more, then similar periods of vibrations and rest, occurring in rhythmical succession until the signal to relax is given. On the other hand, instructions to imagine some act that involves only one relatively brief muscular action, such as "throwing a ball," typically are followed by only one brief series of long vibrations. But if the instruction is varied, "When the signal comes, imagine throwing a ball. After an interval, repeat this experience," then the record shows two periods of long vibrations, separated by an interval of rest. Corresponding results follow the instruction to imagine the act three times.

Recollection is tested like imagination. Instructions are to recall such activities as lifting a glass or a cup of tea at dinner, reading a certain paper, removing a shirt, putting on a coat or rubbers, raising the right arm, picking flowers, pulling up dandelions, pulling up an auger, playing the piano, playing the violin, boxing, punching a bag, brushing teeth, washing dishes, sweeping, running a carpet-sweeper, scrubbing, rowing, hitting a ball at tennis, fishing, landing a fish, and slinging a sledge. *S* stated before or after each test that he had previously had the experience denoted by the instruction.

If instructions are so chosen by *E* as to indicate an activity commonly performed by the right arm, we may reasonably expect that if recollection involves neuromuscular activity, some noteworthy proportion of responses will take place in the right biceps-brachial region and therefore be recorded. However, if instructions are chosen by *S* to indicate an activity which he observes that he recalls with muscle-sensations from the right arm, we should expect a higher proportion of positive results, in accordance with what was said above concerning imagination.

Following the former procedure, 7 out of 9 *S*s show action-potentials predominantly from the right biceps-brachial region; that is, in 60 out of 90 tests. The reports of the remaining 2 *S*s suggest that following the instructions they generally engaged in visual recollection alone. Perhaps this explains why action-potentials were in most tests absent for them.

In general, for any particular *S*, the microvoltages were lower and the series of action-potentials were considerably briefer during recollection than during imagination.

Evidence that specific muscles contract during imagination. The discovery that action-potentials exist in electrodes connected with specific muscles during each process of imagination or recollection leads to the question whether the fibers involved actually contract. Accordingly a lever is arranged so that under controlled conditions flexion of the right arm can be magnified about 80-fold and recorded photographically along with the electrical curve.

The results show that following the signal for *S* to imagine steadily bending his right arm or to imagine lifting a 10-lb. weight in the same manner, the lever records a flexion of the arm, generally of microscopic extent. Soon after the second signal, which signifies that *S* is to relax any muscular tensions present, the lever suddenly returns to the position it had while the arm was at rest. But in control tests, where *S* imagines bending the left arm or imagines lifting the weight with that arm, no microscopic flexion of the right arm and no action-potentials from the biceps region are recorded.

The mechanogram indicates that the maximal excursion of the arm, averaged for each *S*, varies from 0.07 mm. to 0.32 mm. In the majority of the records there is shown a practically uniform motion per sec. This ranges, in most instances, from 0.02 to 0.05 mm. per sec. Considering also other evidence in the physiological literature, we conclude that the detection of action-potentials in muscles always signifies the presence of shortening of fibers.

It is evidently desirable to include as many tests as possible bearing upon the question whether *S* can imagine flexing the right arm or lifting a weight with that arm without actually engaging in at least a microscopic flexion of the right arm. The following tests are devised so as not to depend upon the reports of *S*. (Trained *S*s all agree that in their experience it is impossible simultaneously to imagine bending the right arm and to keep it perfectly relaxed. Whether visualization can occur alone is a matter for subsequent tests.)

(*1*) *S* is instructed, upon hearing the first signal, to imagine bending the right arm or to imagine lifting a weight with that arm. Electrodes are attached to the right biceps region in the usual manner, and the lever is also attached as above described.

(*a*) In one test, the above procedure is carried out and *S* is instructed to "cease to imagine" when he hears the second signal.

(*b*) In a second test the procedure is the same, except that *S* is instructed to "relax any muscular tensions present in the right arm" when he hears the second signal.

The photographic records produced in (a) and (b) are found to be identical in type. They are entirely like those shown in Fig. 1. That is, with instructions and records as above stated, the instruction to cease to imagine is followed by the same result as the instruction to completely relax muscularly. This is evidence, secured without the intervention of the report of *S*, that in this instance ceasing to imagine is, at least in part, the same process as muscular relaxation. It supports the view that the microscopic muscular contraction is here requisite to the act of imagination.

(*2*) *S* is instructed, upon hearing the first signal, to keep the right arm completely relaxed and simultaneously to imagine bending the right arm or to imagine lifting a weight with that arm. Mechanical and electrical attachments continue as above described.

(*a*) In one test, the instruction is added that if *S* finds it impossible to imagine and to relax simultaneously as above directed, he shall at any rate engage in imagination.

(*b*) In a second test, the procedure is the same, except that here the additional instruction is that if *S* finds it impossible to imagine and to relax simultaneously as above directed, he shall at any rate keep his right arm perfectly relaxed.

The photographic records produced in (a) are entirely like those shown in Fig. 1, while those produced in (b) are entirely like those shown in Fig. 2. This indicates, without the intervention of the report of *S* but entirely in conformity with that report, that it is found impossible simultaneously to imagine bending the right arm and to keep it perfectly relaxed. Analogous results apply to imagining lifting a weight with the right arm. The photographic records indicate that *S* either imagines—whereupon action-potentials appear along with at least a microscopic flexion; or else relaxes the right arm completely—whereupon the electrical and mechanical records are negative.

The results recounted under *(1)* and *(2)* afford additional objective evidence that in the instances studied, slight muscular contraction is requisite to the process of imagination.

The galvanic reflex. Differences between the galvanic reflex (G.R.) and the action-potentials (V°_{im}) here found characteristic of imagination and recollection are as follows.

(1) Voltage: G.R. has many times the voltage of V°_{im} and is commonly recorded with the D'Arsonval galvanometer, which, as commonly used, is not sensitive enough to record V°_{im}.

(2) Reaction-time: an interval of 1–4 sec. or more commonly intervenes between the stimulus and the occurrence of G.R. But only a fraction of a second commonly intervenes between the signal to imagine and elongation of V°_{im}. Evidence is presented that the latter phenomena are simultaneous with imagination.

(3) Frequency per second: G.R. commonly lasts considerably more than 1 sec., while V°_{im} occurs frequently per sec.

(4) Wave form: G.R. is a rise or fall of curve, while V°_{im} at usual film rates is a series of connecting almost vertical lines. Photographic records of V°_{im} during imagination resemble those taken with similar attachments when *S* bends his arm 1 cm. or more, excepting the above-mentioned difference in voltage.

(5) Direction of potential: G.R. often is chiefly unidirectional with respect to the line of zero potential. But in all instances either electrode used in recording V°_{im} is alternately positive and negative frequently per sec.

(6) Tissue of origin: G.R. arises from changes within the skin—sweat gland secretion, vascular changes, or altered permeability of cells; while V°_{im} can be recorded directly from muscular tissue, when the skin has been excluded.

(7) Stimulus: for G.R. an exciting sensory stimulus or some word is selected to arouse the emotions. The reaction is involuntary. For V°_{im}, a particular instruction is given to imagine or recollect, and *S* carries it out and ceases, at will.

(8) Contraction: during imagination and recollection, there occurs a muscular contraction, practically simultaneous in duration with the action-potentials. Since the galvanic reflex is generally conceded to be due to electrical effects depending upon elements in the skin, the proof is now conclusive that the electrical manifestations (V°_{im}) during imagination and recollection are not galvanic phenomena.

An objective measurement of introspection. The coincidence of the occurrence of imagination and of lengthened string vibrations is clearly brought out in certain additional tests. A signal magnet is arranged to record on the photographic record a line which can be broken when *S* presses the button of a switch with his left index finger. He is instructed to press the button during the period when imagination is taking place, releasing it at other times. After a little practice, this can be done successfully. To be sure, a perfect parallelism in time between the large linear deflections and the indicated process of imagination is not expected. Deviations of a fraction of a second will doubtless occur, since this period of time may be required for *S* to perceive that he is imagining or has ceased to imagine. In order to gain some idea of the reaction-time of these 3 *S*s, as they lay upon the couch with eyes closed, they were requested to press the button during periods when a buzzer sounded. For all 3 the average value was 0.24 sec.

According to the signal line thus made by the *S*s in these tests, the period during which imagination takes place coincides with the period during which the string shows lengthened vibrations, except for fractions of a second at the beginning or end. As anticipated, the reaction-time of *S* in pressing the button needs to be taken into account. Fifty figures were secured for the 3 *S*s in total, representing the discrepancies in onset or conclusion between the period of lengthened vibrations and the period of imagining indicated by the *S*. The average for these figures was found to be practically identical with their average reaction-time as determined by the method described in the previous paragraph; namely, 0.23 sec. The extremely close correspondence of the latter figure to the value 0.24 previously mentioned is probably in part accidental; but making due allowance for this, the result indicates that the period of imagining coincides with the period of lengthened vibrations, subject to the slight discrepancies introduced by *S*'s reaction-time.

The procedure just described is of interest in that, viewed from a certain angle, it is an objective test of *S*'s introspective observation. So far as I know, this is the first instance on record where such objective measurement has been accomplished.

Relaxation of mental activities. Following the onset of the second signal, which signifies that *S* is to cease to imagine, there generally occurs with *S*s who have been trained to relax a distinct diminution of the large deflections within a time-interval of 0.2 to 0.5 sec. A complete disappearance of such deflections generally occurs under the same conditions within 1.5 sec. Exceptions to this generally were infrequent with the trained *S*s, but they occurred more often with 2 out of 3 of the untrained *S*s. Most notably, an untrained *S* failed to relax, as indicated by the persistence of lengthened deflections, on one occasion for 7 min. and on another occasion for more than 2 min.

The amputated limb. In the light of the foregoing observations it becomes of interest to inquire what occurs in individuals with amputated limbs who assert that they can do everything in imagination with the lost part that they can do with the intact part.

The left arm of a graduate student at the University of Chicago had been amputated above the elbow-joint at about the age of 8 yr. He said that he could imagine doing anything with his left hand that he could do with his right. This assertion would harmonize with our results if it meant that upon imagining acts with his left hand he merely visualizes or verbalizes the action; but not if it meant that he could imagine acts with the lost hand fully in the same physiological manner as normal *S*s.

In brief, the results with this *S* show that when he imagines bending the missing left hand, there generally occur action-potentials from the stump-biceps muscle as well as action-potentials from the muscles that flex the right hand. In other words, when this *S* engages in mental activity concerning his left hand, certain muscles contract; but these, for example in imagined flexion, instead of being merely the muscles that flex the left hand, as in intact *S*, are in the stump of the upper arm, or in the intact arm, or in both places. *S* was not informed as to the purpose or methods of the investigation. It was therefore of striking interest, when, after he had evidently engaged in subjective observation during a number of tests, he suddenly volunteered that he desired to correct his original statement that "he can imagine doing anything with his left hand that he does with his right." He now stated that when he does something with his right hand the left seems in imagination to duplicate the performance, going through the same experience. But he never has experiences of his left hand's performing any act independently of the right. He adds, "My imagination of bending the left hand is but a shadow—a duplicate of what the right hand is imagined to perform." In short, his original statement is ambiguous and he corrects it. He can imagine doing anything with his left hand that his right hand does, but only under one condition; namely, that the right hand, at the moment of the imagination, actually engages in that same act or is imagined to engage in that same act. No independent imagination, such as exists for intact *S*s, exists for this *S*'s left hand.

We find, then, that in this *S*'s mental activities involving the imagined or recalled use of the lost muscles which flex the left hand, a substitution-reaction occurs in the corresponding muscles of the right hand or in the remnant muscles of the left upper arm or in both. In normal *S*s, it was found, at least after a little practice, that mental activities involving the muscles of the left hand are generally accompanied by no reaction in the right arm. A certain measure of independence exists for the two sides, particularly after a little practice. But this *S* apparently has lost the ability to imagine his left hand acting independently of his right hand.

Visual imagination and recollection. Visual imagination and recollection are tested with electrodes placed near the eyeballs. The circuit used yields a pattern on the photographic record distinctive for looking in each particular direction. During a period of general relaxation, when the galvanometer string is vibrating slightly and uniformly, recording a fairly horizontal band on the photographic paper, the signal is given which will incite the subject to visual imagination. Generally within a fraction of a second, the string shoots forth and back or alternately for fractions of a second or more, producing deviations from the horizontal on the photographs. These deviations cease soon after the signal is given to relax any muscular tensions present.

If the room is not too dark, *E* can generally observe that the closed eyelids of *S*s appear quiet during extreme relaxation, whereas a slight movement or convergence of the eyeballs as a rule can be detected at the moment of visual imagination. Likewise the photographic records for visual imagination show patterns resembling those above-described for eye-movements. For instance, the pattern for imagining Eiffel Tower is practically identical with the pattern of the same *S* for looking upward. Evidently in imagining the tower, the *S*'s eyeballs move upward, somewhat as they would upon actually seeing a tower. Correspondingly he reports that in imagining he looks from the base to the top of the tower.

In general, during visual imagination and recollection, we find that electrical records are secured from the ocular muscles, producing photographic patterns like those following instructions to look in one direction or another. As shown by these records, eye-movements (or convergence) characteristically occur during visual imagination and recollection.

When the trained *S* is requested to relax his eye-muscles, the curve becomes relatively horizontal and straight and, according to his report, the visual imagination or recollection disappears. Furthermore, if interrupted at any time when the curve has been relatively horizontal and straight, even if no instruction to relax has been specifically repeated, he reports that visual imagination and recollection have been absent.

Variation of specific muscles contracting during imagination. On introspective grounds, psychologists have long assumed that the same act or object can be imagined by *S* at different moments through different types of imagery. For instance, lifting a weight with the right arm might be imagined with "muscular imagery" in the right arm, or again with visual imagery. The physiologist is obliged to avoid such assumptions, but needs to develop objective tests. These are made in the present instance by attaching a set of electrodes in the ocular region of the subject, while the lever is used simultaneously to record flexion of the right forearm.

When the trained *S* is instructed "to visualize bending the right arm," voltage changes in the ocular region, as from eyeball movement, are recorded in almost all instances. But under the same conditions, action-potentials are absent from the arm-muscles in almost all instances. This indicates that the matter of bending the right arm may be imagined in at least two physiological manners: (1) in visualization—that is, with contraction occurring in eyeball muscles; and (2) in muscle sensation—that is, with contraction occurring in muscles of the right arm.

Further tests are made with electrical and mechanical recording instruments applied as above set forth, but the instruction merely is to "imagine bending the right arm," or to "imagine lifting a 10-lb. weight." While the 3 *S*s employed minutely flex the right forearm in all instances during such imagination, they visualize in some instances but do not in others, as indicated both by the objective records and by their subjective reports.[12]

In different *S*s, then, the same muscles do not always contract during the imagination or recollection of a particular act or object. But the results indicate that during imagination or recollection, muscular contraction, if absent from one region, will be found in another. It is presumed that this principle explains why the instruction to imagine using the right arm is not invariably followed by the occurrence of action-potentials in the right arm: in such instances, we assume, *S* merely visualizes the act. Further tests in this direction remain to be performed.

The speech musculature. Investigations also have been carried out on the musculature of speech during imagination, recollection and abstract thinking. Since Plato called thinking "an inner speech," nothing definite has been developed on the physiology of this relationship.

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[12] Jacobson, *Progressive Relaxation*, 1929.

Introspective accounts of Erdmann,[13] Geiger,[14] Stricker[15] and others agreed that during mental activity there commonly occur feelings like those present in the musculature during actual speech, but left in doubt whether these feelings arise from peripheral contractions. This doubt persists to date. Dodge[16] reports that after anesthetizing his tongue and lips with 20% cocaine solution, "inner speech" is not disturbed, showing its independence from peripheral elements. Mechanical methods to record movements in the speech-organs during mental activities have brought divergent results in the hands of various investigators. Positive results, at least in a considerable proportion of tests, have been reported by Curtis,[17] Courton,[18] Tomor,[19] Wyczoikowska,[20] Reed,[21] Golla,[22] and Clark,[23] but negative results by Krall,[24] and particularly by Thorson.[25] The latter points out the inadequacy of previous methods used to record movements in the speech-organs and, employing Sommer's tridimensional analyzer, obtains magnifications up to 4.5. In spite of these improvements upon previously used methods, she finds that her experiments "throw no light on the actual mechanism of internal speech," and that they show "no qualitative correspondence between overt speech and implicit speech during the verbal formulation of the same ideas." Likewise after a comprehensive review of the whole subject, Schilling finds that investigations have not yet made it possible to say conclusively as to the bodily accompaniments of inner speech.[26]

The present tests are made with electrodes in the muscles of the tongue or under-lip. Instructions are chosen to include not alone imagination and recollection, but also other types of mental activity. Briefly stated, the instructions include: to imagine counting; to imagine telling your friend the date; to recall certain poems or songs;

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[13] E. Erdmann, *Psychologische Briefe*, 1851.

[14] L. Geiger, *Ursprung und Entwicklung der menschlicher Sprache*, 1868.

[15] S. Stricker, *Studien über Sprachvortstellungen*, 1880.

[16] R. Dodge, *Die motorischen Wortvorstellungen*, 1896.

[17] H. S. Curtis, Automatic movements of the larynx, this JOURNAL, 11, 1899, 237-239.

[18] H. C. Courten, Involuntary movements of the tongue, *Studies from the Yale Psychol. Lab.*, 10, 1902, 93-95.

[19] E. Tomor, Die Rolle der Muskeln beim Denken, *Arch. f. d. ges. Psychol.*, 17, 1910, 362-366.

[20] A. Wyczoikowska, Theoretical and experimental studies in the mechanism of speech, *Psychol. Rev.*, 20, 1913, 448-458.

[21] H. B. Reed, The existence and function of inner speech in thought processes, *J. Exper. Psychol.*, 1, 1916, 365-393.

[22] F. L. Golla, The objective study of neurosis, *Lancet*, 201, 1921, 116 ff.

[23] R. Clark, An experimental study of silent thinking, *Arch. de psychol.*, 48, 1922, 1-96.

[24] K. Krall, *Über unwillkürliches Flüstern*, 1925.

[25] A. M. Thorson, The relation of tongue movements to internal speech, *J. Exper. Psychol.*, 8, 1925, 1.

[26] R. Schilling, Ein Beitrag zur Funktion des Vestibularapparates, *Zsch. f. Psychol.*, 91, 1929, 204-246.

to multiply certain numbers; to think of abstract matters such as 'eternity,' 'electrical resistance,' 'Ohm's law,' the meaning of the word 'incongruous,' or 'everlasting;' and to make up your mind what you are going to do tomorrow.

When the electrodes are connected in the speech musculature of the trained *S*, the string shadow is practically quiet during relaxation. But promptly after the signal is sounded to engage in mental activity involving words or numbers, marked vibrations appear, indicating action-potentials. Soon after *S* hears the signal to relax any muscular tensions present, the vibrations cease, and the string returns to rest.

The series of vibrations occur in patterns evidently corresponding with those present during actual speech. For if the amplifier rheostat is set for a relatively low sensitivity and the instruction has been, "When the first signal comes, count aloud but as faintly as possible, 'One, two, three,' " three series of vertical lines are generally found on the photograph separated by a horizontal interval. If now the amplifier rheostat is changed to a sufficiently higher sensitivity and the instruction is this time to imagine or to recall counting but not actually to count, a quite similar photograph is secured. Likewise when verbal matters, such as a poem, are imagined or recalled, records of action-potentials are secured resembling those from actual faint speech, but of considerably less voltage.

For control tests the instructions are; (1) "When the signal comes, do not bother to imagine;" (2) "Imagine Eiffel Tower;" or (3) "Imagine lifting a 10-lb. weight with the right forearm." In the tests previously described, (1) was found to be followed by no evidence of muscular contraction, (2) by evidences of contraction in muscles that move the eyes, and (3) by contraction in muscles that flex the right forearm. But in the present tests for contraction in the muscles of speech, these three instructions afford negative results.

Following each of the objective tests here described, reports were secured in detail from 3 *S*s who had been trained to introspect. They agreed that during mental activities involving words or numbers, they feel tenseness in the tongue and lips as in saying those words or numbers, except that the feeling is slighter and more fleeting. These reports conform with our objective findings.

CONCLUSIONS

(1) When the *S*, lying relaxed with eyelids closed, engages in mental activity such as imagination or recollection, contraction

(commonly slight and fleeting) occurs in specific muscles. Evidence is thus afforded that the physiology of mental activity is not confined to closed circuits within the brain but that muscular regions participate.

(2) During visual imagination or recollection, the muscles that move the eyes contract, as if *S* were looking at the imagined object.

(3) During what psychologists term 'inner speech,' muscles in the tongue and lips contract as if to say the words in swift and abbreviated manner.

(4) During imagination or recollection of muscular acts or of matters that involve such an act on the part of the *S*, contraction occurs in some of the muscle-fibers which would engage in the actual performance of the act. Exceptions are noted when visualization occurs alone which is characteristic of some *S*s.

(5) During a particular mental activity, the muscles of a quietly lying *S*, trained to relax, remain as a rule inactive, excepting those specifically engaged as above stated.

(6) Electrical records along with subjective reports indicate that during general progressive muscular relaxation, imagery and thinking processes dwindle and disappear.

(7) Relaxation of the specific muscular contractions present during a particular mental activity brings about the disappearance of that activity. This is accomplished by trained *S*s in periods sometimes varying from about 0.2 to 1.5 sec. as measured by action-potentials.

(8) The action-potentials measured in the present investigations are proved to be very different from the galvanic reflex.

(9) A method of measuring neuromuscular processes in mental activities is presented, and the possibility of the development of a branch of study analogous with physical chemistry, and which may appropriately be called physical psychology, is herewith opened.

THE INFLUENCE OF DEGREE OF INTERPOLATED LEARNING UPON RETROACTIVE INHIBITION

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One of the functional relationships which it is important to establish in the field of retroactive inhibition is that between degree of learning and amount of inhibitory effect. It is desirable to know, not only what this relation is in cases where inhibition appears, but also whether there are degrees of learning which place the material learned beyond susceptibility to inhibition. In an experimental attack upon this problem the originally learned material constitutes one aspect with respect to which degree of learning may be varied, and the interpolated material constitutes another. The general problem, which is, experimentally, a clean cut and simple one, nevertheless raises some far reaching questions which bear upon the interpretation of results in the whole field of retroactive inhibition.

It was found in a previous study that the relative amount of retroactive inhibition varied inversely as the number of presentations given the original lists.[1] The frequency of repetition of the interpolated nonsense syllable lists was constant at 11 repetitions, while the frequencies of the original lists varied in increments of five from 6 to 26. Even with a number of presentations as high as 26, however, with the consequent overlearning, the amount of the inhibition was, in terms of recall score, 46.2%, and in terms of saving score it was 5.3%. It is evident that a high degree of overlearning does not free lists of nonsense syllables from the disintegrating effects of interpolated lists. These results at once prepare the way for a study of the effect of a variation of the degree of learning of the interpolated instead of the original lists.

The problem of the present study is to measure the influence of varying degrees of interpolated learning upon amount of retroactive inhibition, under conditions comparable to those obtaining in the previous study, when the original lists are given a constant frequency of presentation. In this experiment, as in the previous one, the methodological assumption has been made that degree of learning will increase with increasing frequency of presentation.

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[1] J. A. McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262.

EXPERIMENT I

Method. The same lists of 9 nonsense syllables used in the preceding experiment were employed here. They were exposed at a 2-sec. rate on an electrically driven drum and were learned by the anticipation method, the *S*s being asked to anticipate the syllables by spelling instead of by pronouncing them. Only 8 syllables were learned since, in the anticipation method, the initial syllable is a cue only and is not learned. All *S*s were given practice series on 3 days on each of which they learned a list of 0% syllables,[2] that were much more difficult than the mixed lists of the experiment proper.

The original lists were given a constant frequency of 11 repetitions, which is the same as that given the interpolated lists in the preceding experiment. The interpolated lists were given 6, 11, 16, 21, and 26 presentations, which are the numbers previously employed with the original lists. Thus, six experimental sittings were required: one rest condition and a work condition at each of the five frequencies listed.

Under the rest condition *S* was given 11 repetitions, followed by a 10-min. period of non-learning activity during which he was instructed to read silently from a book of humor, marking the three best jokes out of every ten.[3] All of the *S*s were inexperienced and would have found it difficult to maintain unaided a rest period without rehearsal, and this method was used to insure as much as possible against rehearsal. It is thought, from all the evidence available, that this method is effective. At the end of the 10-min. interval the list was relearned to 3 perfect repetitions in succession.

Under the work condition *S* was given 11 presentations, followed by a 30-sec. interval during which a second list was prepared for exposure. Immediately following this interval *S* was given either 6, 11, 16, 21, or 26 repetitions of an interpolated list. The remainder of the 10-min. period, not occupied by the interpolated presentations, was filled as under the rest condition. At the conclusion of the 10-min. interval the original list was relearned to a criterion of 3 perfect trials in succession, and the interpolated list was given one relearning presentation in order to guard against any suspicion on the part of *S* that this list was less important than the original list and hence to be studied less vigorously.

Subjects. Twelve college students, 5 men and 7 women, went through two complete cycles. That is, they went through the rest and the 5 work conditions twice, in an order which counterbalanced practice effects and positions of syllable lists. External conditions were kept at a high degree of constancy throughout.

Scoring. The data have been scored in the following ways. (1) The total number of correct anticipations has been found for both original and interpolated lists, and these numbers have been used as checks upon degree of learning. (2) Recall scores have been found in terms of the number of syllables correctly anticipated at the first relearning trial. Similar scores in terms of the second relearning trial have also been computed, but little use will be made of them. (3) The numbers of trials required for relearning to a criterion of one perfect trial and to the more rigorous criterion of three perfect trials in succession have been found and from them saving scores have been derived. (4) Relative retroactive inhibition has been stated, for both recall and saving scores, in terms of the formula: (R−W)/R, in which R = the rest score and W = the work score.

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[2] J. A. Glaze, The association value of non-sense syllables, *J. Genet. Psychol.*, 35, 1928, 255-269.

[3] It was necessary to use a 10-min. interval, instead of the 5-min. interval of the previous experiment, in order to give time for as many as 26 interpolated repetitions which, with the time for changing from one list to another, occupied about 9 min. It is unlikely that this difference should significantly affect the results.

Results. The first step necessary in the examination of the results is to inquire whether the constant frequency of 11 repetitions has been accompanied by a corresponding constancy in the number of correct anticipations. It is highly important to ascertain as accurately as possible whether the conditions have actually produced a uniform factor which may fairly be labelled constant degree of learning. The number of correct anticipations may be employed as a control measure. It is clear from Table I that there are no differences which are important in amount or consistent in tendency between the different means within each individual cycle. Differences of four or five correct anticipations between means for 11 trials by every one of the 12 *Ss* are of such little significance that it is unnecessary to present evidence of their statistical unreliability. The means for the second cycle are uniformly higher than are those for the first, thus indicating a clear practice effect from cycle to cycle. Intra-cycle practice effects, however, have been counterbalanced. It may fairly be assumed, then, that differences between the amounts of retroactive inhibition produced under the different conditions of interpolation are not influenced to any important extent by inequalities in the learning of the original lists.

TABLE I

MEAN NUMBERS OF CORRECT ANTICIPATIONS

(Original lists)

		Interpolated frequencies*				
	Rest	6	11	16	21	26
Cycle I	41.7	44.8	40.5	45.4	46.0	43.4
Cycle II	49.4	50.5	46.1	48.7	53.1	49.4

*The number of records included in the means is 12 for each cycle.

Recall scores in terms of the mean number of syllables correctly recalled (anticipated) on the first relearning trial, and the inhibition in recall, are given in Table II. The two cycles agree closely and only the relative retroactive inhibition in terms of total scores need be considered. Every interpolated frequency produces a large percentage of inhibition, but the 6-repetition condition yields from 19–26% less than does any of the others. The four other work conditions vary unreliably among themselves and must be considered equal.[4] The numbers of anticipations on the second relearning trial agree in tendency with the recall scores just discussed, although, as is to be expected, the amounts of inhibition are smaller at the second trial. They are, for the five degrees of interpolated learning, 31.3, 54.9, 46.4, 42.4, and 42.4.

TABLE II

RECALL SCORES AND RETROACTIVE INHIBITION
(Original lists)

		Interpolated frequencies				
	Rest	6	11	16	21	26
Cycle I	4.00	1.16	.75	.50	1.00	.66
Cycle II	5.33	2.66	.58	.91	1.00	.83
Both Cycles	4.66	1.91	.67	.71	1.00	.75
σ_M	.40	.34	.17	.17	.21	.18
% Retroaction		59.0	85.6	84.7	78.5	83.9

It may be concluded, therefore, that 6 repetitions of an interpolated list produce a degree of retroactive inhibition which, though large, is less than that yielded by numbers of repetitions from 11 to 26. Frequencies of 11, 16, 21, and 26 produce virtually equal amounts of inhibition. When interpolated learning is slight, it appears that retroaction is a function of degree of learning, but that as complete mastery is approached and passed, additional increments have no observable effect.

The results from relearning are, in implication, similar to those from recall. The data for relearning to a criterion of 3 perfect trials are the more significant (Table III). Again the two cycles are in essential agreement and there is a practice effect from cycle to cycle which is rather large in amount. Roughly half of the original trials are saved at relearning after rest, but far less than half after the work conditions. When the original lists have a frequency of 11, any frequency of interpolated learning from 6–26 has a strikingly inhibitory effect. Six repetitions produce less inhibition than do 11, 16, or 21 repetitions, which are virtually equal in effect, and than 26 repetitions, which yield a degree of retroaction midway between that produced by 6 and by 11, 16, and 21. This decrease in retroaction from 21–26 repetitions cannot be considered significant in view of the fact that the difference between the two means concerned is 0.84 ± 2.29.[5] It must be concluded that, although amount of inhibition is a function of the method of measuring retention, the influence of varying degrees of interpolated learning is in tendency the same for both recall and relearning. That is, the four higher frequencies yield nearly equal amounts of inhibition, which are less than that following 6 repetitions. The data for relearning to a criterion of only one perfect trial corroborates this conclusion.

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[4] It can be determined by inspection that no difference between the means for these four conditions is reliable. The difference of 2.75 between the means for rest and the 6-repetition condition has a sigma of ±0.52, however. The differences between the rest condition and the other work conditions are still more reliable.

TABLE III

Relearning Scores and Retroactive Inhibition

(Original lists)

		Interpolated frequencies				
	Rest	6	11	16	21	26
RL Trials, Cycle I	6.16	10.08	11.16	11.16	11.00	10.91
RL Trials, Cycle II	4.50	5.58	7.75	7.33	8.08	6.50
Both Cycles	5.33	7.83	9.46	9.25	9.54	8.70
σ_M	1.01	1.28	1.18	1.22	1.73	1.50
Saving (Both Cycles)	51.5	28.8	14.0	15.9	13.2	20.9
% Retroaction		44.1	72.8	69.1	74.3	59.4

It is necessary to inquire whether the increasing frequency of presentation of the interpolated lists has been accompanied by a corresponding increase in the number of correct anticipations. The latter may again be taken as a second measure of degree of learning and may be employed as a check upon frequency. It is evident from Table IV that the number of correct anticipations increases con-

TABLE IV

Mean Correct Anticipations

(Interpolated lists)

	Interpolated frequencies				
	6	11	16	21	26
Cycle I	15.6	42.4	75.4	114.5	144.6
Cycle II	16.4	48.0	84.6	117.8	157.7

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[5] None of the differences between the means of the relearning trials under the work conditions is statistically significant; and no difference between the rest and any of the work conditions is as much as three times its sigma. In the latter case, however, it may safely be assumed that the consistency with which the means for the work conditions exceed that for the rest condition assures the validity of the tendency.

sistently and almost linearly as frequency increases from 6–26 in each cycle. One may, therefore, conclude with the more assurance regarding the influence of degree of learning, since two independent quantifications of it agree.

For comparison, the percentages of retroaction found in this and in the preceding study of degree of learning and retroaction are reproduced in Table V. It is clear from a comparison of these results that the influence of a variation in frequency is a function of the particular list in the retroaction set-up to which it is applied. When a constant frequency of 11 presentations is applied to the interpolated lists and the frequency of the original lists varied as in the interpolated learning of the present experiment, retroactive inhibition decreases consistently as frequency increases. In the present experiment, however, it increases only from 6 to 11 repetitions of the interpolated material and remains virtually constant thereafter. Thus, increasing degrees of learning of the original lists free them progressively from the disintegrating effects of a constant interpolation, while an increasing degree of interpolated learning, beyond 6 repetitions, neither enhances nor diminishes the susceptibility of the original lists when learned to a constant degree. It is particularly noteworthy that the retroaction in terms of relearning remains high throughout in this case, while falling sharply in the former.

TABLE V

COMPARISON OF THE PERCENTAGES OF RETROACTIVE INHIBITION WHEN THE FREQUENCY OF REPETITION OF ORIGINAL AND OF INTERPOLATED LISTS IS VARIED

	Frequency	Number of repetitions				
	varied in	6	11	16	21	26
Recall	Original	82.2	78.4	64.8	61.5	46.2
	Interpolated	59.0	85.6	84.7	78.5	83.9
Relearning	Original	108.5	23.5	13.0	10.5	5.3
	Interpolated	44.1	72.8	69.1	74.3	59.4

Another important factor which apparently affects amount of retroaction is the amount of practice at the experimental conditions under which, in any given case, the inhibition is being measured.[6]

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[6] The appearance of a practice effect whereby inhibition decreases with repetition of the experimental conditions has been reported before. See, for example E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, (no. 128), 1-57; The 'similarity' factor in retroaction, this JOURNAL, 39, 1927, 297-312; and McGeoch, *op. cit.*

This factor may be examined in the present experiment by comparing the amount of retroactive inhibition obtained in the two cycles separately. This comparison is shown in Table VI. In terms of recall score, retroaction decreases markedly from the first to the second cycle only at the 6-repetition condition. At the other frequencies there is no marked or consistent change. In terms of relearning, however, there is a strikingly large decrease in retroaction at the second cycle. In percentage terms the decrease runs roughly between 50 and 68. This marked decrease in inhibition at the second cycle occurs in spite of the somewhat larger numbers of correct anticipations at this cycle, as shown in Table IV. It is worth noting that, in the second cycle, there is considerably less inhibition in relearning after an interpolation of 26 repetitions than after any of the other frequencies above 6. The difference concerned is not statistically reliable, but it suggests that, if practice were carried sufficiently far, there might be, parallel with a general decrease in susceptibility to retroactive inhibition, an inverse relation between frequency and inhibition, at least at the higher frequencies.

TABLE VI

Retroactive Inhibition by Cycles

		Number of repetitions				
		6	11	16	21	26
Recall	Cycle I	71.0	81.2	87.5	75.0	83.5
	Cycle II	50.1	89.1	82.9	81.2	84.4
Relearning	Cycle I	81.1	103.1	103.1	100.0	98.6
	Cycle II	16.6	50.0	43.5	55.0	30.6

The influence of practice is, in this case at least, a function of the method by which retention is measured, the influence being almost absent in the case of recall and very marked in relearning. It is, also, a function of the list the frequency of which is varied. In the previous experiment, in which the frequency of the original lists was varied, a clear decrease in retroaction occurred from the first to the second cycle, when retroaction was measured in terms of recall, while almost no decrease appeared when relearning was the measure.

Experiment II

Method. In this experiment the method of complete presentation has been employed with 10 nonsense syllables constructed proportionately from the o, 6.66 and 13.33 % meaningful divisions of the Glaze list. The original lists were studied for 120 sec., while the time devoted to studying the interpolated lists was varied in steps of 30 sec. from 60 to 180 sec.

Under the rest condition the *Ss* studied a list for 120 sec., were then given 45 sec. in which to write a recall, and were then allowed a period of 5 min. of no learning activity or rest. During this rest period they were asked to read silently from a humorous magazine, marking on each page the two jokes which were considered best. The *Ss* considered the marking of the jokes a second part of the experiment, and their behavior while reading and marking, together with their reports at the end of the experiment, indicate that rehearsal was at a minimum. At the end of the 5 min. period a second recall was taken, for which 45 sec. were allowed.

Under the work condition the *Ss* studied a list for 120 sec., were then given 45 sec. in which to write a recall, and immediately thereafter were given a second list to learn. Varying times—60, 90, 120, 150, and 180 sec.—were devoted to this interpolated learning, making 5 sub-conditions of work. At the end of the interpolated study, 45 sec. were devoted to a recall of the list studied, followed by the reading of jokes for the remainder of the 5 min. At the end of this time a second recall of the original list was taken; and then a second recall of the interpolated list in order to prevent a suspicion that it was less important than the original list and hence less to be studied.

The 45 sec. period permitted for the recalls under all conditions gave ample time for virtually all *Ss*. The necessary lists, face downward and numbered, with paper for the writing of the recalls, were given to every *S* at the beginning, and the time schedule of the experiment was followed uniformly and smoothly.

Subjects. Forty-two college students, in groups of three, went through each of the six conditions—one rest and five work conditions—of the experiment in an order which counterbalanced practice effects. Every *S* had, of course, an individual copy of the lists and of the magazines used, and was seated far enough from every other *S* to prevent the giving or receiving of assistance.

Scoring. The results were scored in two ways: (1) by counting each syllable correctly recalled and in the right position as one (the *Ss* were instructed to learn and recall the syllables in order); and (2) by Lyon's method which assigns partial credits in terms of both syllable and position.[7] It would be inaccurate to compute the amount of retroactive inhibition directly in terms of the numbers of syllables recalled after the interval, since this would take no account of the number originally learned. Hence the measure of retention used is the percentage which the number recalled after 5 min. is of the number recalled immediately. The percentage of retroactive inhibition is, then, the difference between the percentages of retention under rest and work divided by the percentage under rest (rest minus work over rest).

Results. The data are summarized in Table VII. Recall 1 refers to the mean numbers of syllables recalled immediately after the learning of the original lists, and Recall 2 to the mean numbers recalled after the 5-min. interval. The scores in the Recall 1 column are, thus, measures of the degree of learning of the original lists under a constant learning time of 120 sec. They fluctuate somewhat, but hardly over a sufficient range to vitiate the conclusions to be drawn regarding amount of retroactive inhibition.[8]

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[7] D. O. Lyon, A rapid and accurate method of scoring nonsense syllables, this JOURNAL, 24, 1913, 525-531.

TABLE VII

RECALL SCORES AND RETROACTIVE INHIBITION

Condition	Recall 1		Recall 2		% Recall	% Retroaction
	Av.	σ	Av.	σ		
Rest	5.404	2.25	4.238	2.32	78.42	
Work— 60 sec.	4.928	2.22	1.857	2.02	37.68	51.95
Work— 90 sec.	5.738	2.31	1.976	2.47	34.43	56.09
Work—120 sec.	4.619	2.12	1.309	1.77	28.33	63.87
Work—150 sec.	4.976	2.83	1.309	1.73	26.31	66.44
Work—180 sec.	5.238	2.27	1.738	1.80	33.18	57.68

The Recall 2 under the rest condition is clearly much greater than under any of the work conditions and this superiority is reliable statistically.[9] The work conditions do not differ by any very great amounts among themselves. The outstanding feature of the recalls under work is that the percentages of recall decrease steadily and, correspondingly, retroactive inhibition increases, from 60–150 sec. of interpolated learning. The differences involved are not reliable statistically, but the consistency of the tendency argues for the conclusion that retroaction tends to increase slowly with increase in time spent in interpolated learning from 60–150 sec. The decrease in inhibition from 150–180 sec. of interpolated learning cannot be considered established statistically. As it stands, however, it suggests the possibility that amounts of time beyond 150 sec. devoted to interpolated learning might be accompanied by decreasing amounts of retroaction. This suggestion is reënforced by the fact that, in Experiment I, there was in terms of relearning a likewise small though unreliable drop from 21 to 26 repetitions.

In order to test the possibility just mentioned, a check experiment was performed, employing the same experimental procedure and the same lists with 15 *Ss*. A rest and two work conditions were used, the latter with interpolated learning times of 150 and 240 sec. An experiment with three conditions does not exactly duplicate on a smaller scale one with six conditions, on account of the smaller opportunity for such inter-list effects as practice and negative transfer to occur. It should, however, yield tendencies which are comparable. The check experiment gives percentages of recall as follows: rest, 80.00; work, 150 sec., 28.30; and work, 240 sec., 34.61. The percentages of inhibition under the two work conditions are 64.63 and 56.74. The differences concerned are statistically unreliable, but the tendency is the same as that found in the major experiment. These results cannot be taken as final with regard to the magnitude of the decrease in retroaction as the time given to interpolated learning increases beyond 150 sec., but they are important as corroborating the fact of the decline.

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[8] The results yielded by Lyon's method of scoring agree closely with those given and are not, therefore, reproduced.

[9] The difference between Recall 2 under rest and the largest corresponding score under work (at 90 sec.) is $2.262 \mp .52$. The other differences, as can be seen by inspection, are still more reliable.

It remains to inquire whether the changes in amount of inhibition have been accompanied by corresponding variations in the learning of the interpolated lists. One would expect the immediate recalls of these lists to increase as the time given to study increases. This is not the case. The means for the five study times are, in order: 4.595, 4.428, 4.571, 5.952, and 5.523. The two last, after 150 and 180 sec. of study are higher than the others, but there is a definite lack of uniformity of increase.[10] Clearly, immediate recall does not vary consistently with learning time under complete presentation, although retroaction does vary consistently from 60 to 150 sec. of interpolated learning. Evidently factors not measured by immediate recall are involved.

PROBLEMS INVOLVED IN MEASURING DEGREE OF LEARNING

In this paper and in the preceding one on the same general problem, degree of learning has been quantitatively controlled for experimental purposes in terms of frequency of repetition, and a second measure has been used as a check upon the methodological assumption that frequency of repetition may be used to define degree of learning. This second measure is the number of correct anticipations at each frequency. In the former experiment and in this one there has been an almost linear relation between frequency values and numbers of correct anticipations, which serves in one sense to corroborate the assumption mentioned.

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[10] The slightly smaller recall after 180 sec. of interpolated learning can not certainly be looked to for an explanation of the smaller retroaction at the latter condition on account of the general irregularity of the recalls after the other study times and the corresponding regularity of the increase of retroactive inhibition notwithstanding; although the fact that the immediate recalls in the check experiment were 5.866 after 150 sec. of study and 5.333 after 240 sec. points in that direction.

When time spent in study has been used as a criterion, experimentally, of degree of learning, as in Experiment II, it has been checked by computing the number of syllables which could be recalled immediately. Interestingly enough, this by no means bears a linear relationship to time, though, even when the number recalled does not increase, degree of retroactive inhibition may. Time and number recalled are evidently different matters.

It should be pointed out that the methods used for specifying degree of learning by no means exhaust the problem; and it may also be noted that even in the apparent simplicity of the experimental procedure employed there are variables still uncontrolled, whose influence, if it exists, is entirely unknown. The latter difficulty will be discussed first and its discussion will indicate some of the other possible ways of specifying degree of learning.

(1) It must inevitably happen that, when frequency of presentation is varied, time spent in learning varies also. It is possible that a given number of correct anticipations (or any other similar criterion) might be arrived at by different *S*s after widely different numbers of trials, and hence after differing times; and it is thinkable that the variable time factor might, in itself, lead, in spite of a constant number of correct anticipations, to variable results. In these experiments, in which frequency has been varied from 6–26 repetitions and time from 60–240 sec., it may be that the results are a function of mere time spent in effort as well as of other presumably more important factors. One should measure also the influence upon retroaction of varying degrees of learning in equal amounts of time and of equal degrees of learning in varying amounts of time.[11]

(2) It is possible that, under conditions which yield imperfect learning, the percentage relation between amount learned and amount to be learned may be a factor. This is somewhat taken care of in these experiments by the fact of the linear relationship between frequency and number of correct anticipations, when the latter are plotted against the former, although its influence, if present, is unknown. In an experiment which produced roughly the same mean numbers of correct anticipations, but employed, for example, longer lists, a different result might be obtained on account of the different proportion between number of correct anticipations and total items in the list. This factor of ratio is in some sense a measure of the integration of the total list and may well be significant.

(3) Measurement, however, in terms of numbers of correct anticipations is at best rough in that it masks the distribution of the correct anticipations over the list. Thus, 40 correct anticipations of an 8-syllable list in 11 repetitions may mean 10 anticipations each of 4 syllables, 5 each of 8, or many other distributions. Quite possibly, different distributions may have different values for retention and for retroaction.

(4) It is a further possibility that the part of the learning period in which an item is first acquired (here, first correctly anticipated) may be a factor. This would be a function of the homogeneity of the list, of serial position, and of many other factors.

(5) It has already been noted that time may be a factor which operates in experiments upon degree of learning, and that the ratio between number of correct anticipations (or amount learned, in whatever terms) and the total

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[11] To some extent Experiment II does the latter, since varying times spent in interpolated learning yield in some cases not very different immediate recalls.

number of items to be learned may be an important matter. Still another variable should be added, in terms of which the general problem of this paper might be attacked, viz. the criterion to which learning is carried, such as number of correct anticipations per trial, number correctly recalled immediately, a given distribution of correct anticipations over the items of the list, or number of completely perfect repetitions.

That the averaged data have produced results of a moderately high degree of uniformity is a fact—and a surprising one. It may mean that the factors mentioned are influential only in stimulating over-cautious reflections in the experimenter; or it may mean that these factors are not influential enough to dispel a uniformity which might have been much greater in their absence; or it may mean that some of them, at least, are highly correlated with frequency and hence not separable variables in this experiment. It does not mean, probably, that they can be disregarded in interpreting experimental set-ups and results.

From all of the considerations adduced, it should be apparent that the general problem of degree of learning, and the particular problem of the influence of this variable upon retroaction, have but been scratched experimentally. Within the limits of the conditions of these experiments the results are supposedly valid, but these conditions are few among many and themselves require analytic attack. Degree of learning is certainly not a unitary condition which one measures or does not measure. Rather there are many kinds of 'degree of learning.' These considerations reach beyond the present problem almost to the whole field of learning and retention. They are, moreover, of the first importance in work on retroactive inhibition, underlying as they do so much of the methodology for the study of the influence of other factors upon retroaction.[12]

SUMMARY

When nonsense syllable lists are given a constant frequency of 11 repetitions, followed by the varying interpolations, rest, and 6, 11, 16, 21 and 26 repetitions of a second list, and then recalled and relearned after an interval of 10 min., the following results appear.

(1) The constant frequency of repetition of the original lists is accompanied by a virtually constant number of correct anticipations; and the increase in the frequency of the interpolated lists from 6 to 26 is paralleled by a linear increase in the number of correct anticipations.

(2) When retention is measured in terms of recall, six repetitions of an interpolated list produce a degree of retroactive inhibition which, though large, is less than that yielded by numbers of repetitions from 11 to 26. Frequencies of 11, 16, 21, and 26 produce virtually equal amounts of inhibition.

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[12] In this paper the question whether frequency in and of itself promotes learning is left untouched. For the present purpose it matters little whether the frequencies employed or their correlated factors produce the learning.

(3) When retention is measured in terms of relearning, six repetitions of the interpolated list produce less retroaction than do 11, 16, or 21 repetitions, which are virtually equal in effect, and than 26 repetitions which yield a degree of inhibition midway between that produced by 6 and by 11, 16, and 21. On account of the unreliability of the differences, it must be concluded, however, that the four higher frequencies are nearly equal in effect.

(4) Although absolute amounts (percentages) of retroaction are higher for recall than for relearning, the percentages for both agree in tendency. That is, the four higher frequencies yield virtually equal amounts of inhibition which are less than that following 6 repetitions. Beyond a certain point which lies between 6 and 11 repetitions, additional increments have no additional inhibitory effect.

(5) A comparison of the relative amounts of retroaction obtained in this and in the former experiment indicate that retroaction is also a function of the particular list, original or interpolated, to which the variable degrees of learning are applied.

(6) The influence of practice at the experimental conditions under which retroaction is being measured is a function of the method of measuring retention and of the list the frequency of which is varied. When the frequency of the interpolated list is varied, inhibition in terms of recall shows no effect, while that in terms of relearning decreases strikingly from cycle to cycle. When the frequency of the original list is varied, recall shows a practice effect while relearning does not.

When 10-syllable lists, by the method of complete presentation, are studied for 120 sec., followed by varying interpolations, rest, and 60, 90, 120, 150, and 180 sec. of study of an interpolated list, the following results appear.

(7) Retroactive inhibition increases gradually and consistently from the 60 sec. to the 150 sec. condition and falls to 180 sec. A check experiment indicates that inhibition is less after 240 sec. of interpolated study than after 150 sec. of such study. The fact that the numbers of interpolated syllables immediately recalled are less after 180 and 240 sec. than after 150 sec. makes this decrease in inhibition hard to interpret. It can be said, however, that there is a suggestion of a decrease in retroaction beyond 150 sec. of interpolated study, but the amount of this decrease must be very small.

(8) It is significant that, throughout, retroaction is more than 50% in amount and that neither by relatively small (60 sec.) nor by relatively large (240 sec.) amounts of interpolated study can anything like complete freedom from retroactive inhibition be obtained under these experimental conditions.

The influence of degree of learning on retroactive inhibition is, thus, a function of the general method of learning, whether anticipation or complete presentation, and perhaps of other smaller differences between the two experiments reported. The experiments agree fully, however, in showing that neither very small nor very large degrees of interpolated learning, as measured, free original lists, as learned, from the disintegrating influence of interpolated acquisitions. They agree, also, in finding that a relatively low degree of interpolated learning (*e.g.* 6 repetitions and 60 and 90 sec.) produces a somewhat smaller retroaction than do relatively higher degrees of learning which are close together in the magnitude of their effects.

A discussion of the possible factors involved in the conception of 'degree of learning' indicates that it is not a unitary condition, but that there are several kinds of 'degree of learning,' all of which must be taken into account in experiments on retroaction and on retention in general.

PRESSURE ADAPTATION-TIME: A FUNCTION OF INTENSITY AND EXTENSITY

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The topic of cutaneous adaptation is treated in the most general way in handbooks of sense-physiology and psychology. Thunberg described Locke's classical experiment on thermal adaptation but presented no specific experimental data on adaptation to pain and pressure.[1] Titchener merely pointed out certain kinds of stimuli which may be used to demonstrate the fact of cutaneous adaptation.[2] In his survey of the experimental situation in the field of touch in 1929, Nafe treated this topic in a very general way without presenting experimental results.[3] Troland has disposed of tactual adaptation in one brief paragraph,[4] the generalizations of which are drawn from the survey of Thunberg in 1905, just cited. The fact of the matter is that extremely little experimental work has been done on cutaneous adaptation. There are a few recent studies, such as that of Straus and Uhlmann,[5] who first demonstrated experimentally the fact of algesic adaptation; that of Adrian,[6] in which he reported that the adaptation-time to painful stimulation is longer than that to non-painful stimulation; and those of Hahn,[7] which deal with the nature of thermal adaptation.

It is obvious that adaptation can be understood as the establishment of an equilibrium by the receptor, and that we have thus to

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[1] T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen*, III, 1905, 647-733.

[2] E. B. Titchener, *Experimental Psychology, Instructor's Qualitative Manual*, 1901, 81.

[3] J. P. Nafe, The sense of feeling, *Foundations of Experimental Psychology*, 1929, 392-413.

[4] L. T. Troland, *Psychophysiology*, II, 1930, 317.

[5] H. H. Straus and R. F. Uhlmann, Adaptation to superficial pain, this JOURNAL, 30, 1919, 422-424.

[6] E. D. Adrian, The impulses produced by sensory nerve endings, *J. Physiol.*, 61, 1926, 49-72.

[7] H. Hahn, Neue Anschauungen vom Temperatursinn, *Dtsch. Med. Woch.*, 53, 1927, 528-530; Über den Erregungsvorgang der Temperaturnerven, *Arch. f.d. ges. Psychol.*, 65, 1928, 41-54; Die psychophysischen Konstanten und Variablen des Temperatursinnes, *Zsch. f. Sinnesphysiol.*, 60, 1929, 198-232.

consider both adaptation during the continued stimulation and the reëstablishment of equilibrium after the stimulation ceases. These two phases of adaptation may be designated respectively, *stimulatory* and *post-stimulatory* adaptation.

In the experiments to be described here, an attempt was made to determine (1) whether the adaptation-time to continued cutaneous stimulation varies with the intensity and extensity of stimulation, and (2) whether the adaptation-times for continued stimulation and for the reëstablishment of normal equilibrium afterward are essentially of the same magnitude or different. Experiment I deals with the first problem, and Experiment II has to do with the second.

EXPERIMENT I

After preliminary trials with cork stimuli, we constructed an intensity-set of five stimuli, and an extensity-set of the same number of stimuli. All were made of composition metal. All members of the intensity-set had the same diameter, 1 cm., and weighed respectively 50, 100, 500, 1000 and 2000 mgrm. All members of the extensity set weighed the same, 500 mgrm., and measured respectively 5, 10, 15, 20 and 25 mm. in diam. These cylindrical weights were applied to the skin manually by short threads which were fastened at the edges of the weight and drawn together above so as to insure uniform manual application. In order to eliminate thermal stimulation, in the case of the intensity-set, a small disk of paper was placed on the skin long enough for complete adaptation to take place before the weight was placed upon it. In the work on extent, the *E* held the set of weights on one hand so as to keep them warm. A haphazard order of presentation was followed in both cases.

The trials with the intensity-stimuli were made in four regions: (1) the back of the hand between the second and third metacarpals, (2) about 3 in. above the carpal folds, on the volar surface of the forearm, (3) the middle of the forehead, and (4) the cheek. The trials with the extensity-set of stimuli were presented in two regions: (1) the center of the forehead and (2) the cheek. For the trials on the hand and forearm, *O* sat at a low table, but for the work on the forehead and cheek he took a supine position. At no time was *O* permitted to see the weights.

At the moment when a stimulus was applied to the skin, a stop-watch was started so as to measure the interval of time between the application and the time when *O* indicated verbally that the stimulus was no longer felt.

The *Os* analyzed the period of adaptation into three phases. First, there is the brief period of application, which consists of light impact over a field of pressure of quite clearly defined outline. This is followed by a period in which the outline grows vague and dim and the field of pressure undergoes diminution in size until only a small point of pressure remains. During this second phase *O* reports regularly that it appears as if the stimulus were gradually being lifted from the skin. The third phase begins when *O* experiences uncertainty as to whether stimulation is still perceptible. The *Os* generally indicated that adaptation does not terminate suddenly, but gradually within an interval of time in which uncertainty regarding continuation of stimulation prevails. With training *O* establishes a subjective standard by which he is willing to report when stimulation ceases.

Table I contains the average adaptation-times (A.T.) in seconds with mean variations (M.V.) for eight *O*s in the four regions of stimulation by the intensity-series of stimuli. Table II includes the same data for stimulations in two regions with the extensity series of stimuli. The averages in the table for intensity are based on 24 trials in the case of all *O*s, except Z, who furnished 30. Those in the other table are in every case computed from 12 sets of trials.

Inspection of Table I reveals the fact that adaptation-times to stimulation by heavier weights tend to be longer than those to stimulation by lighter weights. It may also be observed that the duration varies with the relative intensity of stimulation. Although inversions appear in certain series of averages, they tend to be insignificant. The tendency for adaptation-time to vary with intensity of stimulation is manifested more or less clearly in the averages of every *O*. The inversions which appear in the series of certain *O*s are leveled out completely when the averages of all *O*s are combined.

Table II indicates a tendency in the opposite direction, namely, for adaptation-time to vary inversely with the area of stimulation. There are several inversions here again, but the tendency may nevertheless be seen, and in the combined averages it is clearly evident. The averages in this table do not show quite the width of distribution generally manifested in Table I. It may be that our steps were too small, for the *O*s reported that differences in size are less clearly observable than those of intensity. This probably also explains the fact that relatively a larger number of inversions appear in Table II than in Table I.

It may be pointed out that such variation in extensity can be resolved in reality to a special case of variation in intensity. With the weight of largest diameter, for example, 500 mgrm. are distributed over relatively a larger field than with any of the other weights, where the same amount of weight is supported by a smaller portion of the skin.

There is little justification for asserting an exact functional relation between adaptation-time and intensity or extent. There is a possibility that a linear relation is masked by a large variability. This variability probably signifies that the judgments were subject to a number of such variable errors, as temporary bodily condition,

TABLE I

ADAPTATION-TIMES (SEC.) FOR PRESSURE WHEN THE INTENSITY OF THE STIMULUS IS VARIED AND THE EXTENSITY IS CONSTANT

	Stimulus	Observers																Combined
		B		*D′*		*E*		*G*		*M*		*P*		*W*		*Z*		
	(mgrm.)	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	Av.
Back of Hand	50	1.5	1.0	.8	.4	2.0	1.1	2.2	.2	4.2	1.6	1.0	.4	.7	.4	7.0	2.5	2.42
	100	2.2	.8	1.0	.6	2.8	1.6	3.0	.5	6.3	1.8	2.5	1.2	1.6	.8	10.7	4.1	3.82
	500	4.3	1.5	1.2	.8	4.3	1.7	6.1	1.2	9.3	1.8	5.9	2.5	4.6	1.2	13.0	5.3	6.01
	1000	4.6	1.8	1.2	.8	5.2	1.8	7.0	1.2	9.8	2.4	6.8	2.6	5.5	2.0	13.6	5.2	6.71
	2000	6.5	2.0	1.4	.6	10.2	6.3	11.0	1.5	14.1	2.6	9.9	3.4	5.8	1.6	17.3	5.4	9.52
Forearm	50	2.3	1.4	.9	.3	1.4	.6	.7	.4	4.0	.5	3.1	1.4	.6	.2	5.5	1.3	2.31
	100	3.0	1.5	1.0	.5	2.2	1.1	2.1	1.2	5.8	1.6	4.6	1.3	1.0	.5	6.6	2.4	3.28
	500	4.1	1.8	1.1	.7	4.0	2.1	4.7	1.6	6.7	1.4	7.5	2.3	2.6	1.2	8.2	3.0	4.86
	1000	4.3	1.6	1.4	1.2	4.4	2.2	5.8	2.2	8.8	2.4	8.5	3.0	3.1	1.4	8.5	3.2	5.60
	2000	7.3	2.2	1.6	1.0	6.8	3.3	9.2	2.4	10.8	3.2	11.5	5.4	3.5	1.6	11.4	4.5	7.76
Forehead	50	4.2	2.6	.8	.4	2.2	1.3	10.6	2.4	5.6	3.0	8.9	3.5	1.6	1.2	6.7	1.0	5.07
	100	4.3	2.4	1.1	.6	2.9	1.6	11.1	2.2	7.4	2.2	12.2	5.6	2.5	1.5	8.3	3.4	6.22
	500	8.3	2.7	1.5	1.0	5.7	2.2	20.9	3.4	10.4	3.2	15.9	6.1	4.5	2.5	12.5	4.2	9.96
	1000	9.8	3.6	1.6	1.2	6.7	2.9	24.0	4.5	12.5	3.5	19.1	7.5	6.2	3.4	13.6	5.4	10.43
	2000	13.1	3.5	1.8	1.2	8.9	3.6	35.0	6.0	19.1	2.4	21.8	8.2	8.9	4.5	19.7	6.7	16.03
Cheek	50	4.6	2.3	.8	.4	2.0	1.0	7.5	1.0	5.1	1.7	18.8	3.3	1.3	.7	5.6	2.1	5.71
	100	5.5	3.0	1.1	.8	2.3	1.2	13.1	2.2	7.6	3.2	12.9	3.6	1.5	1.0	7.0	3.2	6.37
	500	11.6	3.4	1.5	.5	5.2	3.0	30.7	4.4	12.7	3.8	17.3	5.4	2.6	1.3	11.5	4.7	11.63
	1000	13.4	3.8	1.6	1.2	5.3	2.9	33.0	6.0	15.8	3.6	24.5	7.0	4.0	2.0	10.5	3.2	13.51
	2000	18.5	4.0	1.8	.8	8.9	5.1	53.2	5.4	23.3	3.7	30.3	10.3	4.7	2.7	14.2	4.5	19.36

TABLE II

ADAPTATION-TIMES (SEC.) FOR PRESSURE WHEN THE EXTENSITY OF THE STIMULUS IS VARIED AND THE INTENSITY IS CONSTANT

	Stimulus (mm. diam.)	Observers B A.T.	B M.V.	E A.T.	E M.V.	H A.T.	H M.V.	M A.T.	M M.V.	N A.T.	N M.V.	R A.T.	R M.V.	Y A.T.	Y M.V.	Z A.T.	Z M.V.	Combined Av.
Forehead	5	15.5	3.7	13.8	2.5	22.5	5.2	16.6	6.8	11.7	1.4	33.0	7.6	13.2	4.2	24.6	5.4	18.86
	10	12.0	4.0	12.2	2.4	17.2	6.8	10.2	3.8	10.0	1.5	32.8	7.3	14.2	3.7	20.5	5.0	16.13
	15	11.5	3.0	7.4	.8	13.5	3.9	15.6	3.6	9.0	2.7	24.2	9.6	16.2	4.5	19.0	4.3	14.55
	20	10.0	3.1	5.0	.6	9.5	4.6	17.2	7.0	7.2	.6	19.8	8.9	14.2	3.3	15.7	5.5	12.32
	25	8.0	2.5	4.0	1.2	7.2	2.0	10.2	5.8	5.5	.7	17.6	6.4	12.7	2.8	13.8	3.7	9.87
Cheek	5	32.3	9.3	18.2	3.1	17.4	2.4	24.8	9.7	17.7	3.7	24.2	5.0	13.4	2.9	22.0	7.3	21.25
	10	20.0	6.5	12.0	3.5	13.1	2.4	16.8	9.6	15.2	2.2	19.0	7.6	15.0	4.7	20.5	6.4	16.45
	15	16.2	3.9	8.4	1.6	11.6	2.6	15.8	9.3	13.5	2.6	18.8	6.2	16.0	4.7	15.0	4.5	14.41
	20	12.8	4.4	5.8	.6	8.0	3.0	9.0	5.6	11.8	2.4	22.4	7.0	16.0	3.0	12.0	2.0	12.22
	25	7.6	2.4	3.8	.9	5.6	.8	7.6	2.4	8.7	2.2	21.4	3.9	15.5	4.5	12.5	3.0	10.33

fatigue, shift in attitude, spontaneous intrusion of other cutaneous experiences, alteration of the subjective standard as to when adaptation is complete.

With regard to the comparative adaptation-times in the different regions of the skin, the results are not conclusive. Four *Os*, *D*, *E*, *W* and *Z* gave adaptation-times of approximately the same magnitude in all regions of stimulation, but the other *Os* reported definitely longer adaptation-times in the face regions. If all cutaneous receptors have about the same duration of adaptation, and certain *Ss* report differences in adaptation times, this may mean that in some *Os* the receptors are overlaid by thicker and tougher integument in certain parts; but if it is eventually to be discovered that the receptors in certain regions have longer adaptation-times, we are then merely in position to conclude that the difference is considerable in some cases, and that in other cases the difference is so small as to be obscured by the variable errors which play a rôle in the experimental situation.

There are marked differences in the adaptation-times of different *Os*. In the same region of the skin, one *O* requires from 8 to 15 times longer for complete adaptation than another. Some *Os* furnish short adaptation-times in all regions, others comparatively long in all regions, and still others comparatively short on the limb and comparatively long on the face.

Upon learning that both of us had started experiments in cutaneous adaptation, Professor John P. Nafe of Clark University and the writer arranged to meet several times to observe in a preliminary way for one another. Although his work had to do with partial adaptation, while ours was concerned with complete adaptation, he found it convenient to set two of his undergraduate students, Thomas Carleton and Francis Singleton, upon the problem of the relationship of intensity and complete adaptation-time. Professor Nafe has kindly turned over their results to me.

Five weights were prepared, consisting of regulation shotgun shells loaded with small lead. They weighed respectively 8, 28, 48, 68 and 88 grm. Carleton and Singleton observed for one another, presenting the weights in haphazard order, and measured the periods of complete adaptation with a stopwatch. Over *O*'s arm, which was bared and placed with volar side up in a comfortable position on a table, projected the arm of an upright stand. In the end of this arm was a circular opening of a diameter slightly larger than that of one of the shells. The stimulus was dropped gently through this opening upon a small cardboard disk, which had been placed upon the skin previous to the trial in order to rule out thermal stimulation and to insure the more uniform distribution of stimulation. Care was taken to make certain that adaptation to the cardboard disk had been complete before a weight was applied.

After preliminary trials, each of the 5 weights was presented 25 times (1) on the volar surface of the arm and (2) on the back of the hand. Only 5 applications of each stimulus were made at each sitting. The position of application was shifted slightly to avoid local fatigue.

The results are presented in Table III. They show in a convincing manner that adaptation-time varies directly with intensity of stimulation. One *O* consistently had shorter adaptation-times than the other. The steps in adaptation-times for each *O*, corresponding to the step increments in intensity, are not quite uniform; nevertheless, there is a rough approximation to equal steps of increase. The combined average of the two regions shows a good approximation to equal steps. This result would seem to suggest the possibility that the same increment of intensity (perhaps at any point on the intensity continuum, other things being kept the same), will require the same amount of additional time for complete adaptation, for there is apparently a linear relationship between intensity and adaptation-time.

TABLE III

ADAPTATION-TIME (SECS.) FOR PRESSURE WHEN THE INTENSITY OF THE STIMULUS IS VARIED

Carleton and Singleton's results

		STIMULUS (grm.)				
	O	8	28	48	68	88
Arm	*C*	3.22	6.97	10.25	12.03	18.86
	S	12.14	13.63	18.62	25.00	26.22
	Av.	7.68	10.30	14.43	18.51	22.54
Hand	*C*	4.26	9.24	16.46	22.21	25.87
	S	5.94	11.26	16.84	28.50	30.71
	Av.	5.10	10.25	16.65	25.35	28.29
	Combined Average	6.39	10.27	15.54	21.98	25.41

EXPERIMENT II

In this experiment we proposed to compare the adaptation-time to continued stimulation with the adaptation-time which follows the removal of a stimulus. If adaptation consists in the establishment of a condition of balance or equilibrium by the receptor, it may be that the phase of adaptation which takes place during the continued application of a stimulus is the complement of that which follows the removal of that stimulus.

To test out this hypothesis, we performed experiments in four regions of the skin, using our 5 intensity-weights (*vide supra*) as stimuli. Under the same experimental conditions which prevailed in the earlier experiments, we required our *Os* now to report (1) the moment, following the application of a weight, when stimulation first ceased to be perceptibly effective (whereupon the weight was promptly removed by *E*), and (2) the moment at which post-stimulatory adaptation was complete, that is to say, when the after-effects attendant upon the removal of the stimulus had disappeared. Time permitted us to present each of the 5 stimuli only 8 times in the case of two *O*s, *E* and *Z*, only 4 times in the case of *W*, and only twice in the case of *G*. For the last named *O* we did not calculate the mean variations.

The averages for both types of adaptation-time, (S. A.) stimulatory and (P.A.) post-stimulatory, are presented in Table IV. The measurements are in seconds.

The averages of 3 of the 4 *O*s indicate that the adaptation-time following the removal of a stimulus from the skin is roughly of the same magnitude as that for adaptation to the continued application of that stimulus. This probably means that stimulatory and post-stimulatory adaptation represent essentially reciprocal changes in the receptor.

G reported much longer post-stimulatory adaptation-times. She may represent a special case in which the two kinds of adaptation require different amounts of time. However, it is possible that her results might have been more like those of the other *O*s, if time had permitted more trials to be made. In fact, there is evidence to support the inference that this *O*, to a degree less manifest in the case of the other *O*s, failed to maintain in this experiment the same attitude that she had in the previous one. At least, if the results for stimulatory adaptation-time which are given in Tables I and IV be compared *O* by *O*, a greater lack of agreement appears in the results of *G* than in those of any other *O*. In view of the fact that *G* is an assistant in the laboratory, the possibility exists that her knowledge that after-sensations follow the removal of a cutaneous stimulus may have predisposed her to report more delayed adaptation-times in this case. It may be noted, furthermore, that in her case the adaptation-times for the different intensities are less scattered, that is to say, they fail to show as definite variation with intensity as do the results of the other *O*s.

The duration of after-sensations (or after-images) of touch have been measured by Spindler,[8] Hayes,[9] and Dimmick,[10] and each of these investigators presents results which exceed considerably our figures for post-stimulatory adaptation-time. Yet, it is clear that the experiences of pressure during post-stimulatory adaptation are genuine after-sensations. But in dealing with the same sensory datum, on the one hand in experiments on after-sensations, and on the other in experiments on adaptation, the situation of the observer

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[8] F. N. Spindler, After-sensations of touch, *Psychol. Rev.*, 4, 1897, 631-640.

[9] M. H. S. Hayes, A study of cutaneous after-sensations, *Psychol. Monog.*, 14, 1912, (no. 60), 1-89.

[10] F. L. Dimmick, On cutaneous after-images, this JOURNAL, 27, 1916, 566-569.

TABLE IV

Adaptation-Times (Sec.) for Pressure when Intensity of Stimulus Is Varied

S.A. = stimulatory adaptation-time or the time for complete adaptation while the stimulus is continued. P.A. = post-stimulatory adaptation-time or the time for the recovery of the original equilibrium after the removal of the stimulus and thus of the fading out of the after-sensation

O	Stim.	Back of Hand				Forearm				Forehead				Cheek			
	(mgrm.)	S.A.	M.V.	P.A.	M.V.	S.A.	M.V.	P.A.	M.V.	S.A.	M.V.	P.A.	M.V.	S.A.	M.V.	P.A.	M.V.
E	50	2.0	.75	3.0	1.3	1.2	.42	1.8	.95	2.5	.75	3.6	2.1	2.6	1.3	3.8	1.2
	100	3.1	1.2	5.0	2.2	3.0	1.5	4.7	2.3	4.1	2.1	9.0	2.2	3.1	1.8	3.7	1.4
	500	5.2	1.6	6.6	3.6	5.1	2.9	6.5	3.8	7.5	3.0	7.0	1.5	6.8	2.8	7.0	3.1
	1000	7.3	2.3	7.6	3.1	7.1	3.6	8.1	2.4	7.5	2.0	6.9	1.9	9.5	5.0	8.1	4.6
	2000	9.5	4.5	11.5	6.0	10.3	3.8	9.3	3.8	9.2	4.0	9.0	3.0	15.0	7.0	10.8	2.7
G	50	2.5		40		4		30		5		27		6		24.5	
	100	11		45.5		11		17.5		5.5		45		10.5		28	
	500	20.5		46		19.5		27.5		17		36		18		31.5	
	1000	21		46.5		11		28.5		30		38		27.5		39.5	
	2000	40.5		38		62		36		45		51		29		41	
W	50	1.2	.35	2.5	1.5	1.0	.25	2.5	1.0	2.5	1.0	4.2	1.7	.75	.15	1.2	.42
	100	1.7	.6	2.7	1.1	1.0	.25	1.7	.60	1.2	.35	2.5	1.0	1.5	1.0	3.2	1.2
	500	2.0	1.0	2.7	.85	4.0	2.5	4.0	1.5	4.5	1.5	5.7	1.3	2.5	1.0	3.7	1.1
	1000	4.2	2.1	7.0	2.2	4.7	1.9	7.7	1.1	7.0	3.0	7.2	2.6	8.5	3.0	9.0	2.5
	2000	8.7	2.1	10.0	3.0	7.2	3.3	7.5	2.0	8.0	3.2	8.7	4.1	10.5	3.5	11.0	2.5
Z	50	6.3	1.3	7.5	2.0	5.6	.97	6.0	.5	7.8	1.6	8.2	2.5	6.8	2.1	6.8	2.3
	100	10.2	2.1	9.7	2.0	6.0	1.0	5.7	1.5	8.8	2.1	9.6	2.1	8.8	4.3	9.6	3.5
	500	11.0	3.0	10.8	2.8	8.0	1.6	8.0	2.5	16.5	2.5	18.0	6.5	14.2	4.2	13.2	4.5
	1000	12.3	4.4	13.3	5.1	8.6	2.7	10.0	1.7	20.0	6.7	18.1	5.9	14.1	3.1	12.8	4.6
	2000	15.6	2.6	13.8	2.0	10.1	2.6	11.1	3.9	24.7	7.2	22.3	8.3	16.7	3.3	13.8	2.8

may be different. In the case of adaptation experiments, there is probably less likelihood that subjective touches will dove-tail into the waning sensory experience, and the suggestion that the after-sensation should continue for some time may be stronger than that adaptation should consume considerable time.

A closer inspection of Table IV reveals the fact that the average post-stimulatory adaptation-times of *E* and *W* are in most cases slightly greater than their average times for stimulatory adaptation. In the case of *Z*, 9 of the 20 post-stimulatory averages are slightly shorter, and in 2 other cases the measurements are just the same. In view of the fact that, in general, these differences are comparatively insignificant, it seems justifiable to infer that whatever discrepancies the table reveals in this respect are to be attributed to the accidental influences which play a rôle in such an experimental situation.

THEORETICAL CONSIDERATIONS

The implications of these experimental results should be adapted to the prevalent views in regard to cutaneous theory. Since the *all-or-none* law has gained widespread acceptance, the qualitative type of theory, which implies the hypothesis of the *specific energy of nerves* has been largely superseded by quantitative theories. Nafe has recently advanced such a theory of "feeling."[11] In demonstrating that adaptation-time is directly proportional to the intensity or amount of stimulation, and in indicating that in all probability a linear relationship prevails between these two functions, we have shown that adaptation represents a quantitative continuum, and thus have contributed to the support of that theory.[12]

As to what happens in the cutaneous receptor when adaptation to pressure is in progress, we are probably not yet in possession of all of the facts necessary to arrive at a positive conclusion; however, certain available facts indicate the general nature of the function of adaptation.

It has been suggested that adaptation represents the establishment of a state of equilibrium by the receptor. Stimulation is said to disturb the equilibrium of the receptor, and, if the same amount of stimulation be continued long enough, equilibrium is reëstablished by the receptor so that the stimulation becomes ineffective. What, most probably, is the nature of these changes in the equilibrium of the receptor during the time that stimulation and adaptation are in progress?

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[11] J. P. Nafe, A quantitative theory of feeling, *J. Gen. Psychol.*, 2, 1929, 199-211; The sense of feeling, *Foundations of Experimental Psychology*, 1929, 392-413.

[12] Straus and Uhlman, *op. cit.*, reported that adaptation-time of the receptors of the skin varies with intensity in the stimulation of pain, a result that resembles the present finding for pressure.

Owing to the fact that adaptation-time to more intense stimulation is longer than that to less intense stimulation, adaptation obviously cannot consist in a fatigue-producing process in the receptor.

The writer believes that the essential nature of adaptation and the related functions of the receptor may be explained by the bipolar theory recently elaborated by Crile.[13] According to this view, the receptor represents a bipolar electro-chemical unit. In the resting condition, it maintains a state of normal electrical potential. Under stimulation this normal condition is broken down, the degree depending upon the intensity of stimulation. The bipolar unit (or receptor) acts in essential respects like a self-charging condenser. It transforms energy and discharges it in impulses over the nerve. These impulses vary in frequency, depending upon the intensity of stimulation. But if the same stimulus continues to be applied, a recharging process is initiated by the receptor so that in time the normal electrical potential (or equilibrium) is reëstablished. The length of the recharging period is also a function of the intensity of stimulation. A greater amount of stimulation breaks down the electrical charge to a greater degree than one of less intensity, accordingly, a longer period of time is necessary for the normal electrical potential to be reëstablished in the electro-chemical unit.

The careful removal of a stimulus, to the continued application of which the receptor has already established complete adaptation, sets up stimulating conditions of essentially the same degree as the stimulation which was occasioned by that stimulus when it was first applied to the skin. The adaptation-time is, accordingly, approximately the same in both cases.

In the stimulation of an aggregation of receptors, as in our experiments, the same principle of operation probably holds as that which operates when an individual receptor is activated. Although the central nervous system coöperates with the activated receptors in the transaction of these functions, we are not yet in possession of a body of facts sufficient to enable us to infer how and to what degree it plays a rôle in adaptation.

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[13] G. W. Crile, *A Bipolar Theory of Living Processes*, 1926, 405.

SUMMARY

Experimental results are presented, in the first part of this paper, which show that adaptation-time to cutaneous stimulation is directly proportional to the intensity of stimulation, and inversely proportional to the extent of the field of stimulation. It is indicated that the relationship existing here is probably linear.

In the second part of the paper, experimental results are produced, which indicate that the adaptation-time to the continued application of a stimulus to the skin is approximately of the same magnitude as that which follows the removal of that stimulus, after complete stimulatory adaptation had been established.

These results are interpreted as signifying that the cutaneous receptor for pressure may act in its essential operations like a bipolar electro-chemical unit. It maintains, in the rested condition, a certain electrical potential, which is broken down under conditions of stimulation, the degree varying directly with the intensity of stimulation. As long as the electrical potential of the receptor remains below normal level under stimulation, the receptor discharges impulses over the nerve. Nevertheless, the bipolar unit (or receptor) is capable of reacting in such a way, even while uniform conditions of stimulation are sustained, that its normal electrical charge is restored. This function corresponds to adaptation, for when there is complete restoration of the electrical potential in the unit (or receptor), it ceases to discharge impulses; in other words, the continued stimulation becomes ineffective.

EFFECT OF COMPLEXITY OF MATERIAL UPON THE FORM OF LEARNING CURVES

By J. B. STROUD, Kansas State Teachers College of Emporia

In a recent study the writer obtained s-shaped learning curves for poetry.[1] In seeking an explanation of this phenomenon two conditions incident to memorizing poetry seemed sufficiently suggestive to merit further study. These are 'serial order' and 'size of unit of response.' Practically all the *S*s tended to learn the lines in serial order. Inasmuch as they read the entire selection aloud each trial they were naturally required to give some attention to the whole poem, with each reading, while probably making a special effort to remember certain lines. Thus it seemed logical to assume that, while *S* was fixating the initial lines, he would acquire increasing familiarity with the subsequent lines, and would, therefore, be able to learn them with increasing speed, within limits, when he came to make a more direct effort to learn them. Such factors would tend to account for the initial positive acceleration of s curves.

In the study reported above, the unit of response was a line of poetry. If, for example, on a given trial *S* repeated 5 lines correctly he would be credited with 5 correct responses. It seemed that under this plan the *S* might be able to reproduce relatively few whole lines during the initial trials, although he might be able to recall several part lines without credit. This factor would also help to account for initial positive acceleration. The results of the present study indicate that this formulation was too restricted. The experiments reported here were undertaken in order to determine whether the two factors named above are responsible for the s-shape of the curves.[2]

EXPERIMENT I

Apparatus and procedure. The learning material used in this experiment was monosyllabic nouns. The more obvious meaningful combinations were avoided. Two lists of such words were prepared. Both lists were learned by the method of

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[1] J. B. Stroud, Learning curves for poetry, this JOURNAL, 43, 1931, 684-686.

[2] The writer wishes to thank Professor H. A. Carr of the University of Chicago for his helpful criticisms.

direct recall, but under two different conditions. Under Condition A the *Ss* were instructed to learn the words in any order they wished, and under Condition B they were instructed to make an effort to learn the words in serial order. This variation was designed to test the influence of serial order upon the form of the learning curve.

The size of the unit of response was also varied by using 4 different sets of words of varying degrees of complexity. Set *a* consisted of 12 words taken one at a time; Set *b*, 24 words taken two at a time; Set *c*, 36 words taken three at a time; and Set *d*, 48 words taken four at a time. Thus the number of units was constant in every set but the size of the units varied.

Four groups of *Ss* were used. Group I learned a list of words of Set *a* according to Condition A and another list according to Condition B. Group II learned one list of words of Set *b* according to Condition A and another list according to Condition B. Similar procedures were followed in the case of Groups III and IV with Sets *c* and *d*. A counterbalanced order was used both as to lists and conditions. Since a different group of *Ss* was used for every one of the four sets of words, practice did not affect the results.

Readings and recalls were alternated. One reading and one recall constituted a trial. In both learning conditions (A or B), the *Ss* were credited with a correct response for each unit correctly recalled. A unit was considered correct when all the words in it, in case there was more than one word, were recited in the order in which they occurred. A list was considered learned, under Condition A, when all the units were recalled, regardless of the order of the units, and under Condition B when all the units were recalled in serial order.[3]

The units recalled were plotted against trials, in order to obtain the learning curves. The responses were recorded in such a way that it was possible also to plot curves in terms of words against trials as well as units of words against trials.

Where several records are averaged so as to make a composite learning curve, it is imperative when the shape of the curve is under consideration, that all the individual curves be reduced to a common length before averaging them. All the individual curves obtained in these experiments were reduced to 10 equal parts. The actual number of trials varied from 4-25. This particular division seemed sufficient to preserve most of the individual characteristics of the curves and at the same time afforded a convenient working basis. The curves were equated graphically by the method illustrated in Fig. 1.[4]

As illustrated in Fig. 1, a curve was plotted in the usual way for each individual record, the points being connected by straight lines. Each baseline was divided into 10 equal parts comprising what may be termed an abstract baseline as 'a,' Fig. 1. Ordinates were erected from each division point on the new baseline to the intersection with the curve. It was possible to determine the height of the curve at each intersection point by reading the corresponding values on the ordinate as illustrated by x and y. The height of the curve at the intersection of x and y is 2.6. When the curves are plotted on large surfaces they may be equated by this method accurately and conveniently. This procedure usually has the effect of smoothing the curves slightly, but this does not appear to be a limitation when the form of the curve is being studied.

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[3] The words in the respective units had, of course, to be in order. In practically all cases, however, the *Ss* learned the units in serial order. After the first few readings most of them adopted the condition of serial order, as if to take advantage of place associations.

[4] C. L. Kjerstad, The form of the learning curves for memory, *Psychol. Monog.*, 26, 1919, (no. 116), 25-27, appears to have used a somewhat similar method.

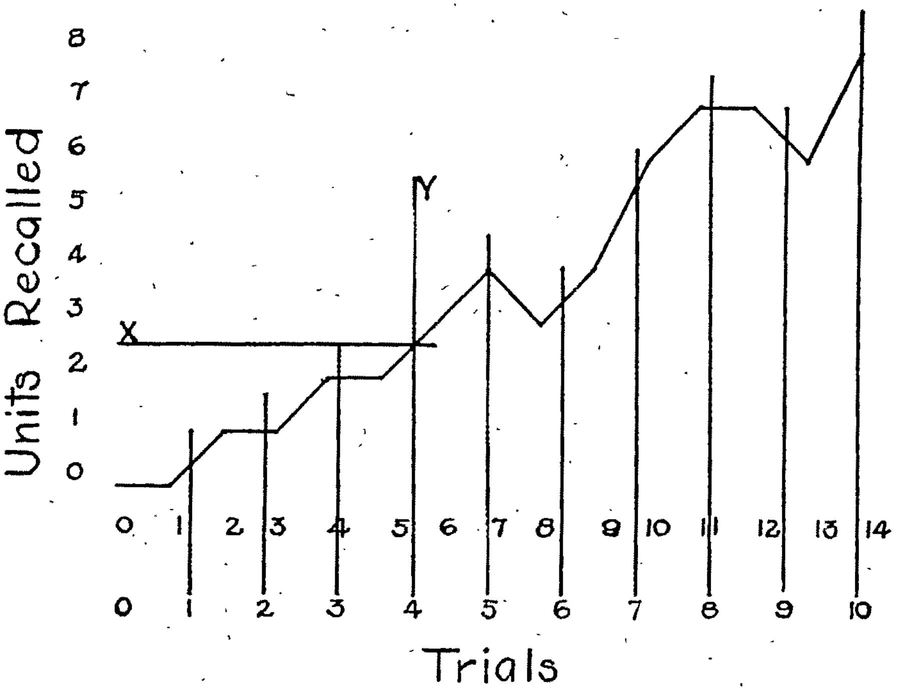

FIG. 1. METHOD OF EQUATING CURVES AS TO LENGTH

The material was presented visually by means of a memory apparatus, and the *Ss* read the words aloud. The following time-intervals were used for the different sets of materials: Set *a*, one word, 3.0 sec.; Set *b*, two words, 3.5 sec.; Set *c*, three words, 4.0 sec.; and Set *d*, four words, 4.5 sec.

The writer served as *E* in all experiments but one which will be mentioned later. The *Ss* were college students of undergraduate standing and practically all naïve in psychological experimentation.

Results. Table I and Figs. 2 and 3 show the results of Experiment I. Table I contains the data from which Figs. 2 and 3 were constructed. It gives the average number of correct responses for every decile, for every set of learning materials, under both experimental conditions. Fig. 2 gives the results for Condition A, and Fig. 3 for Condition B. The curves marked *a*, *b*, *c*, and *d* represent respectively the four sets of learning material.

TABLE I

AVERAGE NUMBER OF UNITS RECALLED PER TRIAL

		Decile										No.
	Set	1	2	3	4	5	6	7	8	9	10	*Ss*
Condition A	*a*	2.1	4.2	5.4	6.6	7.6	8.4	9.3	10.1	11.1	12.0	16
	b	1.2	3.7	5.1	6.1	7.3	8.0	9.0	9.6	10.4	12.0	18
	c	1.0	1.6	2.9	3.6	4.9	6.9	8.3	10.2	10.7	12.0	10
	d	0.4	1.2	1.8	3.2	3.3	5.1	6.8	8.8	10.0	12.0	10
Condition B	*a*	1.8	3.8	4.9	5.9	7.1	7.7	8.8	9.5	10.5	12.0	16
	b	0.7	1.9	3.2	4.5	6.4	7.6	8.6	9.9	10.9	12.0	18
	c	0.2	1.0	2.1	3.3	4.3	6.0	7.4	9.3	10.6	12.0	10
	d	0.2	0.6	1.5	2.6	4.0	5.1	6.8	8.3	10.1	12.0	12

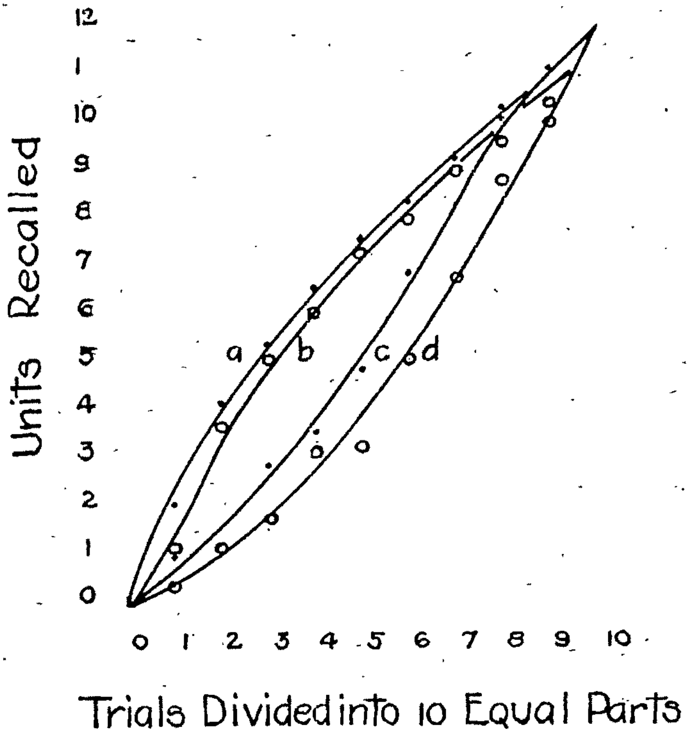

FIG. 2. LEARNING CURVES FOR MONOSYLLABIC NOUNS LEARNED IN ANY ORDER

These data show that both of the factors studied, viz. serial order and size or complexity of unit of response, are capable of contributing to the formation of s-shaped curves. While it is evident that serial order alone will not give s-shaped curves, inspection of the data reveals, that it is, however, an important factor. Curve *a*, Fig. 2 has less negative acceleration than Curve *a*, Fig. 3. Curve *b*, Fig 3 is decidedly s-shaped, while *b*, Fig. 2, representing material of the same complexity, is for the most part negatively accelerated, with possibly a slight initial positive acceleration.

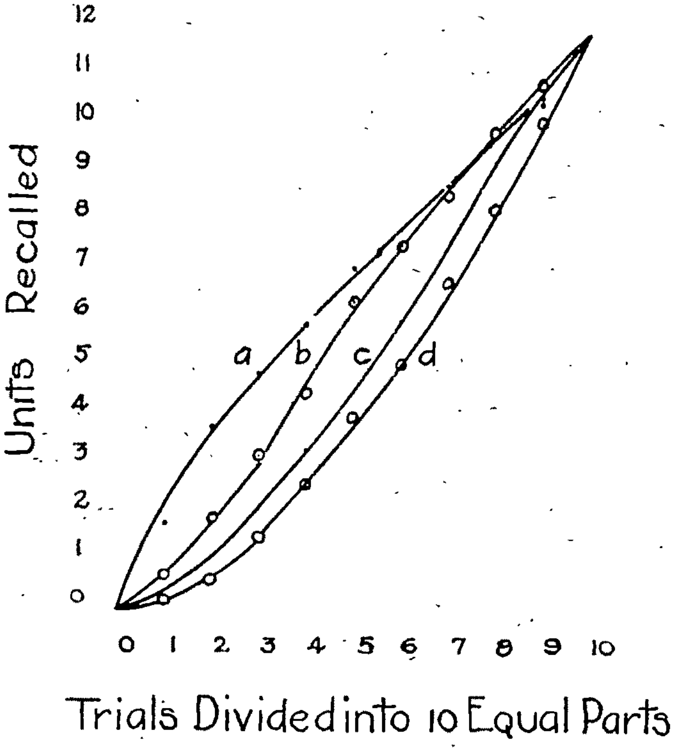

FIG. 3. LEARNING CURVES FOR MONOSYLLABIC NOUNS LEARNED IN SERIAL ORDER

Of the two factors studied, size of the unit of response seems to be the more important. Where the material is learned in serial order an initial positive acceleration is obtained when the size of the unit of response is increased to two words. This feature becomes more pronounced with further increase in the size of the units. Where the material is learned in any order initial positive acceleration is obtained when the unit of response is increased to three and four words.

Summary. The results of this experiment may be summed up as follows. Negatively accelerated curves were obtained when the unit of response was a single word. With an increase in the size of the unit of response the curves assumed an initial positive acceleration with a subsequent negative acceleration. The inflection points on these s-shaped curves rise, with further increase in the size of the unit of response, until they vanish, as in the case of Curve *d* and leave positively accelerated curves. The appearance of the s-shaped curves is hastened by the factor of serial order.

The effect of complexity of material upon the form of the learning curve seemed sufficiently important to merit further study. Inasmuch as the methods used above permitted of no experimental control over the responses, save that contained in the instructions, it seemed desirable to adopt some other method. Hence the method was changed to that of anticipation, and a second experiment undertaken.

EXPERIMENT II

Apparatus and procedure. The material used in this experiment was similar to that used in the preceding experiment. The number of words and their arrangement into sets were also similar. Thus four sets of learning materials were used consisting of 12, 24, 36, and 48 words taken 1, 2, 3, and 4 at a time, respectively. The method and rate of presentation were the same as that used in Experiment I. The time intervals allowed for the responses were 4, 5, 6, and 7 sec., respectively, for the shortest to the largest units used. Presentation and recall were alternated.

The method was carefully explained and demonstrated to every *S* before the experiment began.[5] The *Ss* were undergraduate students of psychology. Several had served in Experiment I, but the majority of them were naïve in so far as they had never served in psychological experiments.

The 12 and 24 word sets (Sets *a* and *b*) were usually learned by *S* at a single sitting, counterbalanced order being used for the different *Ss*. The words of the other two sets (Sets *c* and *d*) were learned at one sitting, but by different *Ss*. As before when the units of response consisted of two or more words, the *Ss* were required to learn the words of such units in the order in which they were presented.

Results. Table II and Fig. 4 give the results for Experiment II. Fig. 4 shows the curves obtained by the method of anticipation for the four sets of learning material.

The curves obtained by the method of anticipation are in general agreement with those obtained by the method of direct recall. The

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[5] The writer wishes to thank Miss Jean Clark for assistance in performing the experiments of this group.

TABLE II

AVERAGE NUMBER OF UNITS ANTICIPATED PER TRIAL

	Deciles										No.
Set	1	2	3	4	5	6	7	8	9	10	*Ss*
a	1.1	2.7	4.5	5.6	7.6	8.8	9.4	10.1	10.9	12	17
b	.4	1.9	3.3	5.3	6.6	7.6	8.7	9.6	10.7	12	16
c	.03	0.6	1.9	3.1	4.7	5.6	7.9	9.0	10.5	12	15
d	.1	0.5	1.4	2.5	4.5	5.6	6.9	8.7	9.9	12	12

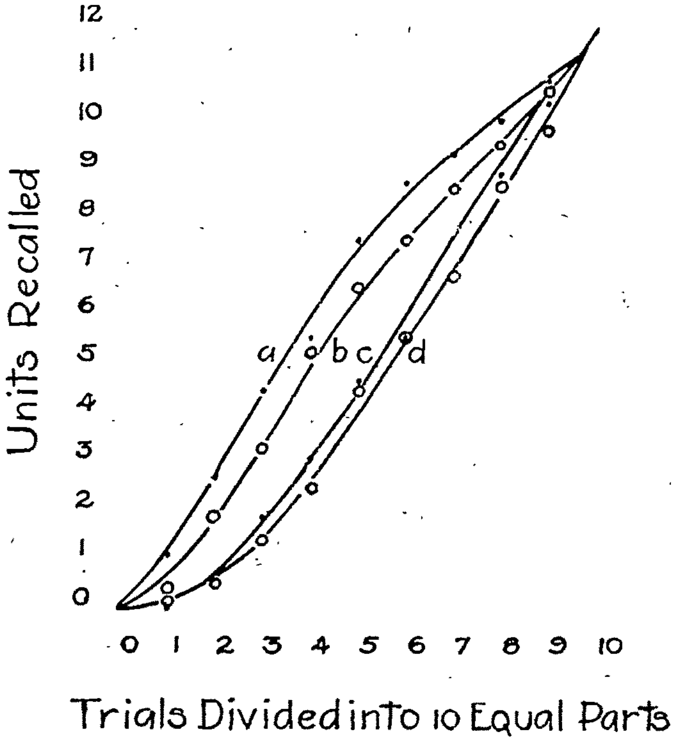

FIG. 4. LEARNING CURVES FOR WORDS BY METHOD OF ANTICIPATION

only notable exception is that of curve *a* Fig. 4, which is s-shaped, the point of inflection being between deciles two and three. Material of the same degree of complexity learned by the method of direct recall gave negatively accelerated curves. This is not a novel result, Kjerstad obtained similar curves for paired nonsense syllables and for paired monosyllabic nouns.[6] Such curves appear to be unstable and subject to wide variation due to individual differences. Altus, cited below, using single lists of monosyllabic nouns varying in length from 6-30, attained composite curves of negative acceleration throughout.

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[6] *Op. cit.*, 82.

The results of this experiment show that the form of the learning curve is significantly affected by the complexity of the unit of response. The inflection points rise to the vanishing point with increasing complexity of material.

SUPPLEMENTARY EXPERIMENTS

(*1*) *Effect of exposure interval.* It seemed possible that the length of exposure period might affect the form of learning curves for complex material, although Kjerstad found that varying the interval from 1.5-3 sec. did not influence such curves for single columns of non-sense syllables and monosyllabic nouns.[7] An experiment was undertaken with the view to throwing light upon this question. Twenty-seven *Ss* learned three different lists of 24 words taken 3 at a time under three different conditions of exposure; namely, 3, 5, and 7 sec., respectively. Practice effect was controlled by counterbalanced order.

TABLE III

EFFECT OF EXPOSURE INTERVAL UPON RATE OF LEARNING

	Deciles									
Interval	1	2	3	5	5	6	7	8	9	10
3 sec.	1.31	2.96	5.55	8.20	11.20	14.30	17.11	19.72	21.50	24.0
5 sec.	1.43	3.50	6.12	9.20	12.81	15.13	17.70	19.70	21.52	24.0
7 sec.	1.46	3.25	5.97	9.19	12.49	14.62	17.80	20.18	21.96	24.0

The results are given in Table III. It is readily seen that the form of the curves, platted from these data, would not be materially affected by these changes in time of exposure. This variation in interval, however, did affect the number of trials required to learn the lists. The average number of trials required to learn the three lists with the 3, 5, and 7 sec. intervals was 12.92, 12.08, and 10.79, respectively. It is interesting to note that the poorer learners benefited considerably by the longer exposure intervals while the better learners were practically unaffected by the changes in the length of interval. The average number of trials required by the poorest 14 *Ss* to learn the lists under the three conditions was 16.58, 15.17, and 12.4, while the corresponding scores of the best 13 *Ss* was 9.27, 9.00 and 9.18 trials.

(*2*) *Effect of practice.* It is possible to make some observation of the effect of practice on the form of learning curves from the foregoing data by comparing the rate of learning of the *Ss* on the three different days. The *Ss* benefited noticeably from the practice as shown by the reduction in the number of trials required to learn the material. The average number of trials required on the 1st, 2nd, and

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[7] *Op. cit.*, 69-71.

3rd days was 14.4, 11.95, and 9.4, respectively. The effect of interval of exposure was controlled by a counterbalanced order. Table IV shows the average number of words recalled at each 10th of the learning period on the three different days. Inspection of this table reveals that the rate of progress was not affected by the amount of practice achieved in this experiment.

TABLE IV
EFFECT OF PRACTICE UPON RATE OF LEARNING

	Deciles									
Day	1	2	3	4	5	6	7	8	9	10
1	1.43	3.17	5.53	7.91	12.04	14.10	16.36	19.40	21.94	24.0
2	1.05	2.68	5.40	8.50	11.26	13.88	17.60	19.70	21.10	24.0
3	1.70	3.87	6.70	10.03	13.20	16.02	18.50	20.30	21.92	24.0

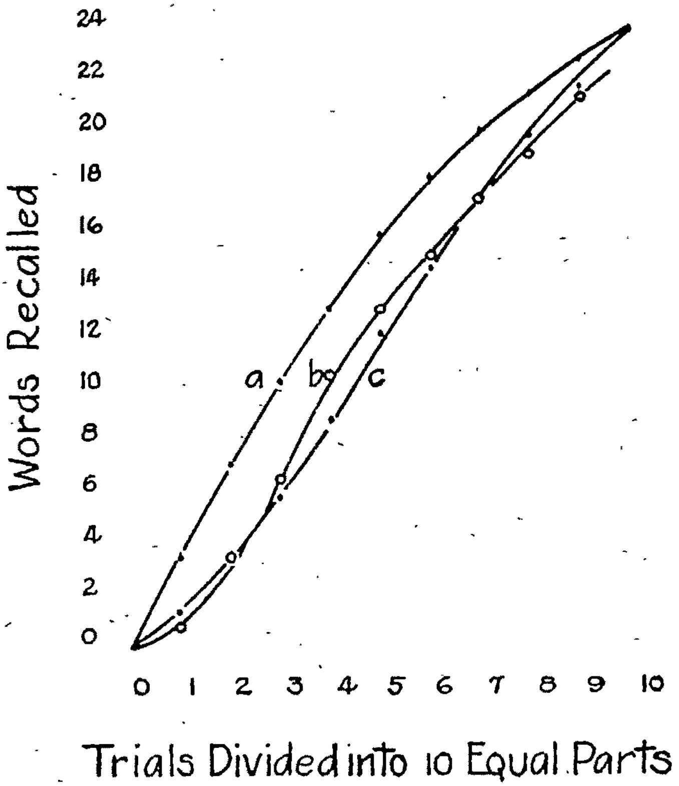

FIG. 5. CURVES FOR 24 WORDS ARRANGED IN THREE DEGREES OF COMPLEXITY

(3) *Complexity vs. length of material.* The question may arise as to whether the form of curves attained from the complex *sets* are due to complexity *per se* or merely due to the number of words used in these sets. The curves of Fig. 5 will

help to answer this question. Curve *a* is a composite of single curves of 20 *Ss* learning 24 monosyllabic nouns taken one at a time.[8] The method of anticipation was used. Curve *b* is taken from Fig. 4, Curve *b*, above and represents the rate of learning 24 words taken 2 at a time. Curve *c* is taken from the data in Table IV above. It is a composite of 3 curves each for 24 *Ss*. It thus represents the rate of learning 24 words taken 3 at a time. Here we have the same number of words learned by the same method, but varying in complexity.

These curves indicate that the changes in the form of the learning curves noted above are mainly due to the factor of complexity rather than to the varying number of words used.

Discussion

The results of the experiments indicate rather clearly that the form of learning curves is materially affected by the size, or complexity of the unit of response and to a lesser degree by the experimental method employed. On the basis of the experimental evidence it appears that the s-shaped curve may be regarded as an intermediate curve between negatively and positively accelerated curves, the type of curve obtained depending upon the complexity of the unit of response.

The results seem to warrant the following general statements. (1) As the complexity of material of a given length is increased the learning curves will change from negative toward positive acceleration; the intermediate curves will be s-shaped. (2) As the complexity of material is increased the inflection points on the intermediate s-shaped curves will rise, approaching the vanishing point. (3) The greater the complexity of a given bit of learning material the higher will be the threshold of 'recallability' of the respective units and the greater will be the number of readings required to reproduce them. (4) When material of a given length is read as a whole with somewhat equal distribution of attention to all of the units, the relative rapidity with which the successive units are recalled by a given subject will be proportional to the number of readings required.

The first and second generalizations may be regarded as descriptive, while the third and fourth may be regarded as explanatory.

The distribution of attention is considered an important matter in experiments of this kind. If no experimental control is exercised in this respect *Ss* may differ sufficiently in direction of attention to alter the form of the curves. Somewhat equal distribution of attention is regarded as most favorable for s-shaped curves and those of positive acceleration. If an *S* adopts a method grasping a definite number of units per trial the learning curve will tend toward a straight line, provided the rate can be maintained. Some approach toward equal distribution of attention may be achieved by having the *Ss* read the material aloud. This may be supplemented with instruction.

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[8] The writer is indebted to Mr. W. D. Altus for these data.

CONCLUSIONS

(1) Types of learning curves obtained for ideational material depend upon complexity of the material. (a) By the method of direct recall negatively accelerated curves were obtained for the 1-word units; s-shaped curves were obtained for the 2- and 3-word units; positively accelerated curves were obtained for the 4-word units. (b) By the method of anticipation s-shaped curves were obtained for the 1-, 2-, and 3-word units, and a positively accelerated curve was obtained for the 4-word units.

(2) The inflection points on the s-shaped curves rise with increase in size of the units of response.

A QUANTITATIVE STUDY OF THE RELATION BETWEEN PULSE AND BREATHING CHANGES AND ELECTRO-BIOCHEMICAL RESPONSES

By ALVHH R. LAUER, Iowa State College, and ERMA A. SMITH,
University of Chicago

So much has been said relative to bio-electrical phenomena within recent years that further elaboration is unnecessary here. Landis and Dewick have very thoroughly reviewed the literature.[1] It suffices to say that the variation in methods of procedure would alone account for certain discrepancies in results noted. However, the immense amount of time and effort spent upon this type of investigation would tend to indicate the existence of a certain basic phenomenon which so far has not been satisfactorily measured.

It is our contention that various factors are involved in electrical changes of the living organism, and the present study is an attempt to determine the association of such relationships. Particular attention is given to the temporal sequences of different processes selected for measurement.

The junior author has shown that electrical accompaniments of certain responses can be measured consistently if the physical characteristics of the apparatus are held constant.[2] When a measure can be repeated with a certain assurance of securing consistent results, the technique may be used to determine, if possible, the antecedent-consequent relationships, or associations of such phenomena as may be under consideration. The following study was thus undertaken to discover the relation of certain physiological processes occurring immediately after sensory excitation.

METHOD AND APPARATUS

The laboratory method is here supplemented by statistical analysis. Measurements were made of 30 college sophomores making a total of 133 reactions. The averages of each were used for tabulating data on the various stimuli. For correlation purposes the 133 reactions were treated separately.

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[*] Accepted for publication September 25, 1931.

[1] C. Landis and H. N. Dewick, The electrical phenomena of the skin (psychogalvanic reflex), *Psychol. Bull.*, 26, 1929, 64-119.

[2] A. R. Lauer, Reliability of the psycho-galvanic reflex, this JOURNAL, 41, 1929, 263-270

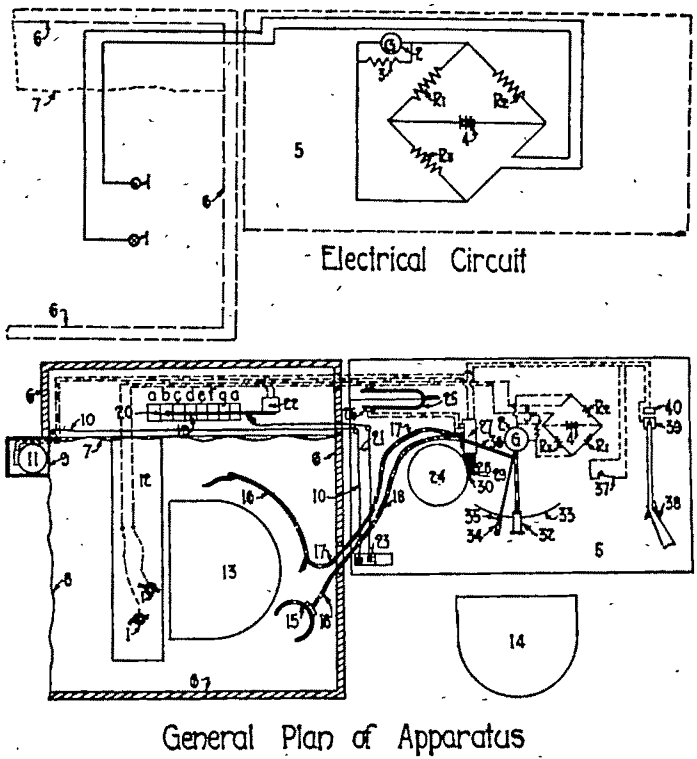

Fig. 1. Showing the Wiring Circuit and the General Plan of the Apparatus

(1) Electrodes
(2) Galvanometer
(3) Shunt
(4) 3-volt battery
(5) Table
(6) Semi-darkened booth
(7) Screen shielding the xylophone and hammer
(8) Curtain shielding skull
(9) Skull support
(10) Cord for releasing and lighting skull
(11) Skull
(12) Support for *S*'s hands
(13) Chair
(14) Chair for E
(15) Sphygmograph
(16) Pneumograph
(17) Connecting tube for 16
(18) Connecting tube for 15
(19) Xylophone
(20) Sound hammer
(21) Hammer release cord
(22) Contact for registering hammer stimulus
(23) Contacts for registering release of all stimuli
(24) Kymograph
(25) 6.57 d.v. fork for timing
(26) Contact on fork
(27) Signal magnet
(28-30) Signal magnets
(32) Telescope
(33) Telescope scale
(34) Needle pointer
(35) Lever for tracing galvanic curve
(36) Controls for curve marker
(37) Key for incidental stimuli
(38) Rifle
(39) Bracket for stimulus-contact
(40) Contact to register firing of gun

The *S* was seated in a cushioned chair (13) (see Fig. 1) within a darkened booth and instructed to sit quietly and relax. Information was further given that surprise stimuli would be presented. A Marey tambour (15) was attached to the left side of the neck just over the external carotid artery and fastened by means of a special strap around the neck. This tambour was in turn connected with the sphygmograph which recorded on a smoked drum (24). A pneumograph (16) was also attached to *S* in the same general way.

The electrical changes were measured by a Wheatstone bridge (4) with a d'Arsonval galvanometer of the Leeds and Northrup fabrication, having 500 ohms coil resistance, 300 megohms sensitivity, with a 3-sec. period and critically damped. The regular Féré technique was used with a 10:1 ratio in the two arms of the bridge and a 3-ohm shunt across the terminals of the galvanometer. The junior author's liquid electrodes were employed,[3] being attached to the palms of the hands. A 3-molar NaCl solution was used as the electrolyte. The two hands were placed on a cushion supported by a long desk (12).

By means of a needle pointer (34) attached to the lever (35) the deflections were registered directly on the kymograph drum (24). Thus the pulse, breathing, and electrical changes were recorded simultaneously, as shown in the sample records in Fig. 2. Time was marked by a signal magnet (29) connected in series with a 6.57 d.v. fork. The fork was calibrated for this experiment.

In the same way the administration of stimuli was registered by a signal magnet (30) in series with a multiple arc circuit, each branch of which respectively was closed with the presentation of one type of stimulus. The latter consisted of: (a) a small xylophone bar (19) struck by a sound hammer (20) raised to the height of 30 cm. and allowed to fall of its own momentum; (b) a skull (11), hidden behind the curtain (8), which was illuminated from within by a 60-w. bulb when the cord (10) pulled aside this covering; (c) the shrill blast of a policeman's whistle blown by *E*; and (d), a shot from a .22 short blank cartridge fired from a clamped rifle (38) pointed in the opposite direction from *S*. The policeman's whistle was less constant than the other three types of stimuli, but in any case the element of personal proximity to *S* was excluded.

After the *S* was seated for a few minutes and his resistance stabilized, the first stimulus was given. The measurement was made, and after stabilization the bridge was rebalanced, if necessary, and a second stimulus given. In about 70.1% of the cases no rebalancing was necessary. The stimuli were generally given in the following order: sound of hammer, whistle, skull, and rifle. In a few cases the order was changed and usually the stimuli were repeated. Mean values were used as scores in the latter cases.

The records furnished several kinds of data for analysis. Body resistance values were copied directly on the data sheet from the bridge reading. Using a method of calibration previously described by the junior author,[4] the resistance and deflections were used to compute the actual ohms change of the *S* and the percentage change. The latter is a result of the actual change in ohms divided by the initial body resistance.

Considering Fig. 2, Curve 1, it will be seen that the galvanometer deflections may be divided into three separate phases with respect to temporal sequence.

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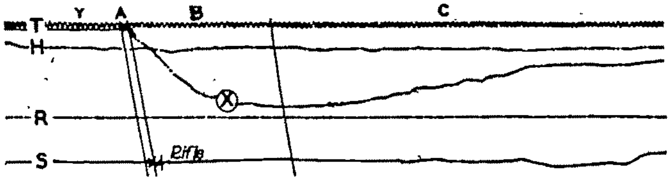
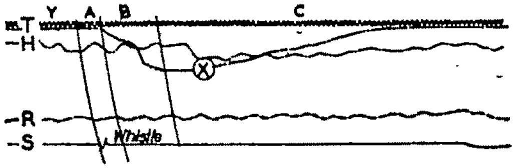
[3] Lauer, A new type of electrode for the galvanic skin reflex, *J. Exper. Psychol.*, 11, 1928, 248-251.

[4] Lauer, *op. cit.*, this JOURNAL, 263-270.

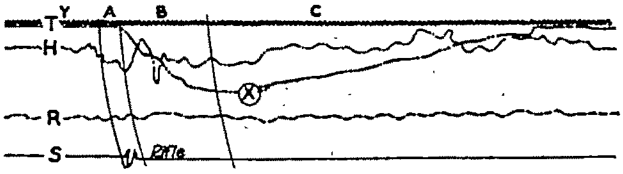
There is, first, the period of time after a stimulus is given until the deflection takes place (A); second, the time required for the deflection to reach a maximum (B); and third, the time required for the return to normal (C).[5]



Curve No. 1 R.B. at 4.15 P.M.



Curve No. 2. A.E. at 5.00 P.M.



Curve No. 3

FIG. 2. SAMPLE RECORDS OF THE PULSE, BREATHING, AND ELECTRICAL CHANGES

T = time line in 1/6 sec.; H = pulse; R = respiration; S = stimulus; X = galvanometer; Y = stable period before stimulus; A = time between stimulus and beginning of response; B = time required for maximum galvanic response; C = time required for galvanometer to return to a point of stability.

In Curve No. 1 the galvanometer did not return to normal. Records like this were obtained in about 30% of the cases. Another unusual characteristic of this curve is the very short duration of period A. Curve No. 2 is more typical of the results obtained in this study. In Curve No. 3 the pulse line (H) shows marked changes—the startling effect of the rifle shot.

In addition to these phases of the electro-organic response, pulse rate and breathing curves were recorded. The curves of Fig. 2 show these results in detail. In addition, body resistance could be found from the data sheet upon

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[5] It may be well to say that there was no clean-cut case of the resistance's having increased immediately after a stimulus. There is always a possibility that attaching the electrodes may produce ohmic changes, and in certain *S*s this may persist for an unusually long time. In one instance, after the bridge was balanced there seemed to be a gradual increase in resistance. After stabilization the stimulus was presented and the resistance continued to increase. Later excitations of the same *S* failed to produce the same results.

which a limited protocol was made. The records were read by means of a small glass in units of slightly under 1/6 of a second as given above. Measurements of segments of the curves were made in millimeters from hairline scratches made on the fixed records. These data of 133 reactions from 30 *S*s were punched on Hollerith cards, and part of the results computed from the latter. In order to study the nature of the relationships the correlations were plotted on standard sheets.

RESULTS

Table I shows that (1) the measures are consistently in one direction which is an indication of the significance of measurements, and (2) there is a fair correspondence in magnitude of the responses with the intensity of the stimulus.

TABLE I

EXTENT AND DIRECTION OF REACTIONS TO DIFFERENT STIMULI

	Bell	Skull	Whistle	Rifle	Mean
Breathing (percentage)......	− 1.6	− 3.0	− 2.7	− 5.3	−1.80
Heart rate (percentage).....	+ 8.4	+ 7.9	+ 6.6	+ 9.6	+8.12
Galvanometer deflections in cm...................	+ 7.8	+ 7.6	+ 6.5	+11.5	+9.09
Change in ohms resistance..	−1560	−2240	−1104	−2426	−1845
Percentage ohms change....	− 7.8	− 8.7	− 6.7	−10.9	−8.20

In Table II the following facts seem significant: (1) the first response is delayed longer than those made to subsequent reactions; (2) the greater the response the longer is the period before return to normal; (3) there is very little difference between the various stimuli with respect to the time between administration of the stimulus and the point of maximum deflection.

TABLE II

DURATION OF DIFFERENT PERIODS OF GALVANOMETER DEFLECTION

Time (in sec.)	Bell	Skull	Whistle	Rifle	Mean
From stimulus to response...	4.39	3.76	3.81	3.77	3.94
For deflection to reach maximum....................	10.39	11.64	9.19	11.76	10.78
Sum......................	14.78	15.40	14.00	15.53	14.72
For galvanometer to return..	21.75	24.64	23.55	39.52	27.03

The temporal relation of these changes suggested a further comparison with existing data. According to Buchanan,[6] pulse changes taking place immediately following the onset of physical exercise, increased 31.8% the first ½ sec. Within 4 sec. after a sensory stimulus was given in our experiments the increase was only 5.1%. The total increase over the resting rate was 9.1%. The upper half of *S*s (those having greatest heart increase) were sorted out by the tabulating machines and compared with respect to time and magnitude of deflections. This is shown in Fig. 3.

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[6] F. Buchanan, The physiological significance of pulse rate, *Trans. Oxford Scientific Club*, 14, 1909, 351.

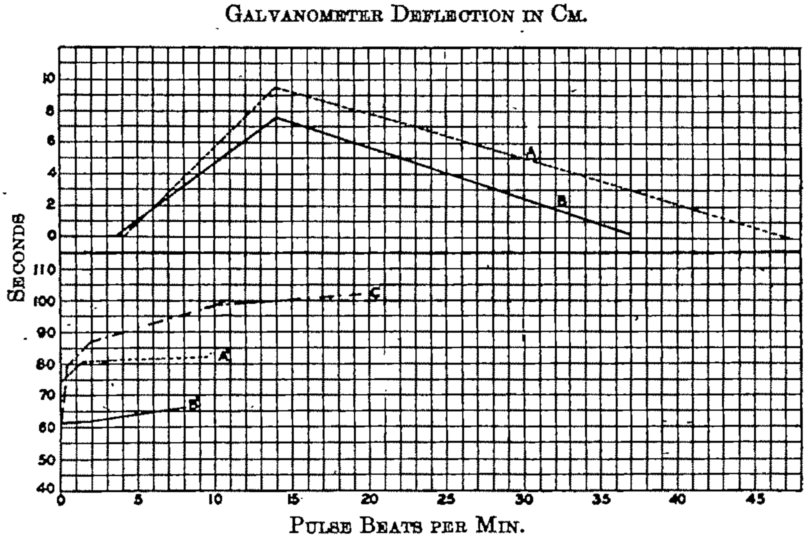

FIG. 3. CURVES SHOWING CHANGES IN THE GALVANIC RESPONSE AND PULSE RATE

Curves A and B represent the galvanic response periods for the upper and lower 50% groups divided on the basis of pulse rate. Curves A′ and B′ show the relative amounts and rate of pulse changes for the same groups. Curve C shows Buchanan's increase of pulse rate with exercise.

It is evident from the curves in Fig. 3 (a) that pulse changes take place much more rapidly than galvanometer deflections from biochemical responses; (b) that persons with a higher normal pulse rate tend to have greater proportional increases than do those with lower pulse rates; and (c) exercise increases pulse rate much more, but the time for the increase is about the same.

In addition to the measurements described above, the standing pulse rate was taken after *S* had rested 2 min. in the laboratory. He was then caused to walk 100 steps at a brisk rate. No graphic record could be made; only a record of pulse rates as taken from the radial artery before and after exercise. The percentage increase with exercise was correlated with the percentage increase after sensory excitation. An $r = -0.2723 \pm .11$ was obtained, indicative of at least a zero, if not a slightly inverse relationship, between these two phenomena. This would seem to signify that different factors are involved in pulse increases during exercise and those during or following sensory excitation.

Finally the 133 reactions were considered separately and correlated to determine the various intercorrelations of the 7 variables used in the major series of this study. The results are given in Table III, which shows the following facts:

TABLE III

INTERCORRELATIONS OF CHANGES FOLLOWING SENSORY EXCITATION

Hc = heart change in beats
Td = time for maximum deflection
Oc = actual ohms change
Dc = deflections in cm.

Tr = heart change in beats
Rt = time of galvanometer return
Br = initial body resistance
Bc = breathing changes in percentage

(The probable errors range from ±0.060 to ±0.022)

	Hc	Td	Oc	Dc	Tr	Rt	Br
Td	+.7962						
Oc	+.3813	+.5478					
Dc	+.3254	+.4178	+.1664				
Tr	−.0754	+.0099	−.0410	−.0736			
Rt	+.3216	+.2135	+.2366	+.4189	−.1177		
Br	+.1640	+.1902	+.6347	−.1568	+.1581	−.1367	
Bc	−.1892	+.0100	+.0157	+.0877	−.0845	+.0122	−.0084

(1) There are significant associations between the following variables: (a) heart changes and the time required for the deflection to reach a maximum; (b) initial resistance and ohms change; (c) ohms change and heart variation; (d) magnitude of deflections and heart variation; (e) return time and pulse change; and (f) possibly between pulse and breathing changes.

(2) There is no significant relation between breathing changes and any of the six other variables studied. The reliability of the measurements of breathing changes was not determined, but the consistency of changes shown in Table I tend to support these results as worth consideration.

(3) Other apparent relationships between return time, deflections, ohms change, and initial resistance are inherent, due to characteristics of the apparatus as described by the junior author.[7]

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[7] Lauer, *op. cit.*, *J. Exper. Psychol.*, 11, 1928, 248-251.

A further study of the relation between body responses and electrical changes was made by multiple correlation technique. The formula, $R_{c\cdot12} = [(r^2_{c1} + r^2_{c2} - 2r_{c1}.r_{c2}.r_{12})/\ (1 - r^2_{12})]^{\frac{1}{2}}$ was used. In turn each aspect of the galvanometric curve and its characteristics was used as a criterion with the heart and breathing changes set as independent variables. The results shown in Table IV were obtained.

TABLE IV

MULTIPLE CORRELATION COEFFICIENTS

$R_{Rt.Ba.Ha.} = +.3301$ $\quad R_{Oa.Ba.Ha.} = +.4177$

$R_{Tr.Ba.Ha.} = +.1038$ $\quad R_{Td.Ba.Ha.} = +.8131$

$R_{Da.Ba.Ha.} = +.3264$ $\quad R_{Br.Ba.Ha.} = +.1655$

From Table IV it is obvious that the length of time required for the electrical changes to take place is most closely associated with pulse and breathing changes. The second closest asociation is the actual ohms change; next comes the time for return to normal, and finally the amount of deflection. Of course some of the other measures may be more important, depending upon the criterion used, but the facts of association are clearly shown.

SUMMARY

(1) When external conditions are held constant the magnitude of the electro-biochemical response is roughly proportional to the strength of the stimulus.

(2) Changes in pulse rate from exercise seem to be brought about by different factors than those producing changes in pulse rate after startling or exciting stimuli.

(3) The period of time required for the galvanometer to return to normal seems to be important, as measuring responses different from those involved in sheer physiological changes.

(4) Body resistance seems only slightly correlated with susceptibility to change after sensory excitation.

(5) Changes in pulse rate after the stimuli were given are highly correlated with the speed of the response.

(6) Breathing changes seem to have no correlation with the electrical changes measured by this technique.

THE RELATION OF BRIGHT AND DULL PRESSURES TO AFFECTIVITY AND THE METHOD OF CORRELATION

By ELIZABETH CONVERSE, Bryn Mawr College

In an endeavor to settle the issues raised by Young's criticism[1] of Nafe's reduction[2] of pleasantness and unpleasantness to bright and dull pressures, Hunt proposed the method of correlation as a solution.[3] Young claimed that Nafe's results were due to laboratory atmosphere. While Nafe denied this, he did admit that bright and dull pressures were not 'attention demanding.' In order to find them the *O*s must be set for them. In order to avoid laboratory bias, the *O*s must not be told of any connection between these pressures and affectivity. Hunt, therefore, instructed his *O*s to report at one time upon the bright and dull pressures aroused by visual stimuli, and at another time upon pleasantness and unpleasantness. "The correlation between the two sets of reports would then support Nafe's claims or disprove them."[4] Experiments with different groups of *O*s and under a variety of checks gave consistently high correlations between these two sets of qualities, with sufficiently small probable errors to insure statistical reliability. Hunt's results are obviously not due to chance. Of three possible interpretations, Hunt concludes that "affection is accompanied by bright and dull pressures," rather than "affection *is* bright and dull pressure" (Nafe's view), or that these qualities depend upon identical receptor processes but different attitudes of report (the third view).[5]

EXPERIMENT I

Following the suggestion made by Hunt that further investigation is necessary to determine which of these views is correct, and, we may add, to determine if some other view may not be even more

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*Accepted for publication July 9, 1932. This study was directed by Professor Harry Helson.

[1] P. T. Young, Studies in affective psychology, this JOURNAL, 38, 1927, 157-193.

[2] J. P. Nafe, An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.

[3] W. A. Hunt, The relation of bright and dull pressure to affectivity, *ibid.*, 43, 1931, 87-92.

[4] Hunt, *ibid.*, 88.

[5] *Ibid.*, 92.

plausible in the light of added evidence, we decided to repeat Hunt's work, keeping conditions, during part of our experimentation, identical with his.

Procedure. In the first set of experiments Hunt's procedure was followed as closely as possible in every respect. Visual stimuli were used, as in his experiments, in order to avoid local pressures. They consisted of 20 standard Milton-Bradley colored papers of various hues, brightnesses, and saturations. The stimuli were 5 in. sq. and exposed behind the opening in a large gray cardboard screen. The *O*s were 4 ft. from the screen, eyes level with the stimulus. Palo daylight lamps served as the source of light in a dark room in order to insure constant illumination throughout. Fifteen persons acted as *O*s for this set of experiments, their psychological training being as follows: 4 *O*s, (*G, Gr, H, Y*) were seniors and somewhat experienced in observing as they were conducting experiments of their own and observing in those of each other; 10 *O*s, (*B, C, K, Ch, Br, Hu, Ba, Ka, P,* and *Q*) had had only an elementary course in experimental psychology, and 1 *O*, (*M*) was then taking her first course in psychology but had not then arrived at the laboratory work.

The instructions resembled Hunt's and were as follows for *G, Gr, H,* and *Y*:

> I shall present you with various colored stimuli and you are to report whether the experience involved is pleasant, unpleasant or indifferent.

The stimuli were given in constant order 10 times: 5 at the beginning of the week and 5 at the end. Every color was exposed for 5 secs., with a 10-sec. interval between successive presentations during which the *O*s gave their report. The following week the same colors were given again in the same way, but this time the instructions called for reports on bright and dull pressure. The instructions and explanatory comments used by Hunt were followed.

> I shall present various colors. You are to attend to the general experience involved in observing them, and to state whether this experience involves a bright pressure or a dull pressure. These pressures are qualitatively similar to those ordinarily elicited by cutaneous stimulation, but in no way identical with them.
>
> You are to report whether the color you have seen is accompanied by bright pressure, dull pressure, or nothing in the way of pressure.

The explanation of terms consisted of the following comments:[6]

> 'Bright pressure' has been described as a tactual bright pressure or touch quality lying between bright pressure and tickle. It has expansive qualities and has been called radial, being more prominent at the edges than in the center. It is bright, buoyant, and has a diffuse bodily feel. The whole experience is diffuse and volumic, and is patterned with the patterns varying from one experience to another. The following adjectives have been used in describing it: bright, sparkling, brilliant, active, effervescent, like tickle, dancing, shimmering, misty, mild, vaporous, light, airy, fluffy, ethereal, smooth, soft, welling, spreading, like expansiveness of the body.
>
> 'Dull pressure' has been described as a dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended. It has qualities of constriction and has been called radial, but more prominent at the center than at the edges. While it is diffuse and volumic, it is less so than bright pressure and is duller and less bright. It has been called dull, massive, drab, dead, rigid, less lively, somber, inert, stiff, gloomy, more dense, heavy, sinking, leaden, thick, cold, rough, grating, insistent, condensed, like bodily contraction.

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[6] Hunt's instructions and explanatory comments were given verbatim, *ibid.*, 89.

Both the affective and pressure judgments were given numerical values as follows: pleasantness and bright pressure were given +1; indifferent 0, and unpleasantness and dull pressure −1. The coefficients of correlation were then calculated by the product-moment formula.

A second group of *O*s (*B*, *C*, *K*, *M*, *Q*) were given these same colors and the affective judgments called for first and the pressure judgments later. Only 3 series, or repetitions of each stimulus, were given this time as it was found that the judgments remained reasonably constant from the very first in the previous observations.

A third group of *O*s (*Ba*, *Br*, *Ch*, *Hu*, *Ka*, *P*) were given 3 series in which the pressure judgments were called for first and later three more series in which the affective judgments were called for. This group was to serve as a check on the other two so far as order of judgments went.

Results. The results of the experiments with the three groups of *O*s are given in Table I.

TABLE I

SHOWING, FOR EVERY *O* OF EVERY ONE OF THE THREE GROUPS, THE CORRELATION BETWEEN THE AFFECTIVE AND PRESSURE JUDGMENTS

Group I			Group II			Group III		
O	r	P.E.	*O*	r	P.E.	*O*	r	P.E.
G	.43	.15	*B*	.59	.09	*Ba*	.32	.14
Gr	.0		*C*	.51	.11	*Br*	.42	.12
H	.66	.09	*K*	.0		*Ch*	.28	.14
Y	.09	.11	*M*	.18	.15	*Hu*	.003	.15
Av. r = 30			*Q*	.50	.10	*Ka*	.82	.05
			Av. r = .36			*P*	.0	
						Av. r = .31		

In every one of the groups one *O* failed to report any pressures with a consequent correlation coefficient of zero. These zero values do not depress the average coefficient of correlation very much. We felt no reason existed, therefore, for discarding the results, particularly as the *O*s seemed to be trying to follow the instructions.

The striking result from our experiments is the low correlation between bright and dull pressures with pleasantness-unpleasantness, low as compared with Hunt's results. The average coefficient of correlation for Hunt's 11 *O*s is 0.89 while that for the 15 *O*s used in our experiments is 0.36. If Hunt's probable errors of his coefficients of correlation are to mean anything they should give some indication of what is to be expected from a random sampling of coefficients of correlation based upon the same conditions. The coefficients for our *O*s fall so far outside the limits of Hunt's as to leave no doubts concerning the significance of the difference between his and our results. So far it would appear that a random sampling of *Os* does not result in a duplication of Hunt's high correlations even when his method of procedure has been followed to the letter—in so far as this is possible at another laboratory.

Experiment II

In order to favor Hunt's hypothesis we next used auditory stimuli which Hunt rejected because they might produce local pressures. This possible criticism of Nafe's work which Hunt wished to avoid in his impartial approach we decided to incur in the hope that better conditions for a high correlation would serve as a better test of the Nafe-Hunt theory.

Procedure. The stimuli in the first auditory experiments consisted of the eight tones of the 2-A audiometer, having frequencies of 64, 124, 256, 512, 1024, 2048, 4096, and 8192 cycles, given at a dial setting of 50 for each in order to keep the intensity of the tones constant and well above the threshold.

The *Os* in this part of the experiment were *C*, *G*, *Gr*, *H*, *Q*, and *Y* who had taken part in the previous experiments and in addition, two experienced *Os*, *F* and *Hi*, graduates in psychology. Fifty repetitions of every tone were given to every *O*. Every tone was given for 5 sec. and an interval of 10 sec. allowed for rest and report, and after every 7 repetitions a longer rest was allowed. The instructions were the same as above, except for the substitution of the word 'tones' for 'colors.' The first 4 *Os* were asked to give judgments of pressures first, and a week later judgments of pleasantness-unpleasantness, while the judgment reports were reversed in the case of the last named *Os*. The results were treated in the same way as described above for the color judgments.

Results. The results of Experiment II are given in Table II, which shows the coefficients of correlation, obtained from the *Os* of the two groups, between the affective and pressure judgments. All the *Os* of Group I reported pressures, including one *O* (*Gr*), who did not in Experiment I. The *Os* of Group II gave results very different from those obtained from Group I.

TABLE II

SHOWING, FOR EVERY *O* OF EACH OF THE TWO GROUPS, THE CORRELATION BETWEEN THE AFFECTIVE AND THE PRESSURE JUDGMENTS

Group I			Group II		
O	r	P.E.	*O*	r	P.E.
G	−.72	.20	*C*	.89	.05
Gr	.33	.21	*F*	.0	
H	.36	.21	*Hi*	.69	.13
Y	−.98	.001	*Q*	.0	
Av. r = −.25			Av. r = .39		

The results with the first set of *O*s on the tones gave an average correlation of −0.25, while the second gave an average correlation of +0.39. If we average both groups we get a coefficient of +0.07, which is nearly zero correlation. Analysis of the individual results shows that anything may happen. The two negative correlations are practically statistically reliable, as well as being fairly high while the same may be said for the two positive correlations in the second group. Two correlations are zero, even though pressures were reported, and two are neither sufficiently high nor sufficiently above their probable errors to be significant. Hunt, in discussing his coefficients, says, "if the effect of variability . . . could be cancelled out, the correlations would approach a value of +1.00 or perfection." As a matter of fact, a larger universe, as represented by the addition of our *O*s, gives lower correlations and contradictory results for different *O*s. Since Hunt's results all were in one direction and consistently high we must conclude that there existed some unspecified conditions favoring high correlations between the affects and pressures in his experiments. The results from our experiments, low, high, negative, and zero correlations, argue much more convincingly for 'neutral' conditions than do the results of Hunt.

EXPERIMENT III

Owing to the fact that some *O*s objected to the tone stimuli on the ground that they were devoid of feeling-quality, another set of experiments was designed in which auditory stimuli were presented having affective quality.

Procedure. Ten musical records containing selections from classical music, jazz, and 'strange music' were used.[7] Only the first few opening chords or bars were used containing a simple theme or melody and lasting only several seconds. They were presented three times in succession and the *O*s were asked to give their judgment on the last presentation. Pleasant-unpleasant judgments were given first, and then bright- and dull-pressure judgments.

Results. The results of Experiment III are shown in Table III. Despite the fact that we used in this experiment stimuli that had very definite affective values, which were much richer than the colors and pure tones used in the preceding experiments, the correlations were not significantly higher than before. With one exception, neither the correlations themselves, nor considered with their probable errors are significant.

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[7] The following records were used: Brahms, Symphony 1, Movement 1, *Victor, 6660-A*; Selections from Traviata, *Victor, 35807-A*; You Rascal, You, *Brunswick, 6225*; Debussy, Afternoon of a Faun, *Victor, 6696-A*; Brahms, Symphony 1, Movement 3, *Victor, 6660-B*; Rimsky-Korsakoff, Scheherezade, Movement 1, *Victor, 6738-A*; I Don't Know Why, *Columbia, 2540-D*; Selections from Traviata, *Victor, 35807-B*; Coplas del Terruno, *Brunswick-Bogata, 40267-A*; Return of Peer Gynt, *Victor, 9328-A*.

TABLE III

SHOWING, FOR EVERY *O*, THE CORRELATION BETWEEN THE AFFECTIVE AND PRESSURE JUDGMENTS

O	r	P.E.
C	.86	.06
G	.31	.19
Gr	.0	
H	.23	.21
Y	−.10	.21

We must conclude, therefore, that the occasionally significant coefficients of correlation found in our results are but due to random sampling and that for our group anything may occur: there may be high, low, zero, and even negative correlations between bright and dull pressures and pleasantness and unpleasantness. A larger universe does not help Nafe's or Hunt's case. It must be remembered that our *O*s were not instructed either for or against the theory that pleasantness *is* or is accompanied by bright pressures and unpleasantness *is* or is accompanied by dull pressures.

But Hunt got consistently high correlations between these two sets of qualities. He believes they support a particular analysis made by Nafe, or the latter's *O*s, Before we take up the question of the meaning of correlation, whether high or low, let us report another set of experiments and their results.

EXPERIMENT IV

We suspected that if any pair of opposites taken to be mutually exclusive by our *O*s were correlated with another pair of similar terms (pleasantness and unpleasantness, for example), the chances were good that some correlation would be found. We chose the paired terms 'personal-impersonal.' If one were asked what these terms have to do with pleasantness-unpleasantness we must confess we do not know; nor do we know what bright and dull pressures have to do with pleasantness and unpleasantness. Hunt implies that he did not either, but held that if the method of correlation should yield high coefficients, then Nafe's contentions would be supported. In that case we would be justified in concluding from the results about to be reported that affection is either personal or impersonal, or that it is accompanied by personal and impersonal (what?), or both depend upon identical processes but different attitudes.[8] Hunt's logic must apply to any pair of correlated variables if the correlations are sufficiently higher than their probable errors.

Procedure. Three series of the 20 colored stimuli previously used were given to *H*, *G*, *Gr*, and *Y*, with the following instructions:

I shall present you with various colored stimuli and you are to report whether they are personal or impersonal. Judge them according to your own definition of personal and impersonal.[9] Later you will be asked for your criteria of personal and impersonal. Try to keep as near as possible to the generally accepted meanings of these words.

We give some of the definitions of these words as reported by the *Os*.

Y: A color was personal if it was warm and rich and had a meaning, whether pleasant or unpleasant; and impersonal if indifferent and cold, with no meaning or effect.

H: Personal was a color intimately connected with myself, a color I might like to wear or have around, while impersonal was the opposite, being in no way connected with myself.

G: A personal color concerned definite things or a definite type of person; or a definite type of person might wear it. Impersonal colors, on the other hand, were general and might be associated with anything or anybody; anybody might wear them.

Gr: Colors were judged as personal or impersonal when they reminded me of something or of some person associated with me, whether pleasant or unpleasant. Impersonal colors called up no such associations.

Results. The results of Experiment IV are given in Table IV. The correlations shown here, as judged by statistical criteria, are all significant, two of them being high. If the reader will turn to the results for this group of *Os* in Experiment I he will find that they are much lower and with one exception do not possess reliability. But the correlations between affective value and 'personal-impersonal' are all fairly high and more than 4 times their probable error. Are we to conclude that all affective values are personal-impersonal or that the latter 'accompany' the former, or that they have identical receptor processes but different attitudes?

Having high correlations that are statistically reliable are we to conclude that the correlated variables can be reduced to one another, are associated with or accompany one another? Authorities in this field speak as follows: "Persons making use of the coefficient (of correlation) should be warned that it reveals conditions which, though necessary, are insufficient for inferences as to causal relations and common elements and other hypotheses involving extra-statistical consideration."[10] It would appear that Hunt's high correlations mean as little for the relationship between pressures and affects as ours do for the relationship between personal-impersonal and the affects. We are not concerned to argue the question as to how Nafe's results were obtained. We believe that Young has furnished a completely convincing explanation. We are only concerned here to show that it is possible to get high correlations between variables which have no psychological relationship to each other, other than that set by the conditions of the experiments. Ask a set of *O*s, with the suasion that goes with the atmosphere of an experiment, to judge a set of stimuli by means of two sets of terms, each containing antithetical terms, and we venture to predict that more often than not high correlations will be found from such procedure. Unless there is other ground for believing that some significant relationship exists between two variables, correlation furnishes no scientific basis for its assertion. High correlations may indeed lead us to look for more significant, psychological relationships but they do not and cannot settle the issues raised in the Young-Nafe controversy. On the contrary, high correlations may be due as much to laboratory atmosphere or other unspecified and unspecifiable conditions as introspective analyses.

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[8] These seem to be the only ways in which Hunt seems to be able to interpret a high correlation coefficient. We shall see that there are other interpretations which do not favor the Nafe reduction.

[9] Nafe's *Os*, the original discoverers of bright and dull pressures, must have used their own definitions of these terms so we were introducing nothing new from the point of view of psychological method.

TABLE IV

SHOWING, FOR EVERY *O*, THE CORRELATION BETWEEN JUDGMENTS OF PERSONAL-IMPERSONAL AND PLEASANTNESS-UNPLEASANTNESS

O	r	P.E.
G	.54	.11
Gr	.57	.10
H	.87	.04
Y	.75	.07

The chief test by which charges of laboratory bias can be met is that of reproducibility of results found in one laboratory by another laboratory. We have failed to reproduce Hunt's high correlations for pressures and affectivity just as Young failed to reproduce Nafe's analyses of the affects into pressures. But even had we been able to get the high correlations Hunt found, we have shown that these can by no means be interpreted in the manner Hunt would have them.

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[10] T. L. Kelley and Eugene Shen, General statistical principles, *The Foundations of Experimental Psychology*, 1929, 849.

Finally the factor of suggestion must not be overlooked as a source of the high correlations. 'Bright' suggests joyous, happy, pleasant states, while 'dull' suggests dead, unpleasant states. So a color judged as pleasant would be more apt to be suggestive of 'bright' pressures than one judged as unpleasant. It is not assumed that 'memory' played any part from the reports under the one set of instructions to those of the other. The 'suggestion' might well be immediate and attach to the color directly. In the same manner 'personal' might suggest pleasantness and 'impersonal' unpleasantness.[11] If a stimulus is pleasant it may be (if an *O* must choose between 'bright' and 'dull' or between 'personal' and 'impersonal') 'bright' rather than 'dull,' 'personal' rather than 'impersonal.' Thus high correlations might well be established between hundreds of pairs of words having no more connection psychologically than the affects and pressures and 'personal-impersonal.'

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[11] It should be understood we did not obtain our high correlation between affects and 'personal-impersonal' by training our *O*s. In fact, they did not understand 'what it was all about' as they afterwards reported. The suggestion we are speaking of we believe to be immediate and has its locus in the instructions, setting of the experiment, and the *O*, particularly in the *O*.

ATTENTION AND BODILY SWAY

By E. B. Skaggs, I. S. Skaggs, and Marta Jardon,
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In 1925 Fearing reported the results of a systematic investigation of the relation between attention and bodily sway,[1] which indicated that the subjects were better able to maintain a given posture of the body when attention was directed away from the task of maintaining it than when attention was directed to that task. These results were in part negated and in part confirmed by Skaggs and Eichkern,[2] in 1928, who found that some subjects swayed less when their attention was directed to the immediate task of muscular coördination, and that others swayed less when their attention was directed to extraneous matters. It was in the hope of explaining these equivocal results that the present study was undertaken.

Method and Procedure

Apparatus. Since the apparatus has already been described,[3] brief mention of it here will suffice. There are three integral parts to it: a body-piece, a meter stick with a cardboard platform attached to the upper end, which is fastened to *S*'s back—the platform projects about 2 in. above his head; a recording sheet,[4] smoked or sensitized paper, which is fastened to the top of the platform; and a stylus, an adjustable overhead rod that moves freely in the vertical direction in a sleeve that prohibits horizontal play, which is brought to bear upon and removed from the sensitized paper as desired.

General procedure. S stood under the overhead piece with his feet in a constant and standard position, *i.e.* with his heels 2 in. apart and turning outward at an angle of 20°. He was instructed to assume a comfortable erect position and to signal by saying "Now" when he had found one that he thought he could maintain. At the signal, *E* brought the stylus to bear upon the sensitized paper and exactly 1 min. later removed it. After an interval of from 1–2 min. the procedure was repeated. All the experiments were made, unless otherwise stated, with eyes open.

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*Accepted for publication May 18, 1931.

[1] F. S. Fearing, Factors influencing static equilibrium, *J. Comp. Psychol.*, 5, 1925, 1-25.

[2] E. B. Skaggs and G. C. Eichkern, Some studies of body sway, *Mich. Acad. Sci., Arts, and Letters*, 10, 1928, 369-79.

[3] Skaggs and Eichkern, *op. cit.*

[4] In the first part of the work we used smoked paper, in the later part we used a sensitized gelatin-film-paper.

Movements of the body were thus graphically recorded. The greatest deviation from the initial position was taken as the measurement of bodily sway. We could not, although that might be highly desirable, measure the total amount of bodily movement with our recording apparatus. All the measurements were made in millimeters.

EXPERIMENT I

Records were obtained in Experiment I under three different instructions: (1) with attention directed to the task of maintaining position (MP); (2) with attention directed to the task of mental multiplication (MM); and (3) with attention directed to the task of reading (AR).

Subjects. Twenty-one *Ss*, 10 women and 11 men, all college students, served in this part of the study. None of the *Ss* had had any previous training in the tests.

Procedure. Every *S* came to the laboratory 5 times. Six records were obtained at every meeting, *i.e.* 2 under every one of the 3 instructions. In all, therefore, 30 records (10 under every instruction) were obtained for every *S*. The order of the tests was rotated at each meeting so as to equalize the effects of practice and other errors due to the temporal position of the tests in the day's series.

Results: (*a*) *Objective.* The data of Table I show that both groups, men and women, are better able to maintain a standing position with attention directed to mental multiplication. The group averages indicate this clearly. If, instead of comparing group averages, we consider the individual cases (not given in the table) we find that 10 of the 11 men, and 7 of the 10 women, sway less when attention is directed away from than when it is directed to the task of maintaining position.

A comparison of results presented in Table I shows, on the basis of group averages, that the *Ss* swayed more under the AR instruction than under the MP. Inspection of individual averages (not given in the table) shows that 7 of the 11 men, and 6 of the 10 women, sway more under the AR instruction.

Under the conditions used in this section of our study, then, we find that *Ss* sway less with attention directed to mental multiplication but more with attention directed to reading, as compared with attention directed to the task of maintaining position.

(*b*) *Introspective.* A study of the introspective reports of the *Ss* indicated that there was great need to define more specifically the condition of directing attention to maintaining position. The instructions to "keep attention directed to the task of maintaining position" were interpreted in various ways by different *Ss*. One kept saying to himself, "Hold fast, steady now. I must keep this position," and so on. Another visualized the overhead piece, translating his kinaesthetic sensations into visual imagery. Another attended to the muscle strains from his leg muscles. Still another watched a spot on the wall. It was obvious that while all of the *Ss* directed their attention away from the task when so instructed there was no uniformity regarding the content of attention when they were instructed to attend to the task of maintaining position.

TABLE I

GROUP AVERAGES UNDER THE DIFFERENT INSTRUCTIONS

		Attention on		
	No.	Maintaining Position (MP)	Mental Multiplication (MM)	Reading (AR)
Men	11	23.9±5.9	18.4±4.1	24.5±6.2
Women	10	30.4±5.3	25.9±4.3	32.2±8.0

EXPERIMENT II

The results of Experiment I demonstrated the necessity of strictly defining the instruction, "attend to maintaining position." With that in mind a second series of experiments was performed.

Procedure. The *Ss* were requested to attend, under the instruction to maintain bodily position, to the strain sensations in the leg muscles and to make their readjustments by carefully noting and reacting to the shifting tensions. This instruction, which is designated as the MP_s instruction, was alternated with the instruction to attend to the task of mental multiplication (MM).

Subjects. Twenty-two college students, 11 men and 11 women, served as *Ss*. Eight records were obtained from every one of them—4 from each instruction at 2 experimental periods. One *S* was started with one instruction, the next with the other instruction, and so on, alternating throughout the group. The tests under both instructions were performed with closed eyes.

Results. Table II, which gives the averages for the group, indicates that there is less sway with attention directed to mental multiplication than with attention directed to maintaining position and strain.

TABLE II

SHOWING GROUP AVERAGES UNDER THE DIFFERENT INSTRUCTIONS (Eyes Closed)

	Attention on	
No. *Ss*	Maintaining Position and Strain (MP_s)	Mental Multiplication (MM)
22	30.6±6.2	27.8±6.9

plication. A study of individual cases, which are not presented, shows that in 14 out of 22 cases the MM instruction results in the better record. Although all of our *S*s directed their attention to the same mental content, the results found in this experiment are substantially the same as those found in the preceding experiment.

EXPERIMENT III

In view of the results of Experiment II it seemed desirable to make a more intensive study of a few individuals who were fairly well trained psychologically and who could give us more trustworthy introspections. While most *S*s swayed less with distracted attention some swayed less with attention directed to the task of maintaining position. The question arose as to whether these differences were due to the fact that we had secured only a small number of records or whether they were indications of important individual differences.

Subjects. Six *S*s (*A*, *J*, *K*, *M*, *N*, and *P*) were used in this study. *A* had had a great amount of training in this work. *M* had had considerable previous training. The other four had had 20 previous tests of bodily sway. After a few preliminary trials 60 records were obtained from every *S*, *i.e.* 20 records under every one of the 3 instructions. Six records, 2 for every instruction, were secured at every meeting. The order of the tests was rotated at each meeting in order to equalize the effects of practice and fatigue.

Procedure. Bodily sway was measured under 3 instructions:-(1) under the MP instruction, as that was defined in the previous section; (2) under the MM_e instruction, *i.e.* under the distraction of easy tasks in mental multiplication (the multiplication of 2-place by 1-place digits); and (3) under the MM_h instruction, *i.e.* under the distraction of difficult tasks in mental multiplication (the multiplication of 2-place by 2-place numbers). In all the distraction experiments *E* called out the numbers and *S* returned the answers orally as soon as solved, when another problem was immediately given him.

Results. Table III presents the means and mean variations for every one of the 6 *S*s. While the averages for the whole group are given we are chiefly interested in the individual averages. *A* and *M* do persistently better under the MP instruction than under the distracted conditions. On the other hand *J*, *K*, and *N* do persistently better in the distracted conditions. *P*, alone, gives uncertain results. He swayed less under the MP instruction than under the MM_e, and more under the MP than under the MM_h.

As a further check upon the reliability of the individual differences we fractionated our data, as shown in Table IV. We have presented the mean for the first and the last 10 records under every one of the 3 instructions. If we are dealing with real and deep-seated individual differences here, the practice effect should not change matters in any material way.

TABLE III

Showing Averages for 6 Trained *S*s

S	Attention on Maintaining Position (MP)	 Easy Problems (MM_e)	 Hard Problems (MM_h)
A	20.8± 5.9	25.7±6.3	22.0± 5.5
J	41.2±12.0	31.4±7.3	35.3± 7.3
K	23.4± 5.0	19.1±4.6	18.1± 5.5
M	26.4±10.3	28.8±6.6	28.3±10.1
N	30.4±12.3	19.5±6.9	16.7± 4.8
P	19.2± 6.1	24.0±8.5	16.8± 5.1
Mean	26.9± 5.93	24.8±3.88	22.9± 5.97

TABLE IV

Showing Averages for First and Last Ten Records

	First ten			Last ten		
S	MP	MM_e	MM_h	MP	MM_e	MM_h
A	20.3	23.1	19.5	21.3	28.3	24.5
J	49.6	31.8	33.1	32.7	31.0	37.5
M	24.4	27.4	27.5	28.3	30.1	29.0
K	24.3	22.3	18.6	22.5	15.9	17.6
N	38.3	19.3	16.6	22.5	18.8	16.8
P	20.9	19.6	15.1	17.5	28.4	18.4

Let us first compare averages for the first and last 10 trials for every *S* under the MP and the MM_e instructions. With the single exception of *P* all the *S*s run true to form in each half of the series. *P* sways less under the MM_e instruction in the first 10 trials but the situation is just reversed in the second 10.

If we compare the averages for the first and last 10 trials for every *S* under the MP and the MM_h instructions we find much the same thing. There are, however, two exceptions. *J* sways less under the MM_h instruction in the first 10 trials, and more in the last 10. *P* sways less under the MM_h instruction in the first 10 trials, and more in the last 10.

All in all, we believe that our data suggest that we are dealing with rather deep-seated individual differences, differences which persist in spite of considerable practice. Most *S*s do better with attention distracted but some do better with attention directed to the task of maintaining bodily position. So much is clear but there yet remains the difficult task of accounting for the fact that some *S*s sway less when their attention is distracted and that others sway less when they attend to the task of maintaining bodily position.

It occurred to us that the explanation of our results might be found in the fact that some *S*s tense their muscles more than others. We proceeded to study this factor in Experiments IV and V.

EXPERIMENT IV

The *S*s in Experiment IV were given the general instruction to attend to maintaining bodily position, and the specific instructions to stand in one series of experiments with arm and leg muscles normally tensed, and in the second series with muscles tensed a little more than normal.

After a preliminary practice series of experiments, during which the *S*s adjusted themselves to the instructions, the main experiments were conducted. The instructions were used alternately; some *S*s beginning with one, and some with the other.

Two groups of *S*s, college students, were used; 15 served with eyes open, and 14 with eyes closed.

Results. The results of Experiment IV are given in Table V. The group averages show that the sway of the body was greater for both groups of *S*s when the muscles were tensed. The individual records (not given in the table) reveal that this relation held for 14 of the 15 *S*s who served with open eyes, and for 8 of the 14 *S*s who served with closed eyes.

EXPERIMENT V

A final experiment was made with 4 trained *S*s. Twenty records were secured for every *S* under each of the instructions of Experiment IV. The results are summarized in Table VI. Three of these *S*s did much better in the normally tense condition whereas one, *M*, did better in the more tense condition.

These data indicate, we believe, that the relative tension of the muscles is an important factor in accounting for the differences which we have found between individuals in our study. The tentative suggestion is offered that most *S*s sway more when attention is directed to the task of maintaining position because, in part at least, they tense their muscles more under that condition than when their attention is distracted. Those *Ss* who sway more with distracted attention may tense their muscles more under this condition. The question of the relation between direction of attention and degree of muscle tension needs systematic investigation in which a more exact and objective method of measuring muscle tension is used.

TABLE V

GROUP AVERAGES WITH EYES OPEN AND CLOSED

		Muscles	
Eyes	No. *Ss*	Normal	Tensed
Open	15	27.3±7.3	37.3±5.5
Closed	14	35.0±7.6	37.4±3.7

TABLE VI

AVERAGES FOR HIGHLY TRAINED *Ss*

	Subjects			
Muscles	*A*	*J*	*L*	*M*
Normal	20.1±5.1	26.2±8.2	25.7±9.0	22.4±6.1
Tensed	26.4±5.7	29.8±5.8	28.0±8.3	19.0±5.3

SUMMARY

Measuring bodily sway in terms of the greatest amount of deviation from an initial position during a unit of time (1 min.) the following conclusions are indicated by our data:

(1) The majority of unpracticed *Ss* sway less when their attention is entirely distracted from the processes of coördinating the body. A few sway more when attention is distracted.

(2) Our practiced *Ss* are evenly divided; one group does persistently better with attention distracted from the task of coördinating the body whereas the other group does better when attention is directed to the task of maintaining body position.

(3) Practically all *Ss*, unpracticed and practiced alike, do better when standing with "just normal muscular tension" than when standing with "greater than normal tension."

(4) The suggestion is offered that varying degrees of muscular tension, associated with the direction of attention, may be an important factor in accounting for the individual differences which we have found.

THE DEPENDENCE OF EYE-HAND COÖRDINATIONS UPON EYE-DOMINANCE

By F. H. Lund, Temple University

Most of the literature dealing with preferential usage, or with dominance of one side of the body over the other, has been concerned with its most conspicuous feature; namely, handedness. It is to this form of dominance, again, that chief consideration has been given in the theories advanced to account for the genesis of body asymmetries.[1] In addition to handedness only two other forms of functional dominance—eyedness and veering—have been given serious consideration. The data and findings, as related to these three forms of dominance, *i.e.* handedness, eyedness, and veering, are summarized briefly.

Handedness. Dextrality, or right-handedness, according to the earliest writers on the subject, characterizes 94% to 98% of the population, and sinistrality, or left-handedness, 2% to 6% of the population. More recent investigators have regarded these estimates as too high for right-handedness. It is maintained that the very low estimate of about 5% left-handedness is based solely on the more obvious dextral and sinistral usage, and that many of the congenitally sinistral (particularly the mildly sinistral) have been changed to dextral because this is the more frequent, and because dextrality has the advantage in a world which is largely dextral. Jordan, who was one of the first to depart from the earlier estimates, and the first to suggest that handedness follows Mendelian principles, maintained (in 1914) that right-handedness is now present in a ratio of 4:1.[2] Ramalay,[3] subsequent to Jordan's statement, placed the ratio at 6:1. Von Bardeleben,[4] in 1914, gave the number of children who have been changed from left-handed to right-handed as 26%. Parson,[5] working from his data on eyedness, reaches the same conclusion, maintaining that the natively right-handed and the natively left-handed are probably present in the ratio of 3:1, or tend to approximate this in accordance with Mendelian principles. As far as actual usage is concerned, a conservative estimate would probably place the distinctly right-handed at 90%, the ambiguous and ambidextrous at 5%, and the distinctly left-handed at 5%.

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*Accepted for publication June 15, 1931.

[1] One of the best summaries of these theories is to be found in B. S. Parson, *Lefthandedness*, 1924, 2 ff.

[2] H. E. Jordan, Hereditary lefthandedness, *J. Genet.*, 4, 1914, 67.

[3] F. Ramalay, Inheritance of lefthandedness, *Amer. Nat.*, 47, 1914, 730.

[4] K. von Bardeleben, Lefthandedness, *J. of Hered.*, 5, 1914, 312.

[5] Parson, *op. cit.*, 99 ff.

Eyedness. Ocular dominance, or the tendency for one eye to become the directing and controlling eye wherever close coördination of eye and hand is required, seems first to have been recognized by Humphrey, who referred, in 1861, to "the correspondence of function between eye and hand."[6] In 1881, LeConte stated that "people are right-eyed as well as right-handed, and left-eyed as well as left-handed."[7] Gould, who has written extensively on this subject, contends that "handedness depends on which is the better seeing eye."[8] Parson examined 877 school children for eyedness and found that while eyedness approximates the Mendelian ratio, handedness, so far as actual usage is concerned, does not. He concludes that "to the extent that virtually all the righteyed are at the same time righthanded, we are justified in believing that the natively lefteyed, irrespective of their present manual preference, were all originally lefthanded."[9] Repeating Parson's tests, Cuff has more recently arrived at similar conclusions. A report of his study states that "the manoptometer indicated that 20% to 30% of the children were probably left-handed natively, and all were left-eyed except one case—also that 100% of the right-eyed were found to be right-handed."[10]

Veering. A third form of functional dominance recently investigated is the tendency to veer, or to turn either right or left according to lateral bias, when normal orienting cues are absent. The consistency of right-veering or left-veering in particular individuals indicates a distinct form of dominance. According to Asa Schaeffer this phenomenon is not peculiar to humans. In his conclusions, following wide observations upon lower forms, he states summarily that "no forward moving organism has been found which does not move in a spiral path when there are no orienting senses to guide it."[11] The veering or spiralling tendency of humans was tested in an earlier experiment by Lund.[12] The results of this study indicated no close relationship between veering tendency and handedness, or between veering and eyedness. On the other hand, consistent veering in a given direction from intended line of movement on the part of blindfolded *S*s, showed a high correspondence with structural dominance on the side opposite to the line of deviation. Thus, those who were right-veering showed more frequent structural dominance of the left side, and *vice versa.* Right-veering, indicating left-dominance, was more frequent than left-veering. This greater frequency of left-dominance is paralleled by more frequent structural dominance (greater length) of the left leg. Most of the subjects with longer left leg were found to have a longer right arm. This fact of 'crossed symmetry'

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[6] G. M. Humphrey, *The Human Hand and the Human Foot*, 1861, 8.

[7] J. LeConte, *Sight*, 1881, 107.

[8] G. M. Gould, *Right-Handedness and Left-Handedness*, 1908, 98.

[9] Parson, *op. cit.*, 114.

[10] N. B. Cuff, from a report on the 25th annual meeting of the Southern Society for Philosophy and Psychology, this JOURNAL, 42, 1930, 459-460.

[11] Asa Schaeffer, Spiral movements in man, *J. Morph. and Physiol.*, 45, 1928, 297 ff.

[12] F. H. Lund, Physical asymmetries and disorientation, this JOURNAL, 42, 1930, 51-62.

(longer arm on one side coupled with longer leg on the other) has been reported by Hasse and Dehner,[13] and by Guldberg.[14]

That structural and functional dominance do not necessarily go together may be seen from the fact that, despite greater frequency of structural dominance of the left leg, most people are, nevertheless, right-footed, and from the fact that functional dominance of the right eye does not always mean that this eye is structurally superior to the left.

PROBLEM

The purpose of the present investigation was to secure further data upon the nature and interrelation of dominant traits. The two forms of dominance especially under consideration were those of handedness and eyedness. To determine the interdependence of these under actual test conditions seemed desirable, particularly in view of the cause and effect relationship which is said to exist between them. Before proceeding to a more precise statement of the problem it will be necessary to consider briefly some of the more pertinent physiological data relating to eye and hand adjustments.

Man's vision is, in the main, binocular. It is binocular particularly in adjustments to distance, to shape, and to form. It is monocular in adjustments to objects seen in indirect vision. In most of the lower biological forms, on the other hand, vision is distinctly monocular, since the eyes are so far apart and the visual axes so divergent that there can be no common field of view for the two eyes. In such cases the eye on the right side controls the movements of the right side of the body, and the eye on the left controls the movements on the left side. "This harmony of function," says Parson, "survives in man at least to the extent that righteyedness accompanies righthandedness, and lefteyedness accompanies lefthandedness. . . . With these facts in mind we are prepared to learn from pathology that the all-important motors centers for the hand and arm and for some of the more expert visual operations on the preferred side lie close together in the left hemisphere for the righthanded, and in the right hemisphere for the lefthanded."[15] This grouping of faculties in one hemisphere would seem to be true for handedness, eyedness, and for written and spoken speech.

From the above we see that eye-dominance is closely linked with monocular space perception and with the general problem of sided-ness. Space perception, we have noticed, is monocular in the case of all objects seen indirectly, for the reason that such objects will appear double when viewed with both eyes. Accordingly, adjustments to these objects are guided by one eye only, the dominant eye. Particularly is this true in the case of eye-hand adjustments calling for close coördination. In this event it is the *hand* that is seen indirectly, since the eyes are trained upon the point of adjustment rather than upon the adjusting member. That the directing and controlling eye should be on the same side as the controlling member would seem to be in the interest of efficiency.

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[13] C. Hasse and M. Dehner, Unsere Truppen in körperliche Beziehung, *Arch. f. Anat. u. Entwicklungsgesch.*, 16, 1893, 249-256.

[14] G. A. Guldberg, Über die morphologische und functionelle Asymmetrie der Gliedmassen beim Menschen und bei den hoheren Vertebraten, *Biol. Centralbl.*, 16, 1896, 806-813.

[15] Parson, *op. cit.*, 22.

But now might it not be well to determine just how far these general facts of monocularity enter into specific adjustments, and, also, how far efficiency will be impaired if the so-called' natural grouping of faculties' is broken up by placing the individual in the position where he must use the non-dominant eye to guide the dominant hand? It was to test this general proposition that the experiment herein described was undertaken.

METHOD AND PROCEDURE

Since the familiar aiming test or target test conforms with all the requirements of precise eye-hand coördinations, it was introduced as a measure of the phenomenon under consideration. The target blank (see Fig. 1) was fastened to the wall at a height corresponding to the *S*'s shoulder. *S* was placed with his shoulder squarely in front of the target, and at such a distance that his pencil would just strike the target when his arm was fully extended. He was told to strike in time with the beat of a metronome (set to beat 90 times per min.), following a two-beat rhythm, so that the pencil would hit the target at one beat and would be drawn back at the next. He was advised to make full, smooth strokes, bringing the hand clear back to the shoulder each time. The crosses were struck at successively and in the order indicated by the numbers in Fig. 1.

Every *S* was given 9 tests: 3 with both eyes open; 3 with the right covered by means of a small conically shaped cap; and 3 with the left eye similarly covered. Following the target tests every *S* was tested for eyedness, using as a measure the monoptometer, a full description of which is given in a previous article.[6]

Of the 247 *Ss* used in the experiment, 128 were juniors or seniors in high school, and 199 were college students. In analyzing the data the *Ss* were grouped according to handedness and eyedness as 'right-eyed dextrals,' 'left-eyed sinistrals,' 'right-eyed sinistrals,' 'left-eyed dextrals,' and 'binocular and ambiguous.'

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[6] Land, The monoptometer: A new device for measuring eye-dominance, this JOURNAL, 44, 1932, 181 ff.

RESULTS

The scores for the various groups are shown in Table I. They represent the average errors made on the target test (1) when using both eyes. (2) when using the dominant eye, the non-dominant being covered, and (3) when using the non-dominant eye, the dominant being covered. These scores were arrived at in the following manner. The total amount of mm. deviations from the point aimed at in ten successive thrusts was first determined for each *S*. Then the average record for every *S* in the three types of test was determined. From these averages a grand average for all *S*s belonging to a particular group was calculated for each type of test. Thus, it will be clear that in the case of the scores entered in Table I a high number really indicates a poor record, since this number represents the total amount of deviation (measured in mm.) in 10 successive thrusts.

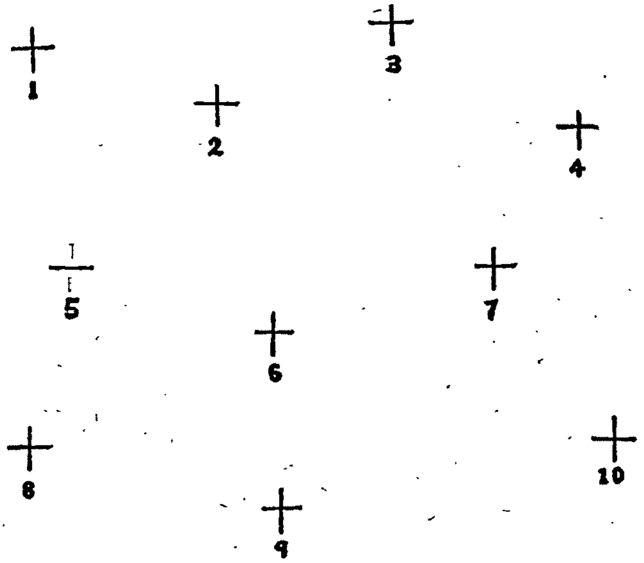

FIG. 1. THE TARGET BLANK
(Actual Size)

The results are interesting in several respects. First, in the fact that the best scores are made with both eyes open—this, despite the contention that we are in effect one-eyed in such adjustments and that the non-dominant eye really acts as an interference. The second point of interest is the decided and consistent advantage which the dominant eye—irrespective as to whether it is the right or the left—has over the non-dominant eye in the case of the unmixed groups, that is, the right-eyed dextrals and the left-eyed sinistrals. Third, this advantage of the dominant eye over the non-dominant is not apparent in the case of the left-eyed dextrals, which is a mixed group.

TABLE I

SHOWING THE RESULTS OF THE TARGET OR AIMING TEST AND THE RELATION WHICH THESE BEAR TO EYEDNESS AND HANDEDNESS

		Eye used					
Classification	Percent	Both		Dominant		Non-dominant	
		Score	P.E.	Score	P.E.	Score	P.E.
Right-eyed dextrals	69.1	55.1	1.9	61.2	2.2	67.1	2.4
Left-eyed sinistrals	4.2	55.4	2.0	59.6	1.9	62.2	2.0
Right-eyed sinistrals	.8	56.2		60.9		63.1	
Left-eyed dextrals	21.1	55.0	1.8	61.7	2.7	61.9	3.1
Binocular and ambiguous	4.7	59.7	2.6	69.5 (right)	3.2	67.1 (left)	3.3

In general, then, there can be no doubt about the fact of dominance. It is just as apparent from these tests of efficiency in actual eye-hand adjustments, as it is in the case of the monoptometer tests. But it must be equally apparent (1) that we are binocular as well as monocular even in so simple an adjustment as the one under consideration, and (2) that highest efficiency is achieved when the two forms of vision are permitted to coöperate, as they apparently do in such situations. It would probably be futile to try to single out the aspects of the perceptual process which are binocular and the ones which are monocular, but it is likely that in a particular situation such as the one represented in the aiming test, binocular vision functions chiefly in judging distance, and monocular vision in guiding the movements of the hand or objects seen in indirect vision. In any event, it would be safe to conclude that there are probably very few if any adjustments of this type which do not involve both types of vision. It would appear equally certain that the use of both eyes under these conditions has the advantage. There was not a single subject for whom this was not true.

The advantage of the dominant eye over the non-dominant eye, while quite definite and decided when averages are considered, did not always hold for individual cases. About one out of every four showed a margin of superiority in favor of the non-dominant eye. With a larger number of tests for such subjects this advantage might be reversed. In the case of the left-eyed dextrals the non-dominant eye yielded the better score in almost as many cases as the dominant eye. This would seem to indicate that the right eye, though not the dominant eye in the case of these subjects, has nevertheless been brought more into use than might otherwise have been the case, since it is the nearer eye so far as the functionally dominant hand is concerned. The data in this case point definitely to the advantage of consistent lateralism.

SUMMARY

The results of the target test, while supporting the general fact of ocular dominance as indicated by monoptoscopic tests, do not support the theory that vision is distinctly monocular in all adjustments calling for high precision of eye and hand, or requiring that the coördinating member be guided through indirect vision. That the dominant eye plays a more important rôle than the non-dominant is evident from the fact that the scores made with the dominant eye (the non-dominant being covered) were better than those for the non-dominant eye. This was true irrespective of which eye was dominant. That binocular as well as monocular vision enters into eye-hand adjustments of this type is apparent from the fact that the best scores were made when both eyes were in use. Thus, ocular dominance is not the simple phenomenon it would at first seem to be. It is likely that we are not distinctly binocular or monocular with respect to any function. That the left-eyed dextrals should have made about equally good scores with the non-dominant as with the dominant eye is interesting in that it indicates that the shift from congenital left-handedness to trained right-handedness results in greater training of the right eye—on account of its advantage of position—than might otherwise be the case.

PRIMACY AND RECENCY IN THE LEARNING OF VISUAL DIAGRAMS

By LELAND W. CRAFTS, New York University

The object of the present experiment was to discover whether primacy and recency—terms referring solely to the temporal position of members of a series—would have significance in the learning of certain visual diagrams. The experimental literature relevant to primacy and recency is large and covers a wide range and variety of material. The present investigation differs from previous experiments in several respects. It differs from the great majority of the earlier studies in employing perceptual material instead of verbal or motor. It differs from the experiments dealing with perceptual materials in that the series to be learned consisted of the successive parts of a single diagram rather than sequences in which every member was a separate and distinct unit. It differs from all previous studies, in so far as the writer is aware, in having the *S*s graphically reproduce the materials presented. In brief, the present experiment differs from those that have gone before in both the nature and the treatment of the material used.

It is, we believe, of importance to see whether current generalizations as to the factors of influence in learning may be extended to new situations and new types of material. If they can be, then they gain in certainty and range of application; if they cannot be, then some sort of amendment or qualification is of course suggested.

PROCEDURE

Subjects. The *S*s were members of 8 classes in elementary psychology. The total number used was 324.

Materials. The materials for learning were visual diagrams identical with those used in a previous experiment and described in detail in an earlier number of this JOURNAL.[1] It may, therefore, suffice here to say that they were: (1) Sixteen circles arranged as irregularly as possible and occupying 16 of 64 small squares into which a large square had been divided. (2) Sixteen separate, unrelated lines, diagonal, horizontal and vertical, also arranged as irregularly as

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*Accepted for publication July 17, 1931.

[1] L. W. Crafts, Whole and part methods with visual spatial material, this JOURNAL, 44, 1932, 527-528.

possible and connecting 32 of 49 points, the latter all equidistant from each other. (3) A single, complete figure, composed of 16 lines also connecting 32 of 49 equidistant points and made as irregular and asymmetrical as possible. An equal number, namely 4, of circles, unrelated lines, or lines that were elements of a figure was allotted to every quarter of every diagram. By 'quarter' is meant the quarters that would be formed if a diagram were bisected horizontally and vertically by two lines at right angles to each other. These quarters will hereafter be called 'parts' and will be separately identified as follows: The upper left quarter will be called Part A, the upper right Part B, the lower right Part C, and the lower left Part D. An effort was made to have all four quarters about equal in difficulty.

Presentation. The material was displayed in the manner described in the earlier experiment referred to above. Rotation of a large sheet of cardboard, from which an area had been cut of a size such as would show only a quarter of the diagram at a time, made possible the exposure of every part separately for the predetermined period.

The exposure times for the various materials were as follows: circles, 23 sec. per quarter; lines, 30 sec.; and figures, 10 sec. These times permitted only partial learning for a majority of the *S*s. The presentation was continuous, *i.e.* there was no interval whatsoever between the exposure of any two successive parts.

Rotation of parts. In order to eliminate the varying difficulty of the parts as a disturbing factor, every diagram was shown to at least four different groups of *S*s. To one of these, hereafter called Group 1, Part A was presented first, then Parts B, C, and D in that order. For Group 2 the order was Parts B, C, D, and A. For Group 3, it was Parts C, D, A, and B; and for Group 4 it was D, A, B, and C. Hence every one of the four parts occupied once every one of the four possible temporal positions in the series of parts. The presentation, it should be noted, was always in a clockwise direction.

Reproduction and scoring. The *S*s received blank copies of the diagrams and instruction sheets briefly describing the nature of the material and the procedure to be followed. They were also informed as to the order in which the parts would be shown. When all four parts had been presented, the *S*s immediately reproduced what they could of the diagram. They were allowed about 3 min. to make the reproduction. Every correct detail was scored 1. No partial credits were given.

RESULTS

The results are given in Table I. The part presented first to a group is called Part 1, regardless of whether it was Part A, B, C, or D; similarly the parts presented second, third, and fourth or last are called Parts 2, 3, and 4 respectively. The letters in parentheses beside the averages show which quarter every part was. N was 'corrected' by eliminating all *S*s who made the perfect score of 16 on the whole diagram, for obviously such scores do not permit any differential learning of the various parts to show itself. The averages given are for the corrected N only.

Inspection of Table I shows that every group made higher scores on Parts 1 and 4 than on Parts 2 and 3 except as follows: with Lines, Group 2; with Figure 1, Groups 1, 3 and 4; with Figure 2, Group 4. The numerous exceptions obtained with Figure 1 are due obviously to the fact that Part A was much the easiest of the four parts; every group secured its highest score on that part regardless of the temporal position in which it was shown. For this reason Figure 2 with parts more nearly equal in difficulty was devised and shown to four new groups entirely without experience in learning these diagrams. With this figure all groups except Group 4 made their highest score on Parts 1 and 4.

TABLE I

GIVING THE AVERAGE SCORES BY PARTS, AND FOR THE WHOLE DIAGRAM, FOR ALL GROUPS AND ALL MATERIALS

	Group	Original N	Corrected N	Part				Whole Diagram
				1	2	3	4	
Circles	1	37	29	$3.3^{(A)}$	$3.0^{(B)}$	$2.6^{(C)}$	$3.1^{(D)}$	12.0
	2	48	33	$3.6^{(B)}$	$3.4^{(C)}$	$2.5^{(D)}$	$3.5^{(A)}$	13.0
	3	38	34	$3.3^{(C)}$	$2.4^{(D)}$	$3.0^{(A)}$	$3.5^{(B)}$	12.2
	4	47	28	$3.0^{(D)}$	$2.8^{(A)}$	$2.9^{(B)}$	$3.8^{(A)}$	12.5
	All	170	124	3.30	2.90	2.75	3.48	12.43
Lines	1	36	35	$2.2^{(A)}$	$1.5^{(B)}$	$1.5^{(C)}$	$2.5^{(D)}$	7.7
	2	45	39	$2.3^{(B)}$	$2.7^{(C)}$	$2.8^{(D)}$	$2.7^{(A)}$	10.5
	3	36	35	$2.7^{(C)}$	$2.2^{(D)}$	$2.4^{(A)}$	$2.8^{(B)}$	10.1
	4	46	44	$3.1^{(D)}$	$2.1^{(A)}$	$2.2^{(B)}$	$3.2^{(C)}$	10.6
	All	163	153	2.58	2.13	2.23	2.80	9.73
Figure 1	1	34	30	$3.4^{(A)}$	$2.1^{(B)}$	$1.5^{(C)}$	$1.4^{(D)}$	8.4
	2	41	33	$3.0^{(B)}$	$1.9^{(C)}$	$1.8^{(D)}$	$3.7^{(A)}$	10.4
	3	35	31	$2.1^{(C)}$	$1.5^{(D)}$	$3.2^{(A)}$	$2.2^{(B)}$	9.0
	4	46	37	$2.3^{(D)}$	$2.5^{(A)}$	$2.2^{(B)}$	$1.8^{(C)}$	8.8
	All	156	131	2.70	2.00	2.18	2.28	9.15
Figure 2	1	43	43	$1.7^{(A)}$	$1.1^{(B)}$	$0.9^{(C)}$	$1.3^{(D)}$	5.0
	2	44	44	$1.5^{(B)}$	$1.3^{(C)}$	$1.3^{(D)}$	$1.4^{(A)}$	5.5
	3	30	30	$1.5^{(C)}$	$1.3^{(D)}$	$0.9^{(A)}$	$1.4^{(B)}$	5.1
	4	37	37	$1.4^{(D)}$	$1.3^{(A)}$	$1.2^{(B)}$	$1.2^{(C)}$	5.1
	All	154	154	1.53	1.25	1.08	1.33	5.18

The average scores for all groups combined show that without exception with all materials the scores made on Parts 1 and 4 were higher than those made on Parts 2 and 3. With the Circles and the Lines, the score on Part 4 was slightly higher than that on Part 1; with both figures, however, Part 1 obtained the higher score.

Since the differences between the scores on the various parts were necessarily slight, it seemed to the writer preferable not to compute their S.D. but rather to depend upon the consistency and uniformity of the results for indication of their validity.

DISCUSSION

The results show that primacy and recency were significant in the learning of the material employed in the sense that the parts presented first and last were better learned than those presented in the two intermediate temporal positions. For recency to have such value is a finding too familiar to require further comment here. As to the value here indicated for primacy, our results are in harmony with those of Robinson and Brown,[2] Jenkins and Dallenbach,[3] Sauer,[4] and Willoughby,[5] and in opposition to those of Thorndike,[6] and Burnett and Welch.[7] In connection with the question as to its significance, Burnett and Welch, summarizing the results of their experiment with unpronounceable nonsense syllables, write as follows: "There was good reason to regard Primacy as a factor in association until experimental control was obtained over the *O*'s tendency to 'think backward' during the exposure of a series to be remembered. When this tendency was prevented, by the coöperation of the *O*, the alleged effect of primacy disappeared."[8] In the present experiment, however, it seems probable that the nature of the material, the continuity of the presentation and the immediacy of the reproduction must have prevented much if not all of any such thinking backward. Hence the value of primacy here indicated is probably not so explainable, but is perhaps due rather to an 'initial set' towards the first member of a series as Jenkins and Dallenbach suggest,[9] or to the absence of 'proactive inhibition' as Foucault believes.[10]

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[2] E. S. Robinson and M. A. Brown, Effect of serial position upon memorization, this JOURNAL, 37, 1926, 538-552.

[3] J. G. Jenkins and K. M. Dallenbach, The effect of serial position upon recall, *ibid.*, 38, 1927, 285-291.

[4] F. M. Sauer, The relative variability of nonsense syllables and words, *J. Exper. Psychol.*, 13, 1930, 235-246.

[5] R. R. Willoughby, Incidental Learning, *J. Educ. Psychol.*, 20, 1929, 671-682.

[6] E. L. Thorndike, The influence of primacy, *J. Exper. Psychol.*, 10, 1927, 18-29.

[7] C. T. Burnett and G. B. Welch, Is primacy a factor in association-formation?, this JOURNAL, 35, 1924, 396-401.

[8] *Ibid.*, 401.

SUMMARY

The purpose of the experiment was to discover whether primacy and recency would have significance in the learning of certain visual diagrams. Irregular arrangements of circles and of lines and irregular figures were divided into four parts. These parts were presented once, in a continuous series; immediate graphic reproduction followed. The factor of the varying difficulty of the parts was controlled by employing a number of groups sufficient to permit every part to be shown in every one of the four possible temporal positions. The results show that with no important exceptions the groups secured higher average scores on the parts presented first and last than they did on those occupying the two intermediate positions. Hence the significance not only of recency but of primacy as well for the learning of this visual material under the experimental conditions here employed seems to have been demonstrated, and the scope of the current generalizations concerning their value may to this degree be extended.

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[9] Jenkins and Dallenbach, *loc. cit.*

[10] M. Foucault, Les inhibitions internes de fixation, *Année psychol.*, 29, 1928, 92-112.

THE SIGNIFICANCE OF EARLY PERSONAL HISTORY FOR CERTAIN PERSONALITY TRAITS

By CHARLES K. A. WANG, University of Chicago

The purpose of this investigation is to make an exploratory study of the significance of selected items of fact in one's early personal history for certain of his personality traits.[1] It is an attempt to answer the question, "What facts in a person's early life are significant in the formation of some of his personality traits?" In doing so, we are interested not so much in individual cases as in the group differences of the various personality classifications.

For information on early personal history a special questionary was prepared. No attempt was made to have it exhaustive; care was taken, however, to include principally items which could be answered with a minimum of subjective judgment.

For information regarding the personality traits, the following tests were used: (1) Allport's Ascendance-Submission Test;[2] (2) Freyd's Test of Introversion-Extraversion,[3] (3) Moss's Test of Social Intelligence,[4] (4) Pressey's X-O Test of Emotionality,[5] and (5) Otis's Test of Suggestibility.[6]

Nearly 600 undergraduate students in the University of Chicago provided data. After eliminating those whose records were incomplete and those over 25 yr. of age, a total of 358 cases, 203 men and 155 women, remained which were used as the basis of this study.

The Otis test of suggestibility was given to the *Ss* during one of their class periods. The other tests, together with the personal history questionary, were permitted to be filled out at home without supervision, and returned later.

Personality traits. For the sake of economy of space, the distributions of raw scores on the 5 personality tests are not here presented. It was found, however, that in most cases our central tendencies differed considerably from those given by the respective authors for comparable groups, *i.e.* for college students. Table I gives a comparison of the median scores for our *Ss* and those given by the respective authors.

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[1] This paper is abbreviated from a doctoral dissertation which is on file, under similar title, in the University of Chicago Library. For a copy of the questionary used in this study and for a detailed presentation of the data, the reader is referred to the thesis which may be borrowed under the usual courtesies of library exchange. The writer is indebted to Professor F. A. Kingsbury for suggesting the problem and for advice and criticism during the progress of the study.

[2] G. W. Allport, A test for ascendance-submission, *J. Abn. & Soc. Psychol.*, 23, 1928, 118-135.

[3] M. Freyd, Introverts and extroverts, *Psychol. Rev.*, 31, 1924, 74-87.

[4] F. A. Moss and T. Hunt, Ability to get along with others, *Indust. Psychol.*, 1, 1926, 170-178.

[5] S. L. Pressey and O. R. Chambers, First revision of a group scale designed for investigating the emotions, with tentative norms, *J. Appl. Psychol.*, 4, 1920, 97-102.

[6] M. Otis, A study of suggestibility of children. *Arch. Psychol.*, 11, 1924, (no. 70), 67-93.

It is not easy to explain all the discrepancies between our group and those of the different authors. In the cases of the ascendance-submission and introversion-extraversion tests, the differences are not so notable. On Pressey's test of emotionality, where the discrepancy is the greatest, it may perhaps be explained in several ways: first, Pressey's norms were obtained about 10 years before ours, and social attitudes about what is right and wrong, pleasant and unpleasant, may have changed sufficiently to affect the scores on his test; secondly, Pressey's group was presumably from smaller cities and rural districts, whereas ours are preponderantly of urban background; thirdly, Pressey's norms were based on only 114 cases, whereas we have 358. On a test whose total score may range theoretically from zero to 500, and empirically from 45 to 339, 114 cases are scarcely enough to establish reliable norms.

TABLE I
Comparison of Median Scores of College Students on Five Personality Tests

Test		This Study		Authors' Norms	
		No. *Ss*	Median	No. *Ss*	Median
Allport A-S	(men)	203	− 0.55	727	0.00
	(women)	155	13.25	120	15.40
	(total)	358		847	
Freyd I-E*	(men)	203	−11.23	100	−11.41
	(women)	155	−10.30	100	−11.12
	(total)	358	−10.85		
Pressey X-O	(men)	203	166.10	56	
	(women)	155	171.36	58	
	(total)	358	167.05	114	230
Moss Soc. Int.	(men)	203	119.74		109
	(women)	155	123.02		115
	(total)	358	121.48	1217	111
Otis Sug.	(men)	203	74.15		
	(women)	155	72.20		
	(total)	358	73.50	83	83.26

*For comparison see E. Heidbreder, Measuring introversion and extraversion, *J. Abn. & Soc. Psychol.*, 21, 1926, 120-138.

On Moss's social intelligence test, the discrepancy may be partly accounted for by the difference in administering the test, since our *Ss* were allowed to fill in the blanks at home.

On the Otis test of suggestibility a very marked difference in the central tendency exists between our group and Otis's standardization group. No explanation seems available, and a search for other reports on the test yielded nothing.

In an effort to discover the interrelations among the personality traits as measured by the tests, coefficients of correlation were computed for each test with every other. The *r* are presented in Table II. It will be noticed that there is some difference between *r* for the same traits in men and in women, with the single exception of that between suggestibility and introversion-extraversion.

In all but two cases, the *r* between the tests for men are higher than those for women. Also, most of the *r* are extremely low, except the one between ascendance-submission and introversion-extraversion for men (−0.51), and the one between emotionality and introversion-extraversion for women (0.31). It must be admitted that all these *r* prove little, since little is known positively about the validity of the tests. Thus whatever findings may result from our investigation will always be subject to this limitation.

TABLE II

Intercorrelations Between the Five Personality Tests.

Test	(A-S)	(I-E)	(X-O)	(S-I)
I-E (men)	−.51±.03			
I-E (women)	−.19±.05			
X-O (men)	−.17±.05	.20±.04		
X-O (women)	−.10±.05	.31±.05		
Soc. Int. (men)	.00±.05	.11±.05	−.21±.04	
Soc. Int. (women)	.16±.05	.01±.05	−.06±.05	
Sug. (men)	.15±.05	−.10±.05	−.20±.04	.22±.04
Sug. (women)	−.04±.05	−.07±.05	−.03±.05	.13±.05

Early personal history. The questionary on early personal history consists of 48 questions, grouped into 5 parts according to the type of instructions necessary and to the chronological reference of the questions.

Part I asked for information as to the number of siblings in *S*'s family, his own position among them, and the number of step- or foster-brothers and sisters.

Part II asked for father's and mother's age at *S*'s birth; in case either parent were dead at the time of answering the questionary, the age of *S* at the time of the death; in case the parents were divorced, *S*'s age at that time; and the person with whom he lived afterwards; age at re-marriage of either parent; other adults living in the home before *S* was 6 yr. old; father's occupation; physical defects of *S*, their cause and the age at which they were acquired; time spent at kindergarten; age at beginning grade school; number of grades skipped or repeated; years needed to finish high school; number of teachers, and favorite and most disliked subjects in high school.

Part III referred to the period from birth to 7 yr. old. It asked whether the mother had been an active business or professional woman; how *S* had been disciplined by his father and by his mother; how much freedom his parents allowed him; what was the type of religious surroundings at the home; the condition of *S*'s health, the predominating type of play, opportunities for companionship with children of his own age, and for travel.

Part IV referred to the period between 7 and 12 yr. of age. It dealt with the extent to which *S* read for pleasure, participated in games at school, attended religious meetings, and had companionship with children of his own age; the nature of his relationship with such other children (*e.g.* whether admired or teased); the extent to which he partook of social amusements; the source of his spending money, and the kind of school he attended.

Part V referred to the period from 12 to 16 yr. of age. It dealt with the extent to which *S* indulged in reading outside of school requirements, and the nature of such reading; the extent of his participation in school games, and in religious activities; the extent of his acquaintances and friends of the same sex, and of the opposite sex; the nature of his relationship with schoolmates or playmates; the types of amusement most indulged in, and the extent to which *S* worked for pay.

In tabulating answers to the questionary it was deemed necessary to group certain ones into convenient categories, or classes. For example, the occupation of the father is grouped under: (1) executive, (2) professional, (3) merchant-salesman, (4) clerk-labor, and (5) farmer; the ages of the parents at time of *S*'s birth are grouped into: (1) under 25, (2) 26 to 30, (3) 31 to 35, and (4) 36 or over; and so on for other questions. After thus grouping the answers to certain questions, the total number of answer-variables used in tabulating the results was reduced to 208. The tabulation was based wholly on frequency, and no effort was made to evaluate or rate any of the answers in any other respect. A frequency distribution of the answers by all the *S*s to the complete questionary is not presented here because the table would contain facts which are mainly of sociological and educational rather than of psychological value.

Results

(*1*) *Personal history vs. ascendance-submission.* In considering the relationship between early personal history and personality traits our method was to trace from the differences in the personality traits to the corresponding differences in the early history variables. To do so we needed simply to differentiate the highest and lowest groups with reference to each personality trait, and to ascertain the corresponding group differences, if any, in the responses to the early personal history questionary. Thus, in showing the relationship between the early history and ascendance-submission, we selected the highest and lowest 20% of men on the A-S test, (repeating the process for women) and compared the frequencies of the responses of each group to each of the questions in the early personal history. Only those answer-variables whose total frequency was over 10 and for which one group was more than twice as large as the contrasted group were considered significant. Most promising for the differentiation of ascendance and submission are such facts in the early history as the following:

For ascendance.
Reading omnivorously for pleasure (men: 23 A, 13 S; women: 21 A, 12 S);
Participating in games at every opportunity (men: 18 A, 10 S; women: 18 A, 8 S);
Being admired by associates (men: 24 A, 4 S; women: 18 A, 4 S).
For submission.
Favoring languages in high school (men: 1 A, 9 S; women: 6 A, 16 S).
Having limited friendship with own sex (men: 1 A, 10 S; women: 2 A, 11 S).

(*2*) *Personal history vs. introversion-extraversion.* Inasmuch as the same test of introversion-extraversion and the same questionary on early personal history were used for both sexes, we are able here to consider the relationship between the trait and the history jointly as well as separately for the two sexes.

The variables which seem particularly promising for differentiating introversion-extraversion are the following:

For introversion.

Having no, or just one or two, playmates (18 I, 3 E).
Indulging in social amusements only when urged (15 I, 3 E).
Practically never indulging in social amusements (10 I, 2 E).
Participating in games only when asked, or practically never (25 I, 2 E).
Having only a few intimate friends among own sex (20 I, 2 E).
Having practically no friends among opposite sex (25 I, 6 E).
Going to shows alone for recreation (23 I, 8 E).

For extraversion.

Having many playmates (9 I, 25 E).
Participating in games at every opportunity (10 I, 27 E).
Being admired by associates (6 I, 15 E).

The variables of particular value in differentiating I-E in women are:

"No consistent effort" in discipline by mother (10 I, 2 E).
"Few kinds" of religious participation (17 I, 5 E).

The source of spending money in early boyhood seemed an important indicator in differentiating I-E in men, as the following results show:

Worked at home for spending money (3 I, 10 E).
Had a regular allowance (5 I, 12 E).
Had an irregular allowance (21 I, 7 E).

(*3*) *Ascendance-submission vs. introversion-extraversion.* It will be recalled that by far the highest *r* in the table of intercorrelations among personality traits is that between ascendance-submission and introversion-extraversion for men, the *r* being −0.51. It will be interesting, therefore, to see how many and what early history variables function in common between I-E and A-S in the direction that supports this correlation. They are as follows:

father's age at *S*'s birth, 36 years or over (14 submissives, 16 introverts, 5 ascendants, 7 extraverts);
favoring languages in high school (9 submissives, 10 introverts, 1 ascendant, 3 extraverts);
participation in games at every opportunity between 7–12 yr. of age (10 submissives, 12 introverts, 18 ascendants, 20 extraverts), between 12–16 (15 ascendants, 17 extraverts, 4 submissives, 6 introverts);
admired by playmates in period from 7–12 yr. of age (18 ascendants, 16 extraverts, 8 submissives, 8 introverts), from 12–16 (24 ascendants, 18 extraverts, 4 submissives, 3 introverts);
only a few intimates of own sex in period from 12–16 yr. of age (10 submissives, 14 introverts, 1 ascendant, 2 extraverts);
no friends of opposite sex when in period from 12–16 yr. of age (16 submissives, 17 introverts, 7 ascendants, 4 extraverts);
finishing high school in less than 3 years (7 ascendants, 5 extraverts, 0 submissives, 1 introvert);
going to parties in period from 12–16 yr. of age (18 ascendants, 15 extraverts, 5 submissives, 6 introverts).

(*4*) *Personal history vs. social intelligence.* In considering the relationship between early personal history and social intelligence, as measured by the Moss test, attention may be called to two points. First, it is questionable whether social intelligence is a personality trait in the same sense that the other four tests imply. Disregarding what an acceptable definition of social intelligence might be, it seems clear, from the content of the Moss test, that what it measures is a certain kind of knowledge or ability rather than a characteristic mode of behavior. Secondly, if the above statement is true, then a certain consistent relationship is to be expected between the test-score and early history, since social intelligence is a product of the social environment and training that constitute a large part of the early personal history. Thus any relation we may find between the history and the test-score would be no discovery, but can be considered merely as a confirmation of the effects of learning or habit formation. It is interesting, nevertheless, to list the early history variables on which those who scored high on Moss's social intelligence test seem to differ significantly from those who scored low.

Only 5 or 6 answer-variables seem to differentiate 'social intelligence' for both sexes in the same direction. It is difficult logically to account for this sex-difference. Furthermore, many of the answer-variables do not show as clean-cut differentiation as those obtained in the case of ascendence-submission or that of introversion-extraversion. The only variables to which especial attention need be directed are the following:

For men:

Fifteen of those having no friends among the opposite sex have a high score and 4 a low score in social intelligence; 11 of those studying art and music have a high score and 3 a low score in social intelligence; 7 of those participating in games at every opportunity tend to have a high score and 18 a low score.

For women:

Fifteen women who skipped 2 grades or more have a high score, 5 a low score; 15 who read omnivorously for pleasure have a high score and 4 a low score;

For both:

Seventeen men and women who never travelled have low scores, 7 high scores; and 17 men and women who played solitary games for recreation have high scores, and 4 have low scores.

(*5*) *Personal history vs. emotionality.* Only one answer-variable, namely "many kinds of religious participation," seems to be significant for emotionality as measured by the Pressey X-O Test. Twenty-four men whose religious atmosphere in childhood was conservative made a high emotionality score, and 12 a low one. Taking men and women together, 13 who "practically never" participated in religious activities during adolescence made high emotionality scores, while 22 made low ones.

(*6*) *Personal history vs. suggestibility.* Of the *S*s, one or both of whose parents were under 25 yr. of age at the time of *S*'s birth, 38 showed a high degree of suggestibility and 19 a low. The direction of this result is reversed for *S*s born of older parents. Of the *S*s who, between the ages of 7–12 yr., participated in games at every opportunity, 12 made high scores in suggestibility and 29 made low. Of those who, during the 7–12 yr. age-period, participated in games only when asked, 9 made high scores and 5 low; and of those who, during the 12–16 yr. age-period, participated only when asked, 17 made high scores and 7 low.

SUMMARY AND CONCLUSIONS

The major results of this study may be summarized as follows:

(1) The answer-variables most significant for ascendance are: reading omnivorously for pleasure, and participating in games at every opportunity. For submission: favoring languages in high school, reading very little for pleasure, and having limited companionship.

(2) Significant for introversion may be mentioned such items as having few playmates as a child, indulging in few social amusements, participating rarely in games at school, and having few or no friends. For extraversion: having many playmates, participating in games at every opportunity, and being admired by associates.

(3) Since the highest intercorrelation between the personality traits was found to be the one between ascendance-submission and introversion-extraversion for men, we noted what answer-variables were significant for differentiating both traits simultaneously. These were: father over 35 at *S*'s birth, *S*'s favoring languages in high school, participation in games at every opportunity, desire for admiration by playmates, lack of intimates of own sex and none of opposite sex during early adolescence, finishing high school in less than 3 years, and attendance at parties during early adolescence.

(4) In social intelligence, where one might expect *a priori* a high degree of relation between the early personal history and the test score, we actually obtained fewer significant answer-variables than in the cases of A-S or I-E.

(5) In studying the relationship between early personal history and emotionality, only one answer-variable applies to both sexes; those who participated in many kinds of religious activity in their adolescence are likely to score high on the Pressey test, and vice versa.

(6) As for suggestibility, especial attention is called to the question on parents' age, the answers to which showed that those *S*'s, both men and women, who were born to parents under 25 years of age had a tendency to be highly suggestible. Also, participation in games at every opportunity and in many kinds of religious activities seems connected with low suggestibility, whereas participating in games only when asked or urged seems to be connected with high suggestibility.

Within the limits of this study and with no claim to completeness or finality, our results seem to justify the following general conclusions:

(1) It appears possible by the method of group differences to discover in a person's early life significant facts in determining some of his later personality traits.

(2) The number and kind of such significant facts and the degree of their significance vary with the traits.

(3) The largest number and the most reliable of the significant facts in early personal history are found for predicting introversion-extraversion, the fewest and least reliable for emotionality.

(4) The results of this study have opened up a large number of specific problems suitable for further research concerning the relationship between certain facts in one's early personal history and certain of his personality traits.

FACTORS INFLUENCING VISUAL IMAGERY FOR LETTER GROUPS

By Henry Bowers, Normal School, Ottawa, Canada

Serious experimental difficulties involved in the study of imagery have hindered the investigation of this mental activity in its relationship with spelling, reading, and arithmetic. It seemed to the writer that a new approach to the study of visual-verbal imagery might be made through evaluation of the relative imaginal content of the experimental material.

Problems

The object of the present study was: (1) to determine if significant differences exist between single lower-case letters[1] in respect of the mean distinctness of their visual images in a group of *S*s; and (2) to determine if significant differences exist between groups of lower-case letters in respect of the mean distinctness of their visual images. If the differences were found we hoped (3) to investigate the relative contributions of the distinctness of the visual images of specified letters or letter-combinations to that of larger letter groups.

Method and Procedure

The 26 letters of the alphabet were typed in lower-case form one underneath another on a strip of paper. Every one of the 183 *S*s, having been given a strip, rated each letter on a 6-step scale. Values 0, 1, 2, 3, 4, 5 were arbitrarily assigned to each step on this scale, The group was divided randomly into two, and the mean estimate for each letter was found for each group. It was immediately obvious that enough reagents had been employed to give stability to the mean estimates of each sub-group. This is indicated mathematically by the Pearson r of 0.99 between the two sets of mean estimates. A similar procedure was followed in the case of lists of 2-letter groups, 3-letter groups, and 4-letter groups. In each of these cases 40 groups of letters were used. The 2-letter, 3-letter, and 4-letter groups were so constructed that it was possible to calculate the r between mean estimates for specified component groups and the whole letter-group; *e.g.* between the first two, or the last three letters of a 4-letter group and the whole group.

It was found (1) that small but very consistent differences exist between single lower-case letters in respect to the mean distinctness of their visual images in a group of *S*s, (2) that substantial correspondence exists between mean estimates by groups of *S*s for the visual imagery of 2-, 3-, or 4-letter combinations, and (3) that the visual imagery of letter groups is not wholly determined by the mean imagery of each constituent letter. It is also influenced by the imagery for subgroups of letters. For example, the imagery of the first two (or the last two) letters in a 3-letter group is more potent in determining the estimate for the whole group than the three letters considered as individuals.

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[1] Twelve-point typewriter type was used.

Table I shows the letter-groups used in the experiment. Following each group is a number which refers to the relative distinctness of the visual images of the group. These numbers, of course, did not appear on the original material. It may be mentioned that except for columns (1) and (2), these numbers are not comparable from column to column, since different groups of students acted as *Ss*. As indicated in the captions of the table, groups of *Ss* numbering from 42 to 183 were used. These *Ss* were Normal School students and form a comparatively homogeneous group.

After a preliminary discussion of visual imagery the following instructions were distributed with lists of numerals.

> Please look closely at each of the following numbers. Then try to see them by means of your *mind's eye*. Some people do this more efficiently when their eyes are closed.
>
> Would you say that—
>
> S. Your mental picture was as clear and distinct as the real numbers?
>
> F Your mental picture was almost as clear and distinct as the original numbers? (Between S and P).
>
> P Your mental picture was fairly clear and distinct as compared with the original numbers? (Between F and N).
>
> N Your mental picture was quite blurred, vague and indistinct as compared with the original numbers? (Between P and H).
>
> H Your mental picture had only the very slightest resemblance to the original numbers? Between N and K).
>
> K You could not form a mental picture of the numbers?
>
> Write the appropriate letter (S, F, P, N, H or K) in capitals at the left of the numbers.
>
> *Caution.* Work slowly, carefully and conscientiously. Do not think that you are making a high score by claiming a very distinct mental picture, or that you are reducing your mark by claiming a vague and blurred one.

RESULTS

The introspections on the numerals were treated as practice and the resulting data discarded. A brief discussion was again held, in which it appeared that negative after-images of the numerals appeared in a number of cases. Advice was given not to fixate the material too long, and to allow the after-images to fade out. The *Ss* were required to form images of the same dimensions as the original figures, and to project them on an imagined white paper at the same distance from the eye as the given strip. An interesting observation was made at this point. One *S* naïvely complained about the numerals "jumping up from the paper" and "going into position." She was found to have a number-form. The letters K, H, N, P, F, and S were assigned arbitrary values, 0, 1, 2, 3, 4, 5, respectively. The mean of all the ratings for each letter or letter-group was calculated. These means are inserted in parentheses after the groups in the above lists. The probable errors of the mean claimed imagery for certain letter-groups (principally those of median imaginal content) were calculated. These data are given in Table II.

A random division of the *Ss* into approximately equal groups was made. Coefficients of correlation between the mean estimates of these halves were computed. They are reported in Table III.

TABLE I

Letter-Groups Used in the Experiment and the Relative Distinctness of Their Visual Images

No. of Subjects

182		183		125		83		92		42	
(1)		(2)		(3)		(4)		(5)		(6)	
a	(2.9)	ab	(2.8)	abr	(2.7)	rab	(3.4)	abrt	(2.5)	trab	(2.7)
b	(3.2)	ck	(2.7)	ckj	(2.5)	jck	(2.7)	ckjm	(2.0)	mjck	(2.2)
c	(3.5)	jq	(2.2)	jqm	(2.1)	mjq	(2.3)	jqmi	(1.8)	imjq	(2.0)
d	(2.9)	rl	(2.7)	rlz	(2.5)	zrl	(2.5)	rlza	(2.1)	azrl	(2.2)
e	(3.2)	bs	(2.7)	bsk	(2.5)	kbs	)2.5)	bsky	(2.0)	ykbs	(2.0)
f	(3.0)	ei	(3.0)	eif	(2.7)	fei	(3.1)	eifh	(2.2)	hfei	(2.2)
g	(2.2)	gt	(2.1)	gtx	(2.1)	xgt	(2.1)	gtxd	(1.6)	dxgt	(2.1)
h	(3.2)	hm	(2.6)	hmg	(2.3)	ghm	(2.3)	hmgn	(1.8)	nghm	(1.9)
i	(3.5)	uy	(2.4)	uys	(2.4)	suy	(3.1)	uysl	(2.0)	lsuy	(2.4)
j	(3.2)	br	(2.6)	brc	(2.7)	cbr	(2.9)	brco	(2.5)	ocbr	(2.7)
k	(2.8)	dw	(2.4)	dwn	(2.5)	ndw	(2.5)	dwnv	(2.0)	vndw	(2.1)
l	(3.7)	fn	(2.7)	fno	(2.8)	ofn	(3.0)	fnoj	(2.1)	jofn	(2.7)
m	(2.8)	kz	(2.3)	kzl	(2.2)	lkz	(2.5)	kzlr	(1.8)	rlkz	(2.0)
n	(3.2)	pj	(2.6)	pje	(2.3)	epj	(2.5)	pjex	(1.9)	xepj	(2.1)
o	(3.9)	lh	(2.9)	lhu	(2.4)	ulh	(2.6)	lhuk	(2.0)	kulh	(2.3)
p	(3.1)	xv	(2.7)	xvp	(2.4)	pxv	(2.6)	xvps	(2.0)	spxv	(2.1)
q	(2.8)	td	(2.7)	tdb	(2.3)	btd	(2.6)	tdbe	(2.0)	ebtd	(2.0)
r	(3.0)	oe	(3.1)	oev	(2.9)	voe	(3.5)	oevz	(2.3)	zvoe	(2.3)
s	(3.1)	mg	(2.2)	mgi	(2.4)	img	(2.8)	mgiu	(1.8)	uimg	(2.0)
t	(3.3)	if	(3.2)	ifd	(2.5)	dif	(3.3)	ifdq	(1.9)	qdif	(2.0)
u	(3.0)	qc	(2.5)	qca	(2.4)	aqc	(2.7)	qcab	(2.2)	baqc	(2.2)
v	(3.4)	nx	(2.5)	nxt	(2.6)	tnx	(2.5)	nxtw	(1.9)	wtnx	(1.9)
w	(2.6)	vo	(3.1)	voq	(2.7)	qvo	(2.7)	voqg	(1.9)	gqvo	(1.8)
x	(3.1)	wp	(2.3)	wpy	(2.0)	ywp	(2.0)	wpyc	(1.7)	cywp	(2.0)
y	(2.8)	zu	(2.4)	zuw	(2.3)	wzu	(2.2)	zuwp	(1.8)	pwzu	(1.7)
z	(3.1)	ya	(2.5)	yaq	(2.3)	qya	(2.2)	yaqf	(1.9)	fqya	(2.0)
		qb	(2.4)	qbh	(2.2)	hqb	(2.2)	qbhx	(1.7)	xhqb	(1.9)
		wf	(2.2)	wfz	(2.2)	zwf	(2.2)	wfzm	(1.7)	mzwf	(1.8)
		sp	(2.8)	spk	(2.7)	ksp	(2.6)	spkt	(2.3)	tksp	(2.2)
		rk	(2.4)	rkg	(2.1)	grk	(2.3)	rkgn	(1.9)	ngrk	(1.8)
		bh	(2.5)	bhe	(2.7)	ebh	(2.8)	bhej	(2.0)	jebh	(2.4)
		eu	(2.9)	eus	(3.1)	seu	(3.2)	eusi	(2.6)	iseu	(2.6)
		mj	(2.5)	mjn	(2.4)	nmj	(2.5)	mjno	(2.2)	onmj	(2.5)
		io	(3.2)	ioc	(3.1)	cio	(3.4)	iocb	(2.5)	bcio	(2.8)
		yd	(2.6)	ydb	(1.9)	byd	(2.9)	ydba	(1.9)	abyd	(2.8)
		xg	(1.9)	xga	(1.9)	axg	(2.3)	xgaz	(1.6)	zaxg	(1.9)
		nf	(2.6)	nft	(2.4)	tnf	(2.5)	nftq	(1.9)	qtnf	(1.8)
		cz	(2.7)	czp	(2.3)	pcz	(2.5)	czpl	(2.0)	lpcz	(2.1)
		qw	(2.1)	qwd	(2.1)	dqw	(2.1)	qwdr	(1.8)	rdqw	(1.9)
		la	(3.2)	lao	(3.0)	ola	(3.5)	laoh	(2.7)	hola	(3.2)

TABLE II

PROBABLE ERRORS OF THE MEAN IMAGERY CLAIMED FOR CERTAIN LETTER-GROUPS

Column in Table I	Letter-Group		No. *Ss*	Mean Imagery Claimed	P.E.$_m$
1	a		182	2.90	.059
1	g	(lowest)	182	2.25	.061
1	s	(median)	182	3.10	.063
1	o	(highest)	182	3.90	.059
2	hm	(median)	183	2.65	.061
2	yd	(median)	183	2.65	.065
3	lhu	(median)	125	2.45	.069
3	nft	(median)	125	2.40	.070
4	qvo	(median)	83	2.75	.086
4	jck	(median)	83	2.75	.091
5	tdbe	(median)	92	2.05	.082
5	bhej	(median)	92	2.05	.078
6	tksp	(median)	42	2.20	.143
6	baqc	(median)	42	2.20	.138

TABLE III

RELIABILITIES OF MEAN ESTIMATES OF IMAGERY

Column Table I	No. Letters in Group	No. Groups in Column	No. *Ss*	*r*
1	1	26	91 and 91	0.99 ± .003
2	2	40	91 and 92	0.93 ± .01
3	3	40	66 and 59	0.86 ± .03
4	3	40	41 and 42	0.92 ± .02
5	4	40	46 and 46	0.85 ± .03
6	4	40	21 and 21	0.77 ± .04

TABLE IV

COEFFICIENTS OF CORRELATION BETWEEN MEAN ESTIMATES

	2-letter groups	3-letter groups		4-letter groups	
Letter	Col. (2) (183 & 183 *Ss*)	Col. (3) (125 & 183 *Ss*)	Col. (4) (83 & 183 *Ss*)	Col. (5) (92 & 183 *Ss*)	Col. (6) (42 & 183 *Ss*)
1st	.73 ± .05	.56 ± .07	.48 ± .08	.44 ± .09	.53 ± .08
2nd	.58 ± .07	.41 ± .09	.55 ± .08	.33 ± .10	.55 ± .08
3rd		.42 ± .09	.22 ± .11	.37 ± .09	.23 ± .10
4th				.50 ± .08	.19 ± .11
1st & 2nd		.77 ± .04		.66 ± .06	
1st & 3rd				.84 ± .03	
3rd & 4th			.72 ± .05		.47 ± .09
2nd, 3rd & 4th					.73 ± .05
Mean of 1st & 2nd	.85 ± .03				
Mean of 1st, 2nd & 3rd		.63 ± .06	.64 ± .06		
Mean of all				.74 ± .05	.67 ± .06

These substantial r indicate that the mean estimates of the whole groups of *Ss* must be very stable. In other words, the employment of still larger numbers would have little effect in disturbing the order of precedence of these letter-groups in respect of imaginal content.

Examination of the six columns of letters will show that data were now at hand for calculation of the following r: (a) between mean estimates for 2-, 3-, or 4-letter groups and those for the individual constituent letters; (b) between mean estimates for 3-letter groups and those for the first two or last two letters; (c) between mean estimates for 4-letter groups and those for the first two, first three, last two or last three letters; (d) between mean estimates for 2-, 3-, or 4-letter groups and the mean of the mean estimates for the constituent letters. These data are reported in Table IV.

One hundred and seventeen *Ss* made estimates on groups of 1, 2, 3, and 4 letters. The mean estimates for a single letter-group were respectively 3.05, 2.5, 2.4, and 2.25.

CONCLUSIONS

(1) Small but very consistent differences exist among single lower-case letters (12-point typewriter type) in respect of the mean distinctness of their visual images in a group of *Ss*. The order of precedence, from those of lowest to those of highest distinctness, is: g, w, (q, y), (k, m), (a, d), (r, f, u), (x, s, z, p), (b, e, h, n, j), t, v, (i, c), l, o.

(2) Groups of 2, 3, and 4 letters may be arranged in series according to the distinctness of their visual images. The following series of 2-letter groups is in order of precedence, the first named being those with least distinct visual images: xg, (qw, gt), (mg, wf, jq), (kz, wp), (rk, uy, qb, zu, dw), (qc, bh, mj, ya, nx), (pj, br, n[illegible], hm, yd), (cz, fn, xv, td, bs, ck, rl), (ab, sp), (eu, lh), ei, (vo, oe), (la, if, io).

(3) The distinctness of the visual images of 2-letter groups is contributed to by the mean distinctness of the two letters, the distinctness of the first, and the distinctness of the second. The order of magnitude of contribution is indicated.

(4) The first and second letters of a 3-letter group appear to be of equal importance in influencing the distinctness of the visual image of the group. There is some evidence that the third letter has rather less weight. The mean of the 3 letters has greater influence than the 3 letters considered singly, but has less weight than the sub-groups, 'first 2 letters,' or 'last 2 letters.' No difference was detected between the contribution of the latter.

(5) The sub-groups 'first 3 letters,' and 'last 3 letters' are of significantly greater importance in determining the distinctness of the visual image of a 4-letter group than the mean distinctness of the 4 letters. In order of importance are 'first 3 letters' (or 'last 3 letters'), mean of the 4 letters, 'first 2 letters' (or 'last 2 letters'), first or second letter, third or fourth letter. The order of the alternatives has not been established.

(6) The feasibility of evaluating verbal material in respect to the distinctness of visual images of it is established, and this is in accordance with conclusions previously arrived at in connection with concrete visual, auditory, and kinaesthetic imagery.[2]

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[2] H. Bowers, Memory and mental imagery: An experimental study, *Brit. J. Psychol.*, 21, 1931, 271-282. See also Constancy of imaginal content, *J. Educ. Psychol.*, 20, 295-298; Studies in visual imagery, this JOURNAL, 43, 1931, 216-229.

THE RELATION OF BRIGHT AND DULL PRESSURE TO AFFECTIVITY

By PAUL THOMAS YOUNG, University of Illinois

In a recent number of this JOURNAL[1] William A. Hunt described an experiment under the above title. Hunt's study rested upon an investigation by J. P. Nafe[2] and a critical experiment by the writer.[3]

Hunt exposed colored papers of various hues and brightnesses through a 5 in. aperture in a cream gray screen. His *Os* were at first instructed as follows:

> I shall present various colored stimuli. You are to attend to the general experience involved in observing them, and to state whether this experience involves a 'bright pressure' or a 'dull pressure.' These pressures are qualitatively similar to those ordinarily elicited by cutaneous stimulation, but are in no way identical with them.
>
> 'Bright pressure' has been described as tactual bright pressure or a touch quality lying between bright pressure and tickle. It has expansive qualities and has been called radial, being more prominent at the edges than in the center. It is bright, buoyant, and has a diffuse bodily feel. The whole experience is diffuse and volumic, and is patterned, with the patterns varying from one experience to another. The following adjectives have been used in describing it: bright, sparkling, brilliant, active, effervescent, like tickle, dancing, shimmering, misty, mild, vaporous, light, airy, fluffy, ethereal, smooth, soft, welling, spreading, like expansiveness of the body.
>
> 'Dull pressure' has been described as dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended. It has qualities of constriction, and has been called radial, but more prominent at the center than at the edges. While it is diffuse and volumic, it is less so than bright pressure and is duller and less bright. It has been called dull, massive, drab, dead, rigid, less lively, somber, inert, stiff, gloomy, more dense, heavy, sinking, leaden, thick, cold, rough, grating, insistent, condensed, like bodily contraction.

This instruction was used to set the *Os* for bright and dull pressure. The next week the same colored papers were presented to the *Os* with a different instruction. Apart from instruction the experimental conditions and procedure were unchanged. The second instruction follows:

> I shall present various colored stimuli, and you are to judge whether they are pleasant, unpleasant, or indifferent.

A week later Hunt repeated the series of pressure judgments and the following week he repeated the series of affective judgments. The total results were treated quantitatively by assigning numerical values; to bright pressure was assigned the value +1, to indifference 0, and to dull pressure −1. Similarly pleasantness was assigned the value +1, indifference 0, and unpleasantness −1. These values were then summed to determine the degree to which a particular color elicited bright pressure, and the degree to which it elicited P.

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*Accepted for publication April 10, 1931.

[1] W. A. Hunt, The relation of bright and dull pressure to affectivity, this JOURNAL, 43, 1931, 87-92.

[2] J. P. Nafe, An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.

[3] P. T. Young, The 'trained' observer in affective psychology, *ibid.*, 38, 1927, 175-185.

The degree to which colors elicited bright pressure was then correlated with their P by the product-moment formula. The coefficients of correlation ranged from 0.73 to 0.99 and their P.E. from 0.003 to 0.08. When corrected for attenuation the coefficients approach +1.00.

Hunt concluded: "These results obviously lend support to Nafe's thesis. They do not, however, prove it."

A group experiment. The following experiment was performed at the University of Illinois with words taken from Hunt's first instruction. It should be noted that Hunt took from Nafe the terms which presumably describe bright and dull pressure; and that Nafe derived these terms directly from the reports of his *Os*. Forty-three descriptive terms were taken from Hunt's instruction, arranged in a chance order and then used as stimulus-words in an affective-association test.[4]

Mimeographed data sheets were distributed to 242 students in the writer's elementary class in psychology. The following instruction was then read slowly:

> I will read some words, one at a time. If a word suggests pleasantness, put down P in the space provided; if it suggests unpleasantness, put down U; if you are not sure, guess.
>
> Do not deliberate, argue or reflect; be passive, matter-of-fact, uncritical. Let the word have its effect and put down the first feeling suggested to you.

Every precaution was taken to gain the coöperation of the group and to maintain conditions satisfactory for a group test.

Fourteen records were incomplete, so the present report is based upon 228 cases, 138 men and 90 women. The *Ss* were sophomores and upper classmen.

From the records the writer tabulated the frequency with which a word suggested P. Table I gives the words arranged in order from most to least P. Since in every case the record was either P or U, the frequency for U is 228 minus the given frequency for P.

At first men and women were tabulated separately,[5] but, since no significant sex-differences were found, the results have been treated collectively. By Spearman's method of rank-differences a positive correlation of 0.96 was found between men and women; $P.E._r$ was 0.008.

Since there were 228 *Ss* half of this number, or 114, will be taken as a convenient indifference point. There are 17 terms (Table I, first column) on the

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[4] The chance order follows: *dull, mild, shimmering, airy, brilliant, dense, lively, smooth, soft, condensed, somber, stiff, like expansiveness of the body, like bodily contraction, sinking, light, rigid, rough, ethereal, grating, inert, misty, like tickle, insistent, welling, active, effervescent, spreading, sparkling, heavy, vaporous, cold, fluffy, massive, dead, drab, gloomy, thick, bright, leaden, dancing, buoyant, diffuse.*

In the above list Hunt's *more dense* was changed to *dense;* and *less lively* to *lively*. The word *effervescent* was inadvertently pronounced *effervescence*. Otherwise the present words and Hunt's are the same.

Incidentally there was mild laughter when the words *stiff* and *dead* were pronounced.

[5] The *Ss* indicated sex on the data sheets but were not asked to give their names.

P side of indifference and 26 terms (Table I, last two columns) on the U side. In other words, the total list is slightly more suggestive of U than of P.

A study of the list in relation to the words in Hunt's instruction shows that *all* P terms (*i.e.* those with a frequency greater than 114) without exception are on the 'bright pressure' list. The U terms (*i.e.* those with a frequency less than 114) contain *all* of Hunt's words descriptive of 'dull pressure' and in addition 6 qualifiers which belong to 'bright pressure.' The 6 U terms which are on Hunt's 'bright pressure' list are all very near to indifference: spreading, welling, diffuse,[6] vaporous, misty, like expansiveness of the body.

TABLE I

Frequency of Association of P with the Stimulus-Words Listed

100% = 228.

lively	224	spreading	109	rough	18
sparkling	224	massive	105	sinking	14
dancing	221	welling	104	grating	13
active	219	diffuse	91	heavy	12
light	219	vaporous	88	stiff	11
smooth	216	misty	86	dense	10
bright	214	condensed	79	leaden	10
bouyant	211	somber	65	drab	8
brilliant	211	like expansiveness of the body	60	gloomy	8
airy	206			dead	7
fluffy	204	like bodily contraction	52	dull	6
mild	199				
soft	188	thick	39		
effervescence	168	rigid	38		
ethereal	165	insistent	33		
shimmering	154	inert	32		
like tickle	130	cold	26		

In general the result is exceedingly clear-cut—more so than the writer had dared to expect. The words *descriptive* of bright pressure suggest P and the words *descriptive* of dull pressure suggest U; some words (near indifference) are relatively nondescript.

Immediately following the 43 stimulus-words on our list another instruction was read to the group:

> I will read a list of terms. Do not write anything until I have read the entire list. Then, if the total list suggests P, write P; if it suggests U, write U.

Two lists were read: first the word-list descriptive of 'bright pressure,' and then the list descriptive of 'dull pressure.' Both lists were taken from Hunt's instruction and read without change. The 'bright pressure' list began: "bright, sparkling, brilliant, active . . . ." The 'dull pressure' list began: "dull, massive, drab, dead . . . ."

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[6] Bright pressure is said to be more diffuse than dull pressure, but the word *diffuse* may apply to either. Strictly speaking the word is not on Hunt's list. See the instruction.

The result was decisive and scarcely needs any comment:

	Men	Women	Total
P suggested by 'bright pressure' list	131	90	221
U " " "	1	2	3
P suggested by 'dull pressure' list	1	2	3
U " " "	131	90	221

There were 3 persons who for some reason or other gave just the reverse of what we had expected.

It is fair to conclude that the terms alleged to be descriptive of 'bright pressure' are highly suggestive of P and those descriptive of 'dull pressure' are suggestive of U.

Interpretation. There are many words in the English language which suggest P and U feelings; in addition to the ones already considered are[7] *enjoyable, pretty, nice, sweet, rich, vibrant, corking, cheerful, happy,* etc.; and *horrid, terrible, draggy, sad, nauseating, uncomfortable,* etc. An affective-association test is a simple means of determining the degree to which a given word suggests P or U. In this connection the writer is reminded of experiments by Titchener, Hayes, and Foster and Roese[8] upon the Wundtian theory of feeling. In their work, curves of preference gained by the method of paired comparisons as well as introspective reports indicated that certain Wundtian affections are related to P and U. 'Calm' and 'relaxation' are apt to be P and 'strain' and 'depression' are apt to be U.[9]

It seems to the writer that many of the words used by Hunt do not *describe* a pressure-like experience. What, for example, is a 'gloomy, drab, dead' pressure? And what a 'brilliant, active, dancing' pressure? Opinions may differ as to how far these words *describe* pressure; but there is no doubt that the 'bright pressure' words *suggest* P and the 'dull pressure' words *suggest* U.

The present result is not surprising when one recalls, (1) that the 'bright pressure' words were used originally in an attempt to characterize P experiences and the 'dull pressure' words were used to characterize U; and (2) that Hunt's *Os* were limited by instruction to two categories of report, namely, pressure (bright or dull) and affection (P or U).

That suggestion is involved is indicated by one of Hunt's control experiments. Three "wholly naïve observers" made a single series of pressure judgments and after a month a single series of affective judgments. Correlations between the two series were 0.82, 0.92, and 0.88, with P.E. respectively of 0.05, 0.02, and 0.03. Obviously in this case prolonged psychological education and training are out of the question. Previously the writer attempted to confirm Nafe's result by giving three *Os* ample practice under conditions which precluded suggestion; but he failed to duplicate Nafe's results.[10] In Hunt's control experiment the effectiveness of verbal suggestion in the absence of practice is demonstrated. In determining a result suggestion, therefore, is more effective than practice (repetition of observation).

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[7] Some of these examples are taken from O. D. Anderson, An experimental study of observational attitudes, this JOURNAL, 42, 1930, 354.

[8] For references, see W. S. Foster and K. Roese, The tridimensional theory of feeling from the standpoint of typical experiences, this JOURNAL, 27, 1916, 157.

[9] Cf. the writer's results, Pleasantness and unpleasantness in relation to organic response, *ibid.*, 32, 1921, 40.

[10] *Op. cit.*, footnote 3.

Can the pressure-affection correlation be demonstrated by methods which rigidly exclude suggestion?[11]

Conclusion. Hunt's high correlations between pressure and affection probably depend upon verbal suggestions given to the *Os* in an instruction. Bright pressure was described in terms highly suggestive of P and dull pressure in terms highly suggestive of U. His experiment shows how easily verbal suggestion can determine a result. Again we are in doubt whether Nafe's thesis can be demonstrated without suggesting it in the instructions or otherwise.

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[11] It would be unfortunate if the present discussion obscured genuine problems. Nafe and others have concluded that P and U are directly observable experiences similar to pressure; that the difference between P and U pressures may best be described by the words *bright* and *dull;* and that there is some evidence that bright pressure is localized in the region of the chest and dull pressure in the region of the abdomen. These statements need to be tested by a method in which the instructions do not suggest the result.

INTENSITY OF STIMULATION AND THE AMPLITUDE AND LATENCY OF THE LID-REFLEX

By Helen Peak, Yale University

This study reports briefly three preliminary generalizations regarding the effect of the intensity of stimulation on the reflex response of the lid to a sudden noise: (1) the amplitude of the lid-reflex tends to increase in direct proportion to the logarithm of the stimulus intensity; (2) the increment of increase of response with increased intensity of stimulation is less under a state of relaxation than under a relatively tense state of the reactor; (3) reflex latency shows a slight decrease with increasing intensity of stimulation. When comparing responses of the same amplitude, however, this difference in time disappeared in all but one *S*.

These facts are incidental to an experiment reported elsewhere,[1] where the description of the apparatus and experimental procedure may be found. To summarize the experiment briefly, we note that the lid-reflex, in response to four intensities of auditory stimulation, was elicited on the background of psychophysiological sets evoked by three types of instruction. In the first set *S* was instructed to relax, in the second, to wink voluntarily just as quickly as possible, and in the third, to press a key voluntarily with the finger and just as quickly as possible. Five *S*s were given about 900 stimulations each. The lid-response was recorded photographically. The stimulating apparatus was calibrated so that the relative intensities could be identified and measured. The relative values derived from this calibration are given in Table I alongside letters which designate the different intensities (A is the lowest intensity). These values are purely relative and cannot be interpreted in absolute terms. They refer to the squares of readings on a photographic film which are direct functions of the displacement of the sounding board. Since the frequency values of these displacements are not known, the absolute intensities cannot be derived. The intensities were arranged in the experimental series so that each occurred once in every four trials of a practice series of twenty-four trials. No intensity succeeded itself.

TABLE I

AVERAGE AMPLITUDES FOR ALL SUBJECTS BY SETS AND INTENSITY

Intensity	Relaxation (mm.)	Lid-voluntary (mm.)	Finger-voluntary (mm.)
(A) 22.09	8.9	11.4	11.2
(B) 44	13.2	17.3	15.9
(C) 102.25	14.4	19.7	19.5
(D) 2218.41	17.2	23.8	23.0

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*Accepted for publication April 1, 1931.

[1] E. Peak, Modification of the lid-reflex by voluntarily induced sets, *Psychol. Monog.*, 42, 1931, (no. 188), 68.

Curves of the form $y = k \log_{10} x + c$ have been fitted to the empirical points of Table I by the method of least squares.[1]

Amplitude. Fig. 1 shows the relationship between the intensity of stimulation and the amplitude of reflex response. In the inset the same data for amplitude are plotted against the logarithm of the stimulus intensity. The actual equations from which the curves were plotted are: (1) relaxation, y (amplitude) $= 3.8907 \log x + 4.035$; (2) lid-voluntary, $y = 5.902 \log x + 3.762$; (3) finger-voluntary, $y = 5.832 \log x + 5.23$.

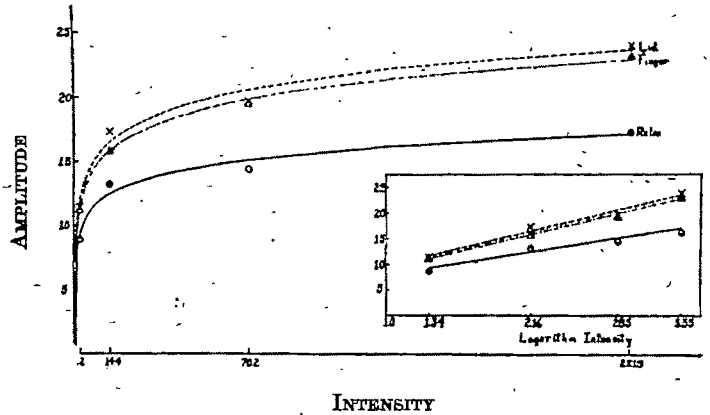

FIG. 1. THE RELATION OF AMPLITUDE OF RESPONSE TO INTENSITY OF STIMULATION

Amplitude values are represented on the y-axis, intensities on the x-axis. In the graph on the left-hand side of the figure, y-values are plotted against x-values; in the other graph, y-values are plotted against log x-values. For both graphs the empirical points are indicated by circles for the relaxation data, crosses for the lid-voluntary, and triangles for the finger-voluntary. The curves have been fitted to the data by the method of least squares.

In each set the empirical points approach close to the lines of best fit through these points so that the lines may be taken to represent the general tendency of the data. It is indicated, therefore, that the reflex response increases in direct proportion to the logarithm of the stimulus intensity, which is the relationship expressed in the Weber-Fechner law. These facts are probably observed best from the semi-log plot in the inset of Fig. 1, particularly the relation of the curves to the empirical points. An increase in amplitude of the reflex with increase in intensity of stimulation has been noted by numerous investigators, though the nature of the relationship has been variously stated and certain limiting conditions, under which the increase does not occur, have been reported.

The slope of the lines is different for the three sets; the greatest difference appears between the voluntary sets and relaxation. The values for the two

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[1] The following formulae were used:

$$k = [(n\Sigma y)(\log x) - (\Sigma y)(\Sigma \log x)]/[(n\Sigma \log^2 x) - (\Sigma \log x)^2]$$
$$c = [(\Sigma y) - (k)(\Sigma \log x)]/n.$$

I am indebted to Dr. E. R. Hilgard for the derivation of these formulae.

voluntary sets differ only slightly. Thus the same added unit of physical intensity produces a greater increase in response under voluntary sets than under relaxation.

The problem of the relation of these lines at the threshold of response (when y = 0) suggests itself. When continued to the x-axis, the relaxation curve meets it at 0.095; the lid-voluntary, at 0.23; the finger-voluntary, at 0.28. These values are very close together, though they suggest a slightly higher threshold for the reflex under voluntary sets than under relaxation. Whether or not this is true depends, of course, on the constancy of the relationship found here between amplitude of response and intensity of stimulation. The evidence from other types of behavior indicates that such a relationship is likely to change near the threshold. A study of reflex thresholds under different sets, as well as the shape of the curves as the threshold is approached, is indicated as an important experimental problem.

TABLE II

AVERAGE LATENT TIMES IN σ FOR EVERY INTENSITY

	Intensity			
Subject	(A)	(B)	(C)	(D)
1	48.3	46.4	46.2	46.0
2	40.4	38.8	38.4	37.8
3	40.3	38.3	38.1	36.6
4	40.5	38.8	39.4	38.1
5	38.2	36.3	37.7	37.4

Time. Numerous references to the relation between intensity of stimulation and the time of reflex response are to be found in the literature. Disagreement obtains, however, even among the studies which have employed more or less similar techniques. We have no conclusive data on the problem but wish to discuss briefly the error in determining latency which arises from the difficulty of reading time on responses of low amplitude.

We found, when examining the assembled time values of the lid-reflex for all *S*s and sets, that there was a slight decrease in latency of responses to greater intensities of stimulation. This was not consistently true from intensity to intensity, but in a comparison of the average time of response to the lowest intensity with the average time to the highest intensity, the highest intensity group showed the shortest time in 93% of the comparisons. Table II contains the composite average times of all sets for each *S* for each intensity of stimulation. Though the differences are small here, their direction is clear and responses to the low intensity A all show longer latencies than those to any of the higher intensities.

In order to determine whether this small difference was affected by the fact that the large intensities produced consistently larger amplitudes of response, which might have been read erroneously at shorter times, we decided to compare the time of response to the different intensities in reactions of the same amplitude, thus making the amplitude factor constant. Reactions were placed in groups according to their amplitudes (group intervals of 3 mm.) and the average latency value for each group was determined. The averages of responses in the same amplitude group were then compared and it was found that the latency of response to the smallest intensity was longer than that to the largest only 47.5% of the time instead of 93% of the time, as it was before amplitude was 'partialled out.'

TABLE III

AVERAGE LATENCY IN σ FOR EVERY INTENSITY
(Amplitude constant)

	Intensity			
Subject	(A)	(B)	(C)	(D)
1	46.4	46.2	47.5	48.0
2	38.0	37.8	37.9	39.8
3	40.5	38.1	38.7	36.2
4	38.4	38.1	38.7	38.8
5	37.2	37.2	40.5	38.2

Table III gives data comparable to those of Table II. Here we have the composite means derived by averaging the means of each amplitude group. In each of these averages the amplitude factor has the same weight. It is clear from Table III that, with amplitude held constant, time differences disappear for all except *S* 4, who retains about the same relative values for different intensities. The factor of amplitude is thus shown to be related to latency. That the error of reading small amplitude responses may be responsible for this fact seems likely, but it should be remembered that a real relationship may exist between large amplitude and short latency which is not due to such a technical error. Whatever the cause of the correlation may prove to be, the empirical fact is established for the present data, and it should be considered in all cases where amplitude changes concomitantly with latency.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF NORTHWESTERN UNIVERSITY*

I. Muscular Tension and Irritability

By G. L. Freeman and E. Taylor Katzoff

The relationship between emotional behavior and muscular tension has been mentioned in several papers of recent date. Two investigators, Johnson[1] and Duffy,[2] have produced evidence favoring the view that individual differences in emotionality are reflected by corresponding changes in the tension of skeletal muscle. Using the pressure exerted upon hand dynamometers as an indicator of tension, Duffy obtained records from nursery children during discrimination and tapping tests. The children were then rated by teachers and physicians for excitability (emotional stability) and school adjustment. Individual tension and excitability scores gave a coefficient of correlation of 0.52, P.E. 0.13. Tension and ratings for school adjustment gave a correlation of 0.58. While it is obvious that changes in muscular tension occur in connection with practically every mental state, the correlation of tension-increments during work with ratings for temperamental differences deserves critical study.

The purpose of the present report is to extend and amplify Duffy's observations.[3] Our specific problem was to record the changes in tension of adult *Ss* during experimentally produced irritations and to relate these changes with some objective measure of their emotional irritability in connection with situations of everyday life. Emotional irritability is here defined as the tendency to be annoyed, disturbed, or stirred-up by external occurrences. The general advantage of test over rating procedures led us to accept the Cason Common Annoyance Test as an indicator of what we have called emotional irritability in everyday situations.[4]

Experimental Procedure

The *Ss* were 50 college students ranging in age from 18 to 24 years. Each served for one session only. *S* was brought into a partially darkened experimental room and seated before a screen. In order to keep the purpose of the experiment from being too obvious, the following misleading information was given.

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*Accepted for publication June 15, 1932.

[1] Buford Johnson, Changes in muscular tension in coördinated hand movements, *J. Exper. Psychol.*, 11, 1928, 334.

[2] Elisabeth Duffy, Tensions and emotional factors in reaction, *Genet. Psychol. Monog.*, 7, 1930, 65; The measurement of muscular tension as a technique for the study of emotional tendencies, this Journal, 44, 1932, 146-162.

[3] Our thanks are due to Miss Dorothy Black, Miss Eleanor Agnes, Miss Marjory Koch and Mr. Emanual Neilson for valuable assistance in the conduct of this experiment.

[4] H. Cason, An annoyance test and some research problems, *J. Abnor. & Soc. Psychol.*, 25, 1930, 224-236.

The purpose of this experiment is to measure changes in perspiration during reading. Seat yourself comfortably with your head against the rest. Take this bulb and hold it firmly in your hand; on no account are you to drop or release it. When the reading light is turned on, you will find a series of humorous articles which you are to read. Begin reading aloud when I say go and continue reading until you come to the end of the selection.

The bulb was connected with a sensitive tambour, writing on smoked paper. Since the air column was placed under pressure by closing the hand over the bulb, both the strengthening or weakening of the grip could be recorded.

S was allowed about 5 min. in which to become accustomed to the experimental situation. A record of the pressure upon the bulb was taken during this entire period, in order to establish a baseline from which subsequent changes could be measured. Six selections were read during the experimental period, and a single irritating stimulus was introduced with each passage, after *S* had become engrossed in the reading. The following stimuli were used in random order: (1) flickering of the reading light; (2) quick removal of support at back of *S*'s head; (3) falling of a large brass weight near *S*'s left thigh;[5] (4) snapping of a large mousetrap; (5) strong upward current of air, driven by electric fan near feet of *S*; (6) persistent filing and scratching on a screen.

Many of the *S*s reported that these stimuli produced irritations with a high degree of emotional content. They were somewhat at a loss to know just why they had been introduced; but the only definite cue to the purpose of the experiment was supplied *after* the session, when *S* was asked to rate himself on the Annoyance Test.

Auxiliary records of quadriceps tonus. Because pressure upon a bulb is a very crude indicator of tension and because it may involve voluntary increments in tension as well as involuntary changes,[6] auxiliary records of quadriceps tonus were obtained for 10 of the *S*s. Tonus was photographically recorded by the method of tendon deformation, as previously described.[7] These records showed a tendency to correlate with the changes in pressure on the bulb.

Scoring of the kymograph records. Changes in the pressure upon the bulb during and following the introduction of 'irritating' stimuli were reckoned in mm. of height from an arbitrary baseline, established at the midpoint of a 5-min. preëxperimental tension curve. Three measurements were taken of the response to each of the six stimuli; these were, (1) maximal height, or depression, in the curve, (2) height 5 sec. before maximal, (3) height 5 sec. after maximal. These three measures were then averaged and the six averages combined to give the composite tension score for each subject.

RESULTS

The results of this experiment may be presented briefly in the correlation found to exist between individual tension scores and subjective ratings for annoyance and irritability. They indicate that the person whose tension increases during irritating experimental situations will also rate such items as the following as being extremely annoying.[8]

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[5] This ball was arranged so that it would fall from a shelf outside *S*'s range of direct vision. An invisible wire prevented the ball from descending beyond a certain height.

[6] We are making the assumption that the tensions dealt with in this experiment were largely of an involuntary nature.

[7] G. L. Freeman, The measurement of tonus by deformation of the tendon, this JOURNAL, 43, 1930, 581-592.

To hear a person talking in a shrill voice.
To see a person picking his nose.
To see public love-making.
To hear water dripping from a faucet.
To have to kiss a relation I do not want to.
To see a person at table spitting out food.
To have a person look over my shoulder and read the book I am reading.

Because we were mainly interested in the *position* of individual tension and annoyance scores, independent of the *size* of the score, the coefficient of correlation was obtained by the rank-difference method. It was found that $\rho = +0.49$ (P.E.$_r$ = 0.07). This compares favorably with the figures reported by Duffy and indicates the tendency for tension to be as positively correlated with irritability in adults as it is in children. The fact that the coefficient is not any larger does not seem to us to depend upon the reliability of our sample as upon the methods employed to test for irritability and tension. From the standpoint of the distribution of scores, the measures of irritability are about as those of tension. The average annoyance score for our group is 18.5, m.v. 3.3. The average tension score is 12.7 mm., m.v. 5.7.

The validity of the tension scores is dependent upon two factors. For one thing, any pressure system is at best a very crude indicator of tension; in the 10 cases, where the more delicate technique of tendon deformation was used concomitantly, the coefficient of correlation between the two indicators was only 0.22.[9] In the second place, individual tension scores would have to be obtained upon a number of different days and under a variety of conditions before one could deal with the problem of individual differences very thoroughly. Duffy's work has a certain advantage over ours in this respect. She used 18 *Ss* in the same situation for several sessions. This arrangement did not seem feasible in our experiment. The advantages of our study lie in the fact that we used a greater number of cases (50), and sought to arouse definite emotional reactions, whereas the occurrence of such reactions in Duffy's experiment was only incidental.

DISCUSSION

Our results are further evidence that the differences in tension between individuals is not a fortuitous matter, but a highly significant one. *Ss* who are tense and highly excitable in experimental situations, also tend to rate themselves high with regard to common annoyances. Annoyance may depend upon a number of things. Among those most commonly listed we find such expressions as 'excitability,' 'emotional maturity,' 'instability,' 'poor adjustment,' and 'irritability.' We have chosen to use the last term to describe the subjective factor underlying most annoyances. In general, it is the tendency to be easily aroused and stirred-up, to respond in a somewhat irrational manner and to react emotionally as well as perceptually to external occurrences.

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[8] To those who are unfamiliar with the Annoyance Test, we might add that *S* can give one of four ratings to each statement (3) extremely annoying, (2) moderately annoying, (1) slightly annoying, (0) not annoying; x indicates that *S* is unfamiliar with situation.

[9] This would be due partially to the differential relaxation between various members of the body. We recall that Duffy (*op. cit.*, 157) found a low correlation between the tension of both hands.

Why should one who is tense become more easily irritated than a person who is habitually more relaxed? The question is not an easy one to answer. We have very little in the way of valid objective differentia between emotions and other mental states. The most pertinent fact which we can bring to bear upon the problem is that the organism is *more highly aroused* in the so-called emotional state. Emotional arousal is measured by some indicator of organic activity. Muscular tension is one of these. The previous question might now be phrased somewhat differently: How can the degree of excitation already present in the organism determine how it reacts to a given external situation? If one accepts the principle that the impulses coming into the central nervous system from the proprioceptors act to lower thresholds of nervous irritability to all stimuli, we have the beginnings of an explanation.[10] If some individuals show themselves to be more easily annoyed and irritated than others, that tendency may be due, in part at least, to a neuro-muscular mechanism which is habitually geared to high pitch. Clinicians and others interested in the diagnosis of emotional tendencies do well to include some measure of involuntary muscular tension in their round of tests.

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[10] We do not propose to discuss this matter in detail here. Cf. G. L. Freeman, Mental activity and the muscular processes, *Psychol. Rev.*, 38, 1931, 428-449, for a suggestive account.

RELATION OF SPEED OF REACTION TO INTELLIGENCE

By L. F. Beck, Brown University

Speed of reaction has frequently been regarded as a contributing factor in intelligence. Its significance is not, however, clearly understood; and the results of experiments made upon this topic have been ambiguous. McFarland, for example, in a summary of the literature, noted the contradictory nature of the results, but concluded nevertheless that there was "a vital relationship between rate and mental ability."[1] More recently Beck reviewed the field, and he came to the conclusion that speed was only of minor, if of any, importance in intelligence.[2]

The object of the present study is, therefore, to obtain further data upon the relation of speed and intelligence by determining specifically the correlation between speed in (1) simple reaction, (2) serial reaction, (3) reading, and the scores in an intelligence test.

Method and Procedure

Simple reactions. A Dunlap chronoscope was employed for measuring the simple reaction-times.[3] Every *S* was instructed to press a telegraph key with his right hand when he observed any movement of the pointer on the dial of the chronoscope. A preparatory stimulus, "ready," was given from 1½–3 sec. prior to the engaging of the recording mechanism of the chronoscope.

Serial reactions. For the measurement of serial reaction, the Seashore serial discrimeter was used.[4] This apparatus calls for the successive activation of the four fingers of the right hand in response to four numbers which are presented visually in a random succession.

Reading test. The reading test was constructed from magazine articles. In order that the *S*s could easily comprehend the material and still not be able to skim and to slight the reading, articles were selected whose content was comparatively simple but whose construction was quite difficult. The time permitted the *S*s for reading was 2 min. At the end of that interval the *S*s were given 24 short questions, every one capable of being answered in a single word or short phrase, the results of which were used as a measure of their rate of comprehensive reading.

Procedure. The testing program was divided into four series as follows: (a) 5 serial reactions followed by 25 simple reactions; (b) 5 serial reactions, 25 simple reactions, half of the reading test, and half of the comprehensive questions; (c) the same as (b); and (d) the same as (a). Thus in all 20 serial reactions, 100 simple reactions, and 2 scores of silent reading were obtained for every *S*.

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*Accepted for publication November 7, 1931. From the Psychological Laboratory of the University of Oregon.

[1] R. A. McFarland, The rôle of speed in mental ability, *Psychol. Bull.*, 25, 1928, 610.

[2] L. F. Beck, The rôle of speed in intelligence, *ibid.*, (forthcoming).

[3] K. Dunlap, The Johns Hopkins chronoscope, *J. Exper. Psychol.*, 2, 1917, 249-252.

[4] R. H. Seashore, Techniques for measuring serial action, *ibid.*, 11, 1928, 45-55.

Thirty *Ss*, 15 men and 15 women, all university students, served in this study. Percentile scores on the Thorndike College Entrance Examination were obtained for every *S* from the University files.

RESULTS

The means and standard deviations of the various tests for the 30 *Ss* are as follows:

	Mean	S. D.
Serial reaction	209.7	25.30
Simple reaction	198.0	20.70
Reading rate with comprehension	217.9	70.42
Thorndike percentile scores	74.3	12.14

The score for serial reaction is the number of individual discriminative finger movements made in a 2 min. period. Simple reaction is expressed in thousandths of seconds, while reading rate is given in words read per min.

According to Wood,[5] Thorndike scores have a reliability of $r = 0.85$. The reliability coefficients of the serial reaction and simple reaction measures are $r_{1I} = 0.96$ and $r_{2II} = 0.93$ respectively.[6] The reliability of the reading test scores is related in part to the number of words deducted from the gross score of each observer for an incorrect answer to a comprehension question. The following table presents this observation.

Penalty	
0 words per question, r_{3III}	$=0.88$
5 " " " "	$=0.90$
10 " " " "	$=0.94$
20 " " " "	$=0.95$
30 " " " "	$=0.94$
40 " " " "	$=0.83$

It appears from the above array that some *Ss* attempted to increase their speed of reading beyond their comprehension rate. This resulted in errors on the comprehension questions as well as unstable measures of reading rate. By the use of weights on the comprehension questions, the relative extent of this instability is revealed. For the purpose of correlation with other measures, the reading scores with a reliability of $r_{3III} = 0.95$ are used.

Correlations among the four measures are as follows:

Serial reaction and simple reaction	$r_{12} = -0.09$
Serial reaction and reading rate	$r_{13} = 0.23$
Serial reaction and Thorndike scores	$r_{14} = 0.25$
Simple reaction and reading rate	$r_{23} = 0.13$
Simple reaction and Thorndike scores	$r_{24} = 0.02$
Reading rate and Thorndike scores	$r_{34} = 0.32$

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[5] B. D. Wood, *Measurements in Higher Education*, 1925, 45.

[6] Each reliability coefficient was obtained by correlating the first half of the particular test with the second half. The coefficient then was corrected for double length of the test by the Brown-Spearman prophecy formula:

$$r_{II} = 2r_{\frac{1}{2}\frac{1}{2}}/(1 + r_{\frac{1}{2}\frac{1}{2}}).$$

First and second order partial correlations closely parallel the zero order coefficients; hence, there are no borrowed factors in the coefficients given above. Neither are there interference factors which tend to obscure relationships greater than those obtained.

By the use of the following formula:[7]

$$\sigma^2\underline{\Delta}/\sigma^2\Delta \infty \omega = (1/r_{1I} + 1/r_{2II} - 2)/(1/r_{1I} + 1/r_{2II} - 2r \infty \omega),$$

a quotient is procured, which, when translated from a table, gives the percentage of measurable differences in test scores which exists in excess of chance differences. By the application of this technique to the above inter-correlations, the following measurable differences readily are determined.

Serial reaction and simple reaction	88.8%
Serial reaction and reading rate	86.9%
Serial reaction and Thorndike scores	75.0%
Simple reaction and reading rate	85.1%
Simple reaction and Thorndike scores	78.2%
Reading rate and Thorndike scores	72.0%

The high percentage of measurable differences between serial and simple reaction in adults indicates that the speed of reaction of a muscle group in the forearm, wrist, and fingers, which is set for a specific response, does not determine the speed of reaction of a series of similar muscle groups which respond successively with no opportunity for a preparatory set.

Further, the measurable differences among simple and serial reactions and reading rate are approximately the same as the differences between the two motor tests themselves. In addition, it should be observed that the correlation between serial reaction and reading rate is higher than that between simple reaction and reading rate; yet, there is a higher percentage of measurable differences in the former. This fact prohibits direct interpretation from correlation coefficients alone.

Finally, the percentage of measurable differences between the Thorndike test and the other three tests is somewhat lower because the reliability of the Thorndike is 0.85. Nevertheless, the percentage of differences suggests that speed in simple reaction, serial reaction, and reading plays a minor rôle in the determination of intelligence as it is measured by the Thorndike Examination.

CONCLUSIONS

(1) In adults, there is little relationship among measures of speed of simple reaction serial reaction, and reading rate. The percentages of measurable differences in excess of chance differences are 88.8%, 86.9%, and 85.1%.

(2) Simple reaction, serial reaction, and reading rate are not the principal determinants of proficiency on the Thorndike Entrance Examination. The percentages of measurable differences are 78.2%, 75%, and 72%, respectively.

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[7] T. L. Kelley, Professor Kelley's reply, *J. Educ. Psychol.*, 16, 1925, 52-55.

A COMPARISON OF METHODS IN DETERMINING THE AFFECTIVE VALUE OF WORDS

By Richard Ledgerwood, University of Maine

The object of the present study was to investigate the application of two of the traditional psychophysical methods, the method of paired comparisons and the order-of-merit method, to the aesthetic judgment of verbal material.

Procedure. It was desired to obtain, for use as stimulus-material, plausible combinations of letters which should be free of denotative meaning. The artificial word-formations which figure in great numbers as the names of characters and places in the novels of James Branch Cabell supplied this requirement. A list of the proper nouns occurring in one of these novels, *The Silver Stallion*, was compiled. This list was first subjected to certain processes of elimination. Formations bearing a discernible likeness to known words were excluded from consideration, as were also compound names and formations possessing diacritical marks. From the remainder, 20 combinations of letters were selected with an eye to variety of constituent letters and diversity in length. The selection was made in such a manner that groups judged by simple inspection of the compiler to be extremely pleasing in character, mediocre, and distinctly ugly contributed about equal numbers.

Arranged in alphabetical order, the words chosen were:

Apsarasas	Fomor	Miramon	Storisende
Breidablik	Glavrab	Ninzian	Thossakan
Coth	Holmendis	Ogde	Vanadis
Donander	Kerin	Porutsa	Yaotl
Ettarre	Ladinas	Ranec	Zipacna

Each of these 20 words was paired twice with each of the other words, positions being reversed the second time so that every word occurred in combination with every other word once on the right and once on the left. The 380 pairs so obtained were arranged in chance order and typewritten on 16 slips of paper measuring 11 by 4¼ in. Twenty-five pairs appeared on each of the first 15 slips and the remaining five pairs on the last.

The 7 members of a class in experimental psychology, comprising 4 men and 3 women, acted as judges. They first made the judgments by paired comparison, underlining the word of each pair that they preferred to the other. When they had completed this task, they were given a simple list of the 20 words and asked to arrange them in order of choice according to the order-of-merit method.

Results. The sum of the frequency of choices on the method of paired comparisons was found for each word. The sum of the ranks for each word on the order-of-merit method was also determined. The order of size of these figures

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is shown in Table I. The order of the composite judgments as determined by the method of paired comparisons is shown in the first column; and the order of the composite judgments as determined by the order-of-merit method is shown in the second column.

TABLE I

ORDER OF COMPOSITE JUDGMENTS

Words	Methods		Words	Methods		Words	Methods	
	P.C.	O.M.		P.C.	O.M.		P.C.	O.M.
Vanadis	1	1	Storisende	8	8	Coth	14	17
Donander	2	2	Thossakan	9	10	Ranec	15	11
Miramon	3	3	Breidablik	10	12	Glavrab	16	16
Ninzian	4	6	Apsarasas	11	14	Zipacna	17	15
Ettarre	5	4	Kerin	12	9	Porutsa	18	18
Holmendis	6	5	Fomor	13	13	Yaotl	19	20
Ladinas	7	7				Ogde	20	19

The results of the two methods concur in showing that *Vanadis*, *Donander*, *Miramon*, *Ninzian*, and *Ettarre* were the 5 names found most pleasing by the judges and *Glavrab*, *Zipacna*, *Porutsa*, *Yaotl*, and *Ogde* the 5 names found least pleasing.

The correspondence of the results yielded by the two methods, measured in terms of the rank-difference coefficient of correlation, was ascertained to be + 0.96. This figure agrees very well with Barrett's finding of an average correlation of 0.987 for three non-aesthetic materials—weights, handwriting, and beliefs.[1]

The amount of agreement among the judges was determined by computing the rank-difference correlation of each *S*'s judgments with each other *S*'s judgments, treating the results of the two methods separately. This procedure gave 21 coefficients to be averaged for the method of paired comparisons and a like number to be averaged for the order-of-merit method. The average agreement between judges was + 0.48 on the method of paired comparisons and + 0.44 on the order-of-merit method. The average agreement between judges on the particular material used when the results of both methods are combined was + 0.46.

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[1] Mabel Barrett, A comparison of the order of merit method and the method of paired comparisons, *Psychol. Rev.*, 21, 1914, 278-294.

APPARATUS

AN ACCURATE DIFFERENTIAL COLOR MIXER

By Forrest L. Dimmick, Hobart College

The differential color mixer, on which the proportions of two or more color disks can be changed while the machine is in operation has long been a favorite device for color demonstrations. There are several forms of the instrument on the market which carry from 2–6 independently variable disks. In the main, however, the shifting mechanisms introduce considerable errors so that only the approximate amounts of exposure of the color disks at any moment can be read from the scale. A machine of this type, which we have built in the Hobart Laboratory, was designed with precision in view. Only two disks are carried by this rotator. Each is fastened to the end of one or the other of two hollow steel shafts about 12 in. in length which fit one inside the other. The outer shaft is mounted at the ends in ball bearings and is driven by a belt from a 1/6 h.p. motor. Two parallel slots, 1/8 in. wide, exactly 9 in. long and directly opposite each other, run through the middle length of the larger shaft. In the smaller shaft is a similar pair of slots cut in a spiral that makes exactly one revolution in 9 in. A 1/8 in. hardened steel pin passes through the four slots and is carried in the end of a third steel tube that runs inside the smaller shaft. As this third tube is pushed in or pulled out, thus changing the longitudinal position of the steel pin, the relative positions of the two shafts and their corresponding disks are changed. The steel pin drives all three shafts together. The rear end of the tube that carries the pin is held in a thrust bearing. A 1/2 in. steel rod, threaded 18 turns to the inch, passes through a nut in the thrust bearing mounting and into the pin tube. The other end is held against end motion in a bearing in the rear support and is furnished with a crank.

We followed Zimmerman's method of attaching the color disks to the shafts. On the front end of the larger shaft is mounted an aluminum plate 14 cm. in diam. and 1/2 in. thick. Near the periphery is a clamp 3 cm. long for attaching a color disk. The center of the plate is recessed to a depth of 9/32 in. for a diameter of 1/4 in. thick which is attached to the end of the smaller shaft. The smaller plate is fitted with a clamp like that on the larger and an arbor similar to the ordinary one used for holding color disks, save that it does not screw down tightly upon the color disks but holds their centers in place, at the same time permitting them to move freely. The advantages of this instrument lie in the double slot and in the dimensions of the screw. The close fitting pin acting in two places reduces lost motion between the two shafts to a practical zero. Any setting can be duplicated with an error of less than 0.5°. The ratio of the screw (20) to the length of the slots (9 in.) gives 180 turns to a complete 360° shift of the disks or one turn to every 2°. Since the handle of the screw can easily be set by quarter turns, steps of 0.5° can be made. Adjustments of this size have been made throughout the entire range and have been found to be as accurate as could be measured with a protractor. The necessity for making 180 turns of the screw to go from all of one disk to all of the other can be obviated by using a split nut in the thrust bearing and providing proper means for opening and closing it according to whether or not the screw adjustment is to be used.

A SELF-RECORDING HUMAN MAZE

By LEON B. SLATER, Ohio Wesleyan University

The self-recording human maze described here is an outgrowth of a peg maze devised by J. F. Shepard at the University of Michigan. The added features which make possible the automatic recording of objective behavior in the maze learning process are set forth in the accompanying line cut. The apparatus is mounted on a laboratory table. At each end of the table is a transverse track made of angle-iron on which the whole assembly moves on the larger carriage from side to side. The smaller carriage moves lengthwise on the larger one. In this way means are provided for rotating the maze regardless of the eccentricity of the point selected in the maze as a center for making a turn.

A stationary screen constructed of ordinary wall-board prevents *S* from seeing the plan of the maze but provides him room to reach forward in order to move the maze as he finds it necessary for operating the apparatus. *S* acquaints himself with the maze by looking through a fixed periscope which has a visual field with a diameter slightly more than the width of a single alley.

To run the maze *S* starts with 'A' in the periscopic field. He draws the apparatus toward him until he reaches a turn in the alley. At this time *S* steps on a foot-treadle which forces a sharpened metal peg through a mounting in the table-top and into the bottom surface of the maze disk, thereby holding the visual field at the point of entrance to the next alley as the turn is being made. The *S*s report that by restricting all movement through the alleys to movement toward them, they derive a persistent impression of actually walking or progressing up the pathways until they arrive at the end, 'B.'

A pencil or marking pen which is set up on the second large disk marks out the exact pathways which are resorted to in the series of attempts to learn the true path.

The apparatus has the following advantages over the other human mazes. (1) It records objectively and permanently all errors by registering the direction of the path taken at any point during a run, the extent of all deviations from the true path, the persistence of errors in a series of runs, the elimination of errors as learning takes place, the exact distance covered in each run. (2) It can be used for any type of maze pattern which can be drawn on coördinate graph paper. (3) It provides almost immediately a complete disorientation of *S* in the maze. (4) It can be used by students as a standard piece of laboratory apparatus. (5) It affords more precision in an instrument for research.

APPARATUS NOTES

A SIMPLIFICATION OF THE RUCKMICK-PATTERSON NON-POLARIZABLE ELECTRODE

In the course of setting up apparatus for the galvanic reflex, we decided to utilize the electrodes described by Ruckmick and Patterson.[1] Since we were inexpert at glass blowing we appealed to the Department of Chemistry for assistance. While waiting we fashioned some substitutes in our own shop that have proven equal in every respect to the more complicated forms of Ruckmick and Patterson.

We took two thistle tubes with long stems (30 cm.) and bent the stem of each tube at about the middle into a 'U' so that the end of the stem was brought up close to the top of the funnel. The 'U' was filled with mercury to the bottom of the bowl. In all other respects, we followed Ruckmick's directions. The advantages that we have to urge for our modification are: (1) the ease of construction, (2) cheapness, and (3) the smaller change in level of the liquid, when the finger is inserted, due to the size and shape of the thistle tube.

Hobart College

FORREST L. DIMMICK
ALGER L. ADAMS

A MODIFICATION OF THE DALLENBACH HEAT GRILL

The copper tubes used in the construction of the Dallenbach heat grill[2] are small (1/16 in. bore) so as to bring the two systems of the grill in close juxtaposition and thus to stimulate differentially narrow and closely integrated areas of the skin. This construction meets the requirements of stimulation but it does not yield a flexible means of varying the stimulus-temperatures.

The temperature of each grill-system, it will be recalled, is controlled by forcing water, by means of pressure from the mains, through copper coils immersed in constant temperature water-baths (hot or cold) and by varying the water's rate of flow through each system. Because of the small bore of the grill-tubes, the limits to which the stimulus-temperatures may be changed by this means is small. If these limits are to be exceeded, the temperature of the water-baths must be changed. This alteration is not only time-consuming, but it requires delicate adjustment of special thermostatic apparatus. In order to avoid these complications the grill described here was constructed of ¼ in. tubing which was rolled flat so as to give a bore ⅙ x ⅓ in. The construction was otherwise similar to the older form.

The larger bore of the grill-systems of the modified apparatus allows a greater volume of water to pass through the tubes and hence increases greatly the limits to which the stimulus-temperatures may be varied. Indeed, it is possible with this modification to obtain the range of temperatures desired merely by varying the rate of flow through baths of boiling and salted ice water. Since the temperature of these baths is constant, thermostatic controls are not required.

Columbia University

MEYER KOCH

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[1] C. A. Ruckmick and E. Patterson, A simple non–polarizing electrode, this JOURNAL, 41, 1929, 120.

[2] For a description see N. C. Burnett and K. M. Dallenbach, The experience of heat, this JOURNAL, 38, 1927, 419.

NOTES AND DISCUSSIONS

VOCATIONAL COUNSELING: THE INTEREST INVENTORY

Groups of men who are engaged in certain occupations sometimes have characteristic likes and dislikes which distinguish them from groups following other occupations. Although the careful student of vocational and occupational interests is loath to overgeneralize this fact, immature persons who are trying to decide upon their life work are likely to gain the impression that this finding is of great significance. Students occasionally believe that they may employ a vocational interest inventory to determine, with a fair degree of accuracy, whether they should or should not enter a particular occupation. And the adviser often places unwarranted emphasis upon the alleged interest of the student. This practice has definite limitations which will be discussed briefly in this paper.

Searching for "psychological types." It is interesting that long after a psychological concept has been discarded, it often survives in disguised form. For example, in spite of the recognition of the general applicability of the normal distribution surface, some investigators continue to speak and to write in a manner which shows that they are still thinking in terms of bi-modal or multi-modal distributions. Certain psychologists, dealing with vocational and occupational interests, apparently believe that they have discovered several types of workers; *e.g.* those interested in people, those interested in things, and those interested in ideals. Such a threefold classification is obviously arbitrary; it is also invalid, if it is applied generally, for most individuals are interested in people as well as in things, and also in ideals!

To the present writers, unguarded optimism with reference to the practical application of the vocational interest inventory is comparable to the early attempt to adapt instruction to the needs of "eye-learners" and "ear-learners." Group differences are magnified; similarities are minimized and often overlooked entirely. By selecting those likes and dislikes which differentiate groups in several occupations (and neglecting the large number of likes and dislikes which members of the various occupational groups have in common), the illusion of "types" is created. It may well be that at some time in the future this search for types will prove profitable (scientifically) and that the type theory will yet receive verification in the hands of competent investigators. Nevertheless, until the evidence is at hand, the fact cannot be too strongly emphasized that the burden of proof rests upon those who continue to believe in the existence of psychological types.

Adult interests vs. youth's potential interests. The scores on the vocational interest inventory show how nearly a person's interests coincide with those of the average mature individual successfully engaged in particular occupations. The value of this information to the vocational counselor rests upon the validity of the assumption that the individual's interests, preferences, likes and dislikes, are somewhat constant factors which may be measured and labelled before the individual has reached maturity, and also before he has had occupational experience. This assumption disregards the fact that motivation modifies and indeed creates interests. According to Woodworth, "almost any object, almost any act, and particularly almost any process or change in objects that can direct one's own activity towards some definite end, is interesting of its own account, and furnishes its own drive, once it is fairly initiated."[1]

Assume that mature, experienced workers in one group *do* have preferences and inclinations which differentiate them sharply from those in other occupational groups. Does it follow that as youths these workers possessed their present stock of interests? Hardly! The occupational *knowledges* possessed by various kinds of workers probably differentiate occupational groups—even more sharply than do the characteristic likes and dislikes. It is hazardous to use tests of *occupational knowledge* to identify potentially successful workers, for the knowledges are to a large extent the result of acquisition. Woodworth's assertion (as well as common sense) suggests that most *interests* are acquired. Surely, successful endeavor is one fact of great importance in creating occupational interest. It is extremely unlikely that the chaotic and frequently undirected activities of growing youth will lead to interests that have much bearing upon occupational fitness or success.

It will be of interest to trace the genesis of one occupational interest. A salesman tries to sell certain products. He calls on his prospective customers week after week. In the course of time he becomes accustomed to meeting and dealing with people. He becomes 'interested in people,' *e.g.* his prospective customers. Does this suggest that his interest in people is due largely to the fact that he is, by original nature, or as a result of early experience, "interested in people"? His interest is likely to be ulterior, and often it is clearly acquired. Of course, one may become habituated to associating with people, and therefore one may acquire a genuine 'interest' in people. It is more probable that the interest in people is not a *general* interest or a true reflection of innate sociability. The sociability is likely to be the reflection of a habituated response from necessary contact with certain people. It is therefore misleading to point to certain prospective workers catalogued as those *interested in people.* It is extremely likely that many, if not most occupational interests, are acquired as correlates to particular occupational endeavor. Prediction of these adult interests would be therefore unlikely from a study of the reactions of these persons in youth.

The origin and the derivation of 'interests.' In connection with the preceding analysis of the development of interests, the following question is pertinent. What is it that initiates and directs vocational interests? A partial answer is given by Douglas Fryer, who studied the interest histories of forty superior adults, and listed five general causes of the development of interests.

> "First, perhaps, is the economic cause, which is seen in almost every history. Vocations are determined, which are very happy choices, by the fact that they minister to the vital needs of life. A second cause, possibly equally important, is the chance stimulation of the environment. Interests are made by the fact that the individual has chance contact with certain objects and activities. A third cause, also equally important, and the one most often mentioned in these histories, is a personality, or personalities, older than the subject, who suggest a line of development. . . . The personality influence as a determiner of interests has, it would seem, been neglected in the understanding of the development of interests. Fourth, as a causative factor, is the general motivation of the individual to be recognized among his fellows. This is an obvious cause to most clinicians. The intrinsic factors of an interest seem less important in most of these histories, but they constitute a

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[1] R. S. Woodworth, *Dynamic Psychology,* 1918, 202.

fifth general cause in the development of interests. . . . it will usually be observed that the prestige of a vocation, the general motivation to be somebody in relation to one's fellows, is equally important. . . . Generally, intrinsic interests seem not to be strong causative factors in the development of the interests of these subjects."[2]

Fryer states that economic need and the desire for social recognition are extremely important determiners of vocational interests. Chance stimulation of the environment and the example of influential friends also are actuators of interest. Possibly these causes of 'interests' explain in part why scores on the interest inventories ordinarily fail to show any marked correlation with measures of occupational success. In many instances, particularly among young workers, the interests develop with experience, chance relationships, and countless other environmental factors. These factors are difficult to anticipate, and therefore they are seldom represented in the interest inventory.

'Interest' scores as predictors of future success. Certain early investigators believed that interests might prove indicative of ability and therefore might be valuable prognosticators of occupational success. Nevertheless, vocational interest scores show practically no correlation with degrees of vocational success. Ream administered the Carnegie Interest Inventory to groups of successful and unsuccessful salesmen.[3] The scores were ascertained for the successful and for the unsuccessful, and differential score patterns were established. The experiment was repeated with a new group of salesmen, but the differentiation could not be similarly established from the scoring pattern.

Craig made a comparable effort to differentiate between successful and unsuccessful saleswomen.[4] Excellent differentiation was obtained within the primary groups; a scoring key was then devised. When a control group was studied similarly, the scoring key failed to differentiate the successful from the unsuccessful. Freyd tried to distinguish between successful and unsuccessful salesmen,[5] and he obtained results similar to those of Ream and Craig. Kornhauser attempted to distinguish successful from unsuccessful college students by studying their interests.[6] His attempt also was unsuccessful.

For the occupations that he has thus far studied, Strong reports a negligible coefficient of correlation between degree of vocational success and vocational interest score.[7] Garretson,[8] who worked with ninth-grade boys, reports that "little relationship exists between the degree of preference for and ability in a curriculum as measured by objective tests of curricular aptitude. The range of coefficients of correlation between preference scores and measures of curriculum ability is +0.190 ±.055 to −0.304 ± .52."[8]

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[2] D. Fryer, *The Measurement of Interests*, 1931, 413-414.

[3] M. J. Ream, *Ability to Sell: Its Relation to Certain Aspects of Personality and Experience*, 1924, 47-48.

[4] R. D. Craig, The preference-interest questionnaire in selecting retail saleswomen, *J. Personnel Research*, 3, 1924-1925, 366-374.

[5] M. Freyd, Selection of promotion salesmen, *J. Personnel Research*, 5, 1926-1927, 142-156.

[6] A. W. Kornhauser, Results from a questionnaire of likes and dislikes used with a group of college freshmen, *J. Appl. Psychol.*, 11, 1927, 85-94.

[7] E. K. Strong, Jr., *Manual for Vocational Interest Blank*, 1930, 6 f.

[8] O. K. Garretson, Relationships between expressed preferences and curricular abilities of ninth grade boys, *Teachers Coll. Cont. Educ.*, 396, 1930, 72.

These data show conclusively our inability to distinguish between successful and unsuccessful individuals by means of the present-day interest inventories.

Of course this situation may not obtain for all kinds of work and workers. Group interests may some day be identified which will differentiate successful from unsuccessful workers within a particular occupational field. Nevertheless, it seems to the present writers that the burden of proof rests upon those who assume that interests are highly indicative of ability.

The interest inventory and the high school pupil. In view of the foregoing data, the writers believe that it is unwise to give boys of high school age the impression that they may follow their present inclinations to ultimate advantage. In some instances such a policy might be justified by the results. In a far larger number of instances it probably would not prove fruitful. In the growing individual, interests and inclinations are not constant factors; they are subject to change. Abilities not yet tapped may be the potential sources of new interests and inclinations. Moreover, intensity of effort during the initial periods of almost any endeavor is likely to be accompanied by dissatisfyingness. Later the activity itself may furnish the stimulus to further activity, but the initial periods are frequently attended by dissatisfyingness. This is particularly true in the initial periods of developing a skill, but it is probably true also in the case of the young worker when he is becoming adjusted to his life work. There are also many vocational interests which are transitory. Indeed, it would be difficult to find a characteristic of a growing boy more subject to change than his vocational ambitions.

Many recognize the fact that interests often are transitory, and that our present stock of guidance devices are inchoate. For example, Strong states that he has obtained no data for high school boys,[9] and that it will be many years before the validity of employing his inventory for counseling high school youths can be determined.[10]

Strong therefore urges caution in the use of his inventory. Nevertheless, the interest inventory is now being used occasionally by optimistic and over-enthusiastic counselors. In his recent book, *How to Find the Right Vocation,* Kitson describes how the device may be employed.

> "The test [the interest inventory] has been given to workers in about thirty occupations and count has been made of the frequency with which the workers in a single occupation, such as accountancy, prefer certain things in the list. Then it is given to a boy just being graduated from high school or just entering college. His likes and dislikes are then compared with those of the persons in these thirty occupational groups. If he likes the same things as those liked by the accountants, he is encouraged to enter that occupation. If, however, his likes correspond to those of physicians, he is encouraged to enter medicine." [11]

Although Kitson mentions some of the limitations of the interest inventory, he does not present the limitations in such a way that proper caution will be exercised by those who are trying "to find the right vocation."

The Vocational Interest Quotient (*V.I.Q.*). There has recently come to the attention of the writers a device for ascertaining pupils' Vocational Interest Quotients (V.I.Q.). The Manual of Directions that accompanies this device contains the following explanatory statement.

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[9] *Op. cit.*, 6. [10] *Op. cit.*, 1.

[11] H. D. Kitson, *How to Find the Right Vocation*, 1929, 134.

"For a number of years, I have sought to devise a convenient measure of vocational interest that would be *as helpful in its field as the I.Q. has been in mental testing.* My investigations have resulted in the invention of the Vocational Interest Quotient (V.I.Q.) and I feel that these have now been carried far enough to warrant *practical use* of the device and to invite wider coöperation in *further establishing its usefulness*" (italics ours).[12]

In the judgment of the present writers the foregoing attempt is at present unwarranted as well as injudicious. It is rather commonly charged that the psychologist often presents new concepts or laws before he has verified them. The use of the V.I.Q. seems to be an instance of presenting a new method (which will have significant social consequences if it is used) before it has been thoroughly tested. Why should the V.I.Q. be put to "practical use" *before* its usefulness has been thoroughly established? Certainly, the present writers have been unable to find evidence which shows any significant relationship between V.I.Q. scores and anything else of significance in demonstrating the validity of the device.

Other aspects of the V.I.Q. merit attention: the similarity of the names, I.Q. and V.I.Q., and the attempt of the author to imply similarity between the two measures. The symbol I.Q. is of course widely known and its significance is generally understood. The novice may assume that the I.Q. and V.I.Q. are alike in many respects. Perusal of the V.I.Q. literature reveals that this supposition has no basis in fact. For example, the I.Q. represents ability that is largely native and relatively constant or stable. The I.Q. therefore makes certain kinds of prediction possible. The V.I.Q. probably does not reflect native as contrasted with acquired abilities; the constancy of the V.I.Q. pattern certainly has not been demonstrated; the individual has not one but several.

The writers do not intentionally belittle sincere and honest research in any field whatsoever. What the writers do object to is the *"practical use"* of a tool, the validity of which has not been demonstrated scientifically. The Manual of Directions instructs the examinee to compute his V.I.Q. for the occupations in the booklet.[13] He should then "rank his V.I.Q.'s in descending order and consider most seriously those occupations that are in the upper quarter of rank."

Even the novice in psychology must realize that the suggestibility of pupils would nullify much of the validity of data obtained by such a procedure. As a research tool the vocational interest inventory has a legitimate place; the present writers submit that, in our present state of knowledge, its "practical use" is a bit premature.

Ohio University — Harvey C. Lehman
Northwestern University — Paul A. Witty

The Intensive Factor in the Localization of Sound

The purpose of this note is to suggest a possible means by which dichotic intensity-differences mediate localization of sound by virtue of their reduction to time-differences. If we assume the essential identity of phase-differences and time-differences, as stated by Boring,[1] all localization of sound might then be supposed to depend on a single, peripherally effected criterion.

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[12] H. W. Hepner, *Skilled Trades for Men: Directions for Finding Your Vocational Interest Quotients (V.I.Q.)*, 1931, 1.

[13] Hepner, *Manual of Directions for Counselors Using Vocational Interest Quotient Booklets*, 1931, 4.

If the fibers of the basilar membrane vibrate in resonance to a given imposed frequency, we may follow Banister's[2] suggestion that nerve-impulses in the auditory fibers are initiated when, as far as the cilia are concerned, a certain phase of their cycle has been reached. Perhaps it is more accurate to say, with Troland,[3] that the liberation of the nerve-impulses takes place at a more or less definite magnitude of displacement of the basilar membrane. The movement of the basilar membrane would then be correlated with a variable amount of tension or pressure on the hair cells. Hartridge[4] thinks that impulses are initiated during the downward movement of the basilar membrane, a belief which implies that the threshold of stimulation corresponds to a more or less fixed amount of tension on the hair cells. With very weak intensities the displacement of the basilar membrane would be insufficient for the requisite tension. With great intensities, however, the basilar membrane would continue to move after the threshold tension had been attained. When a sound is just audible, we may suppose that the threshold tension is reached at the point of maximum displacement of the basilar membrane, whereas in the case of intense sounds the threshold must be reached before the peak of the cycle.

The process outlined thus far is supported by Troland's[5] conception of the initiation of the series of temporal differences responsible for the curvature in the neural wave-front, which he presents as the neurological correlate of the experience of tonal brightness. The center of the displaced strip of basilar membrane moves most rapidly and stimulates hair cells soonest. From the center towards either end of the displaced area, movement of the basilar membrane is progressively slower, and impulses are initiated progressively later in the cycle. It seems that this notion is capable of a further extension. When frequency is diotic and intensity dichotic, a certain fiber or group of fibers in the more intensely stimulated ear must initiate impulses prior to any of those initiated in the less intensely stimulated ear. In this way dichotic intensity-differences are correlated with differences in time.

This conception may be invoked to explain the superiority of dichotic phase-differences over dichotic intensity-differences in auditory localization. In the case of intensity-differences there must be a series of time-differences corresponding to the wave-fronts in both ears. In other words, the differences of time include more than those of the hair cells responding to maximally displaced basilar fibers in both ears. The spacing of the temporal factor might well reduce accuracy of localization.

If, as Hartridge thinks, the hair cells are stimulated only during the downward movement of the basilar membrane, it follows that the upward movement is lost in so far as excitatory effectiveness is concerned. The limit of time-variation depends on the amount of displacement necessary for excitation. Assuming that the limiting points are the position of rest and the position of greatest downward movement of the basilar membrane, ¼-wave would be available. But this is a minimum, since the hair cells in their 'resting' positions might be past the points of discharge. Adaptation would prevent response of the hair cells in their 'resting' positions, but the level of adaptation might well change, so that with continuous stimulation the limiting positions could conceivably become the maximum upward and the maximum downward displacements of the basilar membrane. In this way, ½-wave might become the outside limit of time-variation.

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[1] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 164-173.

[2] H. Banister, A suggestion towards a new hypothesis regarding localization of sound, *Brit. J. Psychol.*, 17, 1926, 142-153.

[3] L. T. Troland, The psychophysiology of auditory qualities and attributes, *J. Gen. Psychol.*, 2, 1929, 28-56.

[4] H. Hartridge, A vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 12, 1922, 362-382.

[5] Troland, *op. cit.*, 47-51.

It must be admitted that the establishment of a temporal gradient as the essential factor in auditory localization would complicate the conception of brain events. Boring[6] has argued for the relation of conscious dimensions to the physical dimensions of brain processes. An intensive shift in the brain may be correlated with a spatial shift in the excitatory mode, and seems natural as the ground for a change in localization. With time-differences as the primary criteria, it becomes necessary to translate them somehow into spatial dimensions. Nevertheless the establishment of concomitant time-differences with differences of intensity does not create any essentially new theoretical difficulties, since any assumptions concerning cortical events with dichotic phase-differences and time-differences may be extended to account for the time-factor derived from the intensity-factor as suggested in this paper.

Tufts College W. A. BOUSFIELD

THE PROGRESSIVE-WEIGHTS TEST OF SUGGESTIBILITY WITH ADULTS

A lifted-weight test devised by Binet for measuring suggestibility in children[1] was adapted by the writer and given to 187 *Ss* in the Ozark highlands. The *Ss* were drawn from a region divided occupationally between agriculture, commerce, manufacturing, and mining, coming from every size of community, from open country to a city of 35,000. The age-range was 17-55 yr., with the mean at 30. There were 90 men and 97 women. Social and economic backgrounds varied widely, occupations ranging from unskilled labor to professional service. Ten (5.5%) had less than an eighth-grade education; 42 (22.5%) had completed the eighth grade; 84 (44.8%) had had 1-4 yr. of high school; 6 (3.2%) had technical or professional training. The *Ss* therefore form a representative sampling of the local population, within the age range set.

Before submitting to the test, all had marked a ballot on matters of belief and opinion in religion,[2] and had worked with a line-length test.[3] None were familiar with the psychological laboratory and its methods, a high percentage were suspicious of anything resembling a clinic or operating room, and in preliminary experimentation many showed that they suspected magic or trickery. The 'weights' test was therefore given in the home or place of employment, on any table or counter or work-bench of the right height (30 in.) available.

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[6] Boring, The physiology of consciousness, *Science*, 75, 1932, 32-39.

[1] A. Binet, La suggestibilité, 1900, ch. iv. Cf. G. M. Whipple, *Manual of Mental and Physical Tests*, 1921 (3rd ed.), vol. 2, 232-236.

[2] See P. R. Stevick, A sampling of Ozark creeds, *Mountain Life and Work*, 7, 1932, 20-23.

[3] Whipple, *op. cit.*, 242-246.

A set of 15 weights, identical in size and appearance, with the exception of conspicuous numbering 1-15, were used. The first four weighed 20, 40, 60, and 80 grm., respectively, the remaining eleven weighed 100 grm. each. They were arranged in serial order, with No. 1 at the left, and about 2 cm. between weights. While they were being arranged, *E* took care to exhibit the brass end, showing that the shell was a dummy. He also called attention to the identical size, shape, and color of the weights and to the number at the top.

The instructions to *S* were as follows:

> What I want you to do is to lift each one just once, like this (here *E* began lifting weights one at a time), and as you lift it you are to tell me whether it is heavier, lighter, or the same as the one just before it (by this time *E* had lifted the first four weights). Do you see? You lift each weight *just once* (here *E* demonstrated a second time) and as you lift it you tell me whether it is heavier, lighter, or the same as the one just before it.

As each weight was lifted, *S*'s report was recorded. If he hesitated he was asked, "What is your first guess?"

The number of 'heavier' judgments after Weight 5 until a 'same' or 'lighter' judgment broke the progressive series constitutes the score. Only 3 *Ss*, a chemical engineer, a dairyman, and a library desk clerk, judged the entire series correctly. The frequencies for scores 0-10 are 116, 35, 11, 8, 3, 3, 2, 1, 1, 1, 6. Sixty-two percentage (116) show no signs of a suggestion of progressive increase. Only about 20% register more than 1 score-point in suggestibility. Failure of the test to yield anything approximating a normal distribution indicates its extremely limited value with untrained adults. The *r*, moreover, for correlation with a measure of feeling of conformity to majority opinion on religious belief is −0.007. This is in line with recent research, which points away from such general traits as suggestibility.

Ozark Wesleyan College PAUL R. STEVICK

THE METHOD OF SINGLE STIMULI

An increasing interest in absolute judgments and absolute methods makes it desirable to revise the terminology used in discussing these judgments and methods.[1] The following terms have emerged from a consultation with Professors E. G. Boring, S. W. Fernberger, and C. C. Pratt.

(1) *Upper category:* those reports which will surpass the other reports in frequency if the stimulus-magnitude is sufficiently increased.

(2) *Lower category:* those reports which will surpass the other reports in frequency if the stimulus-magnitude is sufficiently decreased.

(3) *Intermediate category or categories:* those reports which will decline in frequency as the upper category increases on one side, and as the lower category increases on the other.

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[1] Two principal references are: E. G. Wever and K. E. Zener, The method of absolute judgments in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493; S. W. Fernberger, On absolute and relative judgments in lifted weight experiments, this JOURNAL, 43, 1931, 560-578.

(4) *Limen*, with the symbol L: that value of stimulus, arrived at by interpolation, at which the probability of any given report except an intermediate report equals the summed probabilities of the other reports.

(5) *Upper limen*, with the symbol L_U: the limen of the upper category.

(6) *Lower limen*, with the symbol L_L: the limen of the lower category.

(7) *Central interval*, with the symbol CI: $L_U - L_L$ taken without regard to sign.

(8) *Point of symmetry*, with the symbol PS. Experimenters must choose between the following definitions:

(a) the mean of the upper and lower limens, $(L_U + L_L)/2$;

(b) Σi, the point at which the psychometric functions of the upper category and the lower category intersect;

(c) the mean of the distribution of reports in the intermediate category;

(d) the mode of the distribution of reports in the intermediate category.

(9) *Method of single stimuli:* any psychophysical method in which a report follows the presentation of one stimulus only.

The first six definitions apply to the method of constant stimuli or to the method of constant stimulus differences.[2] L is Urban's S. *Central interval* corresponds to *interval of uncertainty* in the method of constant stimulus differences, and the definition is the same; it seems best, however, to avoid the term *uncertainty* because it is not clear what that term would mean in an absolute method. *Point of symmetry*[3] takes the place of *point of subjective equality*, because *equality* has no meaning in an absolute method. Formulae for calculating PS are given elsewhere.[4]

A psychophysical method may be designated either by the process of judgment which the observer is believed to use, or by the general arrangement of stimuli appropriate to the method. There may be disagreement about the former, but not about the latter; hence *method of single stimuli*[5] seems better than *absolute method*. *Method of single stimuli* quite properly includes the method of constant stimuli, which is used in determining stimulus-limens and terminal limens. *Absolute judgment* and *absolute method* are misnomers if *absolute* is opposed to relative: an absolute judgment upon one stimulus is not independent of similar judgments upon preceding stimuli, and absolute judgments are accordingly relative to other absolute judgments. But *absolute* has a proper meaning if it is opposed to *comparative*: an absolute judgment need not involve a comparison of the given stimulus with some given preceding stimulus. In this sense, terms such as *absolute judgment*, *absolute impression*, *absolute series* are held to be admissable.

Harvard University JOHN VOLKMANN

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[2] The substance of several of the definitions is taken from E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

[3] This term has already appeared: S. W. Fernberger, Time relations for the different categories of judgment in the 'absolute method' in psychophysics, this JOURNAL, 44, 1932, 509.

[4] Boring, *op. cit.*, 288 f.

[5] C. S. Myers has used the term *method of single exposures* to designate an absolute method: *Text-book of Experimental Psychology*, I, 1911, 306 f.

THE SEVENTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

On Friday and Saturday, May 13 and 14, the University of Indiana at Bloomington was host to the Midwestern Psychological Association during its seventh annual meeting. The meetings were largely attended and a wide geographical distribution of states was represented. Professor Herbert Woodrow, Head of the Department of Psychology at the University of Illinois, served as President. There were seven sectional meetings during which 36 papers were read and discussed and 15 laboratory reports were given. These papers had been carefully selected by the Program Committee from among three times that number which were offered. Besides these regular sessions there was a symposium on learning, which was held on Friday evening under the chairmanship of President W. L. Bryan of the University, who also served as toastmaster at the annual dinner which was held at the Memorial Union Building on Saturday evening. The Department of Philosophy invited the members to a lecture by Professor E. S. Ames (Chicago) on "Imagery and Meaning in Religious Ideas" held on Thursday evening. The annual business meeting occurred on Saturday afternoon. The principal items of business were the reports from the Council, from the Nominating Committee, and from the secretary. It was decided to accept the invitation of the Iowa State College at Ames for the next meeting to be held preferably during the first week of May, 1933. The Council of the Association attended a dinner on Friday evening after which a short business session was held. The Committee of Fifteen on the first course in psychology was represented by nine members and met on Friday morning to discuss the reports of its sub-committees. Professor J. P. Nafe was elected to the general committee to fill the vacancy caused by the death of Professor A. P. Weiss.

The Psi Chi National Honorary Fraternity held a luncheon on Friday noon. President and Mrs W. L. Bryan invited the members of the Association to tea at their residence at the close of the sectional meetings on Friday afternoon. Motor trips were also organized to provide members with an opportunity of visiting some of the scenic landscapes along Hubbard's Ridge in the State Park of Brown County.

The first paper presented during the symposium on learning was on "Gestalt Psychology and Learning" by Professor R. H. Wheeler (Kansas). It was discussed by Professor J. A. McGeoch (Missouri). It was followed by a paper on "The Double Rôle of the Primary Stimulus in Conditioning" by Professor E. A. Culler (Illinois). The third paper was presented by Professor J. Peterson (Peabody College for Teachers) on "Some Developments in the Psychology of Learning in the Last Thirty Years" to which Professor H. B. English (Ohio State) responded. There was a very lively discussion on the part of the audience, principally of Dr. Wheeler's paper, in which Professor C. E. Spearman (London) participated.

In the first section, over which L. L. Thurstone (Chicago) presided, L. D. Goodfellow (Northwestern) compared the ability of the eye, ear, and finger-tip to discriminate short intervals of time. Through the use of three psychophysical procedures, right and wrong cases, just noticeable differences, and average error, he showed that audition gave the smallest differential threshold, cutaneous pressure the next smallest, and vision the largest. There were considerable individual differences in the absolute thresholds, in the amount of learning, and in the relative threshold values for the different techniques. J. P. Cartzdafner (Ohio Wesleyan) followed with a comparison of the psychophysical methods. By means of the procedure of average error, of limits, and of constant stimuli and through the use of specially devised apparatus, which was a modified form of Galton bar, he obtained 5 00 judgments from 10 *O*s. He computed the difference-threshold as the average transitional point between the 'just perceptibly different' and the 'just imperceptibly different' points. He concluded that the judgments of equality in the constant method are so significant that they should not be discarded; that the assumption regarding the limits of the tails in the curve of distribution is valid and verifiable; that the differences in the threshold as revealed by the various psychophysical procedures are a function of the procedures themselves and not a function of the mathematical processes involved; and that within the scope of this experiment the constant procedure gives the lowest threshold values and the greatest precision. R. Leeper (Arkansas) furnished "the evidence for a theory of neurological maintenance of states of emotional motivation." He cited examples to show that the central nervous tissues, in contrast with the peripheral, may follow different principles of operation; that the cortex is capable of independent maintenance of simple attitudes; that the subcortical structures like the thalamus must be capable of persistent independent activity; and that there may be similar analogous mechanisms in the cortex for the maintenance of motivation as a result of training. J. R. Patrick and E. Rowles (Ohio) reported the effect of various stimuli on basal metabolic rate, blood pressure and galvanic reflex. "Adrenalin and emotion" was the subject of a paper offered by C. Landis (N. Y. Psychiat. Inst. & Hosp.). By injecting adrenalin into normal and psychopathological individuals he found a wide variation in mental and physiological results. But in some cases a genuine pleasant or unpleasant emotional experience was aroused. He did not believe the theory of imbalance of the divisions of the autonomic system was substantiated by his results and that the emotional experience must always be related to the entire environmental setting in which intellectual processes may also play an important rôle. The session was closed by a paper read by E. Jacobson (Chicago) on the electrophysiology of mental activities. By means of apparatus developed to measure transient potential differences as small as one millionth of a volt, he has been able to measure neuromuscular states during such mental activities as recollection, and visual, auditory, and kinaesthetic imagination. Specific muscles contracted during each type of mental activity investigated.

Under the chairmanship of J. B. Miner (Kentucky) a parallel section of papers opened with a study by M. H. Young (Indiana) on clinical psychology in industry and the professions. The author is continuing the work begun by her late husband, Dr. H. H. Young, who laid out an adjustment program with a large industrial concern. In conjunction with coöperating medical experts she has analyzed the mental difficulties of 54 subjects and classified their difficulties for further psychological study. R. Stagner (Wisconsin) then presented a paper on the relationship of certain personality traits to academic aptitude and achievement. A number of traits, like those of neurotic temperament, self-sufficiency, and dominance were investigated in connection with academic maladjustments. He indicated that stable individuals, very self-sufficient students, and those receiving high scores for dominance, achieve academic results that are more closely related to their capacity than are those found in contrasted groups. H. L. Koch (Chicago) next read a paper entitled "A Study in Rating Technique with Special Reference to Activity in Pre-School Children." Fifty pre-school children were observed in 9 types of pursuits. The author suggested the danger of using a child's behavior during one type of occupation as an index of his behavior during all others and she advised the importance of taking interactivity correlations into account. Activeness seemed to be influenced in a complex way by room-temperature but not by humidity within the ranges studied. H. M. Williams (Iowa) reported an analytical study of the expressive use of language by pre-school children. Using 72 children he found that development occurred along three lines: the skill in the mechanism of speech; the organisation of the sentence; and extent of vocabulary. The correlation between scores and intelligence were of the order of 0.6. A scale of block constructions for young children was proposed by M. W. Bailey (Ohio Wesleyan). Working with 44 children, ranging from 2–9 yr. of age, a standardized project of building anything they wished was developed as a graded series which was judged in terms of 10 photographs ranked according to the method of equal appearing intervals. F. N. Maxfield (Ohio State) next discussed the picture element in certain test material, and raised the question whether several of the puzzle-picture tests were not really form tests. He made a proposal involving true pictures of the completion type for very young children. The last paper was read by D. Gohring (Wisconsin) on variations in strength of emotional responses in newborn infants. Sixty infants from 1–10 days of age were tested for strength of motor, vocal, and blushing responses measured on a 4-point scale under the situation of restraining the arm-activity for 40 sec. and of removal of support by jerking the blanket. Motor responses to jerking the blanket seemed to be non-emotional righting reflexes in regaining balance; the infrequent vocal responses were usually only a whimper; and blushing responses almost never occurred.

W. F. Book (Indiana) served as chairman of the third section which began the afternoon program. F. McKinney (Missouri) read the first paper on the quantitative and qualitative essential elements of transfer. He was concerned with the problem of ascertaining how many and what elements of a given original situation are adequate to evoke a given response under conditions of transfer. Seventy-five associated four simple geometrical forms with four letters of the alphabet. In an altered situation in which a graded series of modifications or forms were made, the *S* was asked to write the letter associated with each modified figure presented. Transfer was found not to be proportional to the amount of the identical stimulus present but it varied with the position of the modified quantity or with its relationship to the rest of the figure. The relative retention values of stylus and mental maze habits was discussed by R. H. Waters and G. B. Poole (Arkansas). They tested the hypothesis that the apparent better retention of motor habits than verbal habits is due to the factor of greater over-learning. By rigidly controlling the factor of over-learning they concluded that the result was a function of the relative degree of complete learning. A long report of an experiment on learning and time by G. S. Snoddy (Indiana) then followed. On the basis of numerous control situations he came to the conclusion that learning involves adaptation to a whole situation and a total set of temporal conditions. Coordination then becomes an item in a total space-time configuration involving a contraction of these dimensions during the learning period. Learning curves can hence be readily fitted by simple logarithmic equations. Through fractionation of the learning period the impression is gained that all of the elements of the learning curve can be explained in terms of the energy-cycles of thermodynamics. W. Schwarzbek (Ohio State) next presented a paper on the influence of variable intercyclic intervals on pursuitmeter learning, and a new method of treating learning data. By testing 12 groups of a total of 86 *S*s with the pursuitmeter he came to the conclusion that some theories of learning wrongly assume an original re-enforcement of correct responses with a weakening of this tendency in the course of time. His experiments indicated that under these conditions there might have been an optimal interval of rest between arbitrary practice-units as much as 48 hr. in length. C. R. Griffith (Illinois) discussed a similar problem of maturation under the titel of "The Phenomena of Off-season Learning." By measuring in successive years the initial, maximal, and final skills of basketball players, tennis players, baseball players, and golfers, he concluded that the phenomena of off-season learning do not exist and that the increased efficiency after a lapse of time in learning may be of the nature of better judgment rather than of increased technical skill. This section was concluded by the report of an experimental study of Thorndike's theory of learning by M. Hayden (Wisconsin). She used 4 procedures including the one advocated by Thorndike in order to investigate other factors, besides the satisfaction of *E*'s comment which influenced improvement in efficiency.

A parallel section, over which J. R. Kantor (Indiana) presided, was opened by a paper on the relative rôles of wave-phase and acoustic intensity in sound-localization offered by O. C. Trimble (Purdue). By studying the effect of the respective factors with tones ranging from 125–7500 d.v. with 4 trained *O*s he discovered that localization on the basis of intensity is more immediate and definite than on the basis of phase. Phase is adequate for tones up to about 1250 d.v. This predominance of intensity over phase is effective even when the former is opposed to the latter. In the absence of E. Lindermann (Iowa) his paper on the distortion of space perception produced by *Cannabis Indica* was read by C. A. Ruckmick (Iowa). Eight out of 10 *O*s showed changes in perception which incorporated a dynamic relationship between the perceptual changes and the state of activity prevailing in other parts of the motor and sensory equipment of the individual. His findings discredit any isolated reflex theory involving the central nervous system on the basis of inhibition from higher centers when affected by toxic agents. H. F. Schick (Central Inst. for the Deaf) next presented a paper on a new interpretation of the action of the sweat-glands in connection with electrodes during experiments on affective responses. By means of diagrams the supposed contraction of the gland with its accompanying ejection of excretion in varying amounts was made responsible for the changing electrical resistances recorded. This paper was defended by M. F. Meyer (Missouri). A paper, by L. S. Woodburne (Michigan), on the effect of the visual angle on relative depth perception followed. In it the author questioned the classical assumption that the size of the object fixated was negligible in tri-dimensional perception. His apparatus was designed so that the size of the object could be made the same in appearance no matter what the actual distance from the *O* was. From 5000 judgments he concluded that the size of the visual angle certainly did not give a necessary cue in the accurate perception of relative depth. R. F. Wallace (Ohio State) then showed the effect of the distortion of stroboscopic figures due to the relative velocities of the stroboscopic figure and the slit that moved before the eye He. proposed and checked a formula giving a determination which correlated 0.996 with the empirical situation. The last paper in this section, on visual acuity and illumination, was given by W. W. Wilcox (Kansas). By devising an apparatus exposing bright bars on dark ground and dark bars on bright ground and providing for an adjustment of intensity and the physical widths of the bars, acuity was carefully measured. This theoretical curve showing variation of visual acuity with intensity closely agreed with experimental results.

The fifth session opened on Saturday morning under the chairmanship of H. Carr (Chicago) with a paper by H. F. Harlow (Wisconsin). By means of motion pictures he illustrated the fact that social facilitation of feeding in the rat and in the Rhesus monkey was independent of learning. A paper on comparative tests on a human and a chimpanzee infant of approximately the same age by W. N. Kellogg and L. A. Kellogg was also demonstrated by motion pictures. In eight different situations the comparative initial response and rate of learning were traced in the chimpanzee and human baby during four and one half months of growth in the home under exactly identical conditions. The chimpanzee was 7½ mo. old at the beginning of the study and the human baby 10 mo. old. The human did better in delayed reactions and in detouring. In the "cap-on-head" test the ape showed marked superiority. There were also distinct differences in reaction to ice and to rotation. P. E. Fields (Stanford) then reported some visual discriminations obtained with a new multiple type jumping apparatus. A number of patterns were exposed to 84 white rats in 165,000 trials. Figure discriminations were made much more rapidly than in the usual type of apparatus because the multiple form of presentation forced the rat to use visual stimuli to the exclusion of other sensory channels. The functions of the superior colliculi in vision were discussed by J. D. Layman (Chicago). By destroying the superior colliculi the effect upon the discrimination of light from darkness and of various types of patterns was studied. He found that the extensive lesions made did not destroy visual capacities tested. There was some defect, however, in visual functioning as indicated by the slower formation of habits and in the hesitancy of the rats to jump. K. S. Lashley and L. E. Wiley (Chicago) noted the retardation from cerebral lesion in mazes differing in complexity. One hundred and twenty-seven operative animals and 60 normal animals were used. No difference was found in the relative difficulty of the mazes for animals with slight and those with extensive lesions, but a degree of retardation was evident and was best described as an exponential function of the extent of lesion. H. G. Swann (Chicago) described an apparatus for olfactory discrimination in rats, and discussed the reaction of rats to a variety of odors under visual, tactile, and auditory handicaps. While it was difficult to extirpate the olfactory lobes completely, of the 23 operative animals only 5 failed to show discrimination. There was a complete loss of habit, however, followed by slow relearning, indicating that olfactory cues were significant. The last paper was contributed by E. S. Girden (Illinois) on cannibalism in dogs. In five of eight tested animals aversion for raw dog flesh was shown but was not permanent. When dog meat was cooked no animal showed aversion to it.

The sixth section of papers running simultaneously with the above was presided over by A. R. Gilliland (Northwestern). It opened with a paper by E. B. Greene (Michigan), who tried to secure measures on relative human efficiency which should compare individuals in terms of method, motive, and fatigue. The results from various age groups showed a marked difference between easy and hard tasks which were explained through subjective reports that indicated very little change in fatigue by numerous changes in motive and method of application. The lowest quarter of students changed more and showed more improvement than the highest quarter on the easier tasks. But the reverse was true for the harder tasks. M. F. Meyer (Missouri) next read a paper on "Oberlin Grades Its Students in the Registrar's Office." He traced the various stages of growth in the manner of marking students but deplored the unfortunate retrogression recently in evidence at Oberlin whereby for purposes of transferring credits to other colleges a ranking order has been converted into percentages. D. Beise (Ohio) discussed the reliability and validity of certain physical tests compared with the reliability and validity of mental tests. She statistically examined 4 tests usually given in the gymnasium and disclosed the fact that these tests lacked the criteria of validity possessed by such mental tests as the Stanford revision of the Binet series. She concluded that tests which depend largely upon the cerebro-spinal nervous system for their responses show a higher measure of reliability than do those whose responses are governed by the autonomic nervous system. In a paper on the effect of pre-school attendance upon the I.Q., B. E. Wellman (Iowa) showed that preschool attendance increased the intelligence test scores. There were greater gains over two years of attendance than over one year; and over three years than over two years. Gains took place during the winter months while they were in school but not during the summer months when they were on vacation. The final paper, by F. P. Robinson (Iowa), on the rôle of eye-movement habits in determining reading efficiency showed that an increase in the efficiency of eye-movements also tended to increase the reading ability of poor readers. The groups were matched for intelligence, reading ability, class, and sex, which led to the conclusion that the resultant gains could only be attributed to the factors involved in the training technique.

Under the chairmanship of C. A. Ruckmick (Iowa) a 2-hr. session was arranged for reports from the various laboratories. A new procedure called for reports from the research workers themselves and under a limited schedule for presentation and discussion. Reports were ready by J. Pessin (Wisconsin), "Effects of social stimulation on human maze learning;" R. C. Davis (Indiana), "An investigation of the loss and acquisition of emotional reactions;" P. T. Young (Illinois), "Changes in food preferences of the white rat;" H. H. Remmers (Purdue), "Affective selection and liberalizing influence of college courses;" H. Woefle (Illinois), "Attitude toward intelligence tests;" D. L. Bidwell (Ohio State), "An experimental study of the learning and retention of paired associates;" R. K. Compton (Illinois), "An experimental and analytical study of organic set;" Z. Peel (Arkansas), "Similarity of form of interpolated learning and retroactive inhibition;" T. W. Forbes (N.Y. Psychiat. Inst.), "A study of sleeping postures of catatonics;" R. W. Husband (Wisconsin), "Comparative motor abilities of athletes and non-athletes;" D. P. Boder (Lewis Institute), "Some electronic devices;" W. F. Book (Indiana), "Learning studies;" G. Finch (Illinois), "The comparative auditory acuity of dogs and human subjects." At the close Dr. C. E. Spearman was invited to discuss his coöperative research, which he is conducting this year at the George Peabody College for Teachers in an effort to bring together under a simplified mathematical formulation various types of mental measurements. He is seeking to prepare the way toward a mutual reconciliation of controversial points of view. M. F. Meyer also demonstrated a mechanical device which proved that a flexible artificial lens could change its focal length.

University of Iowa CHRISTIAN A. RUCKMICK

Anton Abram Grünbaum: 1885-1932

Grünbaum turned definitely to the study of psychology as his chosen field while he was a student at Würzburg in 1906. For three semesters he worked under Külpe and presented as his dissertation for the doctor's degree a detailed investigation of the contents and forms of abstraction.[1] He came to Würzburg at the time when Külpe's interest in the experimental study of higher mental processes was at its height, and was able to profit from association with an unusual group of men and women devoted to the working out of their director's ideas. In his own problem, for example, Grünbaum had as observers, among others, Külpe himself, Dürr and his wife, Miss Martin, Michotte, Bühler, Katz, Pfeiffer, Segal, and Spearman. A few years before, Külpe had made his famous report at Giessen on preliminary results obtained in an abstraction-experiment. Mittenzwey, under Wundt, followed soon after with a long critical, historical, and experimental study of the same topic in which the methods and conclusions of Külpe were treated with scant respect. Grünbaum's monograph served both as a reply to Mittenzwey and as a completion of Külpe's own results, with the addition of evidence regarding the abstraction and perception of items in a specially determined relationship. The work appeared in the same number with Bühler's reply to Wundt's attack on the introspective technique used at Würzburg.

The influence of Würzburg is apparent in nearly all of Grünbaum's publications during the next twelve years. Only one or two articles do not fall within the field of the higher mental processes or of sensory and perceptual phenomena. In 1916 he went from Bonn, where he had been assistant in the psychological laboratory under Störring, to Amsterdam. In 1922 he was made director of the psychological laboratory at Amsterdam, and was also closely connected with the work in the psychiatric and neurological clinic in the same city. This latter activity was beginning to make itself evident in his more recent publications.[2] Studies in thought, memory, abstraction, and determination gave place to work on reaction-times, psychogalvanic reflexes and their diagnostic value, aphasia' various aspects of motor disturbances, child psychology, and, far removed from the influence of Würzburg, the behavior of spiders. Grünbaum wrote chiefly in German, although several articles appeared in French and in English. His untimely death occurred in Amsterdam on the tenth of January, 1932.

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[1] Über die Abstraktion der Gleichheit: ein Beitrag zur Psychologie der Relation, *Arch. f. d. ges. Psychol.*, 12, 1908, 340-478.

[2] Publications later than those listed in *The Psychological Register* follow: Experimentelles zur Lehre vom systematischen Zusammenhang der motorischen Erscheinungen, *Zsch. f. d. ges. Neur. u. Psychiat.*, 120, 1929, 286-340; (with Q. Snapper) Lactic acid eliminated in urine and sweat during various games, *Biochem. Zsch.*, 208, 1929, 212-220; The pointing position of the hand as a pathological and primitive reflex, *Brain*, 53, 1930, 267-277; (with L. Bouman) Über motorische Momente der Agraphie: Schreibstörungen und Zwangsgreifen, *Monatssch. f. Psychiat. u. Neur.*, 77, 1930, 223-260; Aphasie und Motorik, *Zsch. f. d. ges. Neur. u. Psychiat.*, 130, 1931, 385-413.

Harvard University CARROLL C. PRATT

Leonard Thompson Troland: 1889-1932

Leonard Thompson Troland, whose death occurred May 27th, 1932, was one of those rare individuals who can follow two distinct careers at the same time and be successful in both. He studied psychology at Harvard University under Münsterberg, Holt, and Langfeld from 1912 to 1915, obtained his doctorate from Harvard in the latter year with a thesis on visual adaptation, and carried on his work in vision during 1915-16 as Sheldon Fellow at the Nela Research Laboratory in East Cleveland. In September, 1916, he was appointed Instructor in Psychology at Harvard, and he continued to teach psychology there until the time of his death. Besides a host of articles on psychological problems—mainly problems of vision—he published three major works in psychology: *The Mystery of Mind*, a rather popular introductory text (1926); *The Fundamentals of Human Motivation* (1928); and *The Principles of Psychophysiology*, a general treatise in four volumes of which only the first three had been published at the time of his death (1929-32).

Parallel to this work in psychology, however, ran a well-filled career in theoretical and applied physics. As an undergraduate at the Massachusetts Institute of Technology, from which he received the degree of S.B. in 1912, Troland took a number of courses in optics and in theoretical physics under D. F. Comstock. In 1917, he collaborated with Comstock in publishing *The Nature of Matter and Electricity*, an introductory exposition of the basic concepts of modern physics. The following year he became research engineer for Kalmus, Comstock and Wescott, Inc., and began with Comstock, E. A. Wever and J. A. Ball the collaboration which was to make him co-inventor with these men of the Technicolor Process for the production of colored motion pictures. His work upon the Technicolor Process continued with Kalmus, Comstock and Wescott, Inc., until 1926, and thereafter, until the time of his death, with Technicolor, Inc. A measure of his contributi to optics is afforded by his election, in 1922, to the presidency of the Optical Society of America.

It is undoubtedly his interest in physics which was the most important factor in shaping Troland's psychology. True, philosophy played an important part in his life. As a graduate student at Harvard he studied Greek philosophy with J. H. Woods, ethics with R. B. Perry, epistemology with Perry and Bertrand Russell, logic with Josiah Royce, and philosophy of nature with E. B. Holt. He held a definite philosophical position, a form of psychical monism tempered by acceptance of physico-chemical explanations at the scientific level, and contributed a number of articles on philosophical issues.[1] Philosophy, however, did not affect his psychology except as it provided a setting. In treating a psychological problem he was indeed wont to emphasize its philosophical connections in the introduction and in the conclusion, but his main concern was always to formulate exact functional relationships between quantitative variables. The ideal towards which he was ever striving was a psychology patterned after modern physics, a psychology made up of mathematical equations.

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[1] The chemical origin and regulation of life, *Monist*, 24, 1914, 92-133; Philosophy and the world's peace, *J. Philos.*, 13, 1916, 421-437; Paraphysical monism, *Philos. Rev.*, 27, 1918, 39-62; The significance of psychical monism, *Psychol. Rev.*, 29, 1922, 201-211; Psychophysics, the key to physics and metaphysics, *J. Wash. Acad. Sci.*, 12, 1922, 141-162.

This ideal accounts largely for his many excellent contributions to visual psychophysics. In the first place, it determined him to concentrate his experimental work in vision, the sense-department in which control and measurement of the stimulus has unquestionably reached the greatest development. In the second place, it determined him to select within his chosen field problems which were not merely soluble, but whose solution could be stated in the unambiguous language of mathematics. His doctoral thesis, for instance, dealt with visual adaptation, in which time-exposure could be related mathematically to wave-length, intensity and area of stimulation.[2] Again, his subsequent experimental contributions include work on the relation of the duration of a negative after-image to that of the exposure producing it,[3] of flicker-photometer frequency to the color and intensity of the alternated lights,[4] and of the differential threshold for brightness to color differences between compared fields,[5]—all of them problems leading to mathematical functions. Indeed, results not subject to mathematical description were usually considered by Troland to be merely preliminary in nature.

It is also this ideal which accounts for the extreme definiteness and clarity of his psychophysiological theories. As early as 1914—a year before receiving his doctorate—Troland voiced his faith in what he called the "method of mathematical hypothesis,"[7] the method by which processes are described in terms of equations developed from a limited number of simple mathematical postulates. In the previous year he had already applied this method in presenting a physico-chemical hypothesis to explain visual response.[8] He applied it again, in 1914, in formulating a general mathematical theory of adaptation[9] and in 1918, in giving a mathematical interpretation of his data on the heterochromatic brilliance threshold.[10] It is true that his theory of audition,[11] so well in accord with the results of Wever and Bray, can hardly be termed quantitative. Even in this case, however the "method of mathematical hypothesis" shows its influence in the choice of fundamental relationships readily subject to quantification. The result of this faith in mathematical formulation is that Troland's sensory hypotheses conform well to the standards of good scientific theorizing: they are devoid of unintentional ambiguity; they account accurately—not just approximately—for available facts; and they frequently imply—not merely suggest—further observable, but as yet unobserved, facts and are thus capable of real verification.

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[2] The thesis is *Studies in Visual Equilibria, Harvard University*, 1915. Cf. The colors produced by equilibrium photopic adaptation, *J. Exper. Psychol.*, 4, 1921, 344-390.

[3] The laws of visual minuthesis: the threshold pre-exposure time and the equilibrium time for a projected negative after-image, *J. Franklin Inst.*, 181, 1916, 579-181.

[4] Notes on flicker photometry: Flicker-photometer frequency as a function of the color of the standard, and of the measured, light, *ibid.*, 181, 1916, 853-855; Flicker-photometer frequency as a function of light intensity, *ibid.*, 182, 1916, 261-263.

[5] The heterochromatic brightness discrimination threshold, *ibid.*, 182, 1916, 112-114; The heterochromatic differential threshold for brightness, I. Experimental, *Psychol. Rev.*, 25, 1918, 305-329; II. Theoretical, *ibid.*, 359-377.

[6] The absence of the Purkinje phenomena in the fovea, *J. Franklin Inst.*. 182, 1916, 111f.; The reversal of a faded negative after-image by brightening the stimulus field, *ibid.*, 529 f.; Preliminary note: the influence of changes of illumination upon after-images, this JOURNAL, 28, 1917, 497-503.

[7] Adaptation and the chemical theory of sensory response, this JOURNAL, 25, 1914, 525.

[8] A definite physico-chemical hypothesis to explain visual response, *Amer. J. Physiol.*, 32, 1913, 8-40.

Troland's application of the "method of mathematical hypothesis" was not limited to sensory processes. In 1920, he propounded in mathematical terms a "system for explaining affective phenomena"[12] which was to become the nucleus of his subsequent volume on motivation.[13] On the surface this theory appears to have the same virtues as his sensory hypotheses. On the surface only, however. Consider the basic equation $a = k\,(dc/dt)$, in which a represents degree of affection, c the average conductivity of active cortical synapses, t time and k a constant. Does this relationship have any definite empirical implications? It surely would have many if only c were subject to measurement, but such is not the case—nor is likely to be the case for many decades. The "method of mathematical hypothesis," to be fruitful, requires the use of well-defined variables. In Troland's theory of affection it is of little more value than it was in the psychology of Herbart.

Familiarity with mathematical relationships is surely no warrant of expository ability Yet in Troland there did seem to be a definite causal relationship between these two factors. Not only was he a clear writer and an erudite one, but he had an extraordinary knack of classifying facts under functional relationships in such a way that they constituted a logically and psychologically organized whole. It is this ability which distinguished his many surveys of the literature of vision, particularly *The Present Status of Visual Science*.[14] It is likewise this ability which made his *Principles of Psychophysiology* a truly great contribution, comparable with the like work of Wundt, of Ebbinghaus, and of Ladd and Woodworth. The three volumes published to date include many interesting and novel features, such as the definition of perception as the relation of consciousness to the stimulus object and that of sensation as the relation of consciousness to the afferent arc. Their outstanding merit, however, is that they provide for the entire field of modern psychophysics and psychophysiology a source which, though adequate

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[9] Adaptation and the chemical theory of sensory response, this JOURNAL, 25, 1914, 500-527.

[10] The heterochromatic differential threshold for brightness, *op. cit.*, note 5 *supra*.

[11] The psychophysiology of auditory qualities and attributes, *J. Gen. Psychol.*, 2, 1929, 28-58.

[12] A system for explaining affective phenomena, *J. Abn. Psychol.*, 14, 1920, 376-387.

[13] *The Fundamentals of Human Motivation*, 1928.

[14] The present status of visual science, *Bull. Nat. Res. Council*, 5, 1922, 1-120 (no. 27).

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Minnesota Mechanical Ability Tests. By D. G. PATERSON and R. M. ELLIOTT, directors of the investigation; L. D. ANDERSON, chief investigator; H. A. TOOPS, statistical consultant; and EDNA HEIDBREDER, editor of the report. Foreword by R. M. Yerkes. Minneapolis, University of Minnesota Press, 1930. Pp. xxii, 586.

This is the report of an investigation subsidized by the Committee on Scientific Problems of Human Migration of the National Research Council. Yerkes, who was chairman of this Committee in its early history, traces in the Foreword the origin and purpose of the research. The work was carried out in the University of Minnesota with the aid of numerous assistants. The authors say that the members of the subsidizing committee "were generous to the point of indulgence with their encouragement and financial backing (p. ix), so it is presumable that a fair opportunity existed to try out the concept of 'mechanical ability' (or abilities). The term is used in this research "to refer to whatever capacities and abilities are necessary for certain kinds of work—specifically, work that involves the manipulation of tools, the operation of machinery, and the planning and execution of pieces of work which involve these and similar activities" (p. 5). The practical purpose was to construct a scale of tests of mechanical ability in the sense indicated in the quotation. Providing for proper motivation is no small part of such test construction.

If "maximum motivation" is obtained in the tests, can one from these data predict direction and degree of success under conditions of motivation in actual mechanical work? It was assumed that a battery of tests would be better for this purpose than a single test, and that the tests constituting this battery must be reliable and correlate as highly with the criteria as possible and only little or not at all among themselves. To secure these tests and criteria required considerable preliminary experimentation and a large number of subjects for different tests.

In later experimentation it was found that several tests of low reliability could be made consistent in their measurements merely by a suitable lengthening of the tests. To ascertain effects of such lengthening, three different applications of the preliminary tests were given. The subjects in this preliminary experiment were 217 junior high school boys, each being tested 11 hours on group and individual tests, including a retest series after a seven-day interval. The boys' success in five different shop courses was determined on the basis of their teachers' judgments by means of the rank method. Rank scores in the several courses were all converted into σ scores and averaged to obtain "the combined shop criterion." This criterion was found to give positive but low correlations with all 29 of the tests used (including intelligence tests); only two of these correlations were above 0.40 (Link's Spatial Relations, longer board, time being 0.44 and the Paper Form Board, 0.43). Seventeen of these 29 correlations were greater than four times the P.E., or than 0.15. By the Toops multiple-ratio technique a battery of seven tests was constructed, which gave a correlation of 0.60 ± .03 with the criterion, of 0.07 ± .05 with intelligence scores, and low inter-correlations. Reliabilities obtained by means of a retest 7 days later were not satisfactory but it was found that the defect could be largely removed by a proper lengthening of the tests.

The work thus far done was preliminary. During the academic year 1924-25 the experiment proper was carried out on a new set of 150 boys then entering the Jordan Junior High School. The following improved tests were used, a great number of the earlier tests having been discarded: The Minnesota Assembly tests (after Stenquist); the Minnesota Spatial Relations tests (after Link); the Minnesota Paper Form Board tests (after Army Beta); a card sorting, a packing blocks, and a nine-hole steadiness test; and the Stenquist Mechanical Aptitude Picture Tests, I and II. Age, Otis S-A Higher Examination scores, academic success, experience in mechanical work, interests, motor ability (agility and gymnasium success), anthropometric measures (height, weight, vital capacity), social and economic status, and home influence—all determined by as objective means as possible—were studied in their relations to success in the tests. The third and fourth of these factors were determined by means of questionaries and interest-analysis blanks, respectively. The Otis test was preferred to Alpha "because of its simpler timing and scoring methods," and because in the preliminary experiments the inter-correlation of these tests "was very high" (p. 113). From Table 88 (pp. 432-433) this correlation is found to be 0.74 ± .02, which gives a predictability of the one score when the other is known of only 0.33. The reviewer has for years obtained considerably better predictability of success in psychology examinations with Alpha than with Otis scores, and he doubts the wisdom of the use of the latter instead of a revision of the former test, despite the greater convenience of the Otis test.

The criterion of mechanical ability—the standard against which all the tests are rated as to validity—was also much improved in the experiment proper. A long chapter (Chapter 9) of 58 pages describes fully the construction and nature of this somewhat complex criterion, giving rating scales and other devices for measuring the degree of excellence of different kinds of shop work in the schools, test blanks on mechanical information and on training. These rating scales are in terms of carefully ranked samples of completed work, reproduced in half-tone. Several criteria of different activities—each usable separately for its particular kind of shop work—were thus produced, and since there was no single rating or ultimate criterion with respect to which these could be severally weighted, it was necessary to convert the scores into standard units and to combine them by weighting equally the different units (in the form of weighted averages), except that the quality-quantity criterion was the combined scores of the quality and the amount of all shop work done, not divided by the number of projects completed (p. 202).

In the various stages of the combination of different and more special criteria, the construction of the final criterion of mechanical ability in general seems to have been based on the assumption that all the different tests contributed alike to measuring some sort of general, unique mechanical ability trait. It can hardly be contended, however, that any such trait has been established. A battery of tests of some more or less vague mechanical ability has been constructed, and the scores on different elements so combined as to correlate more highly with success in shop work in the schools than with intelligence tests. That seems to be about all that can be inferred from the work as to the real nature of mechanical ability. It may be noted, moreover, that in converting raw scores into scores for equal weighting, this purpose to weight equally is hardly fulfilled unless the σ of each set of test scores is corrected for unreliability; since the size of the σ is a function of its unreliability. The error here, however, is not great, since each test was made as reliable as practicable and low reliability of any test would have a tendency to underweight it in the combined score. In the last part of the book (pp. 309-586) are to be found detailed directions for administering and scoring the battery of tests finally developed; statistical methods; correlation tables; percentile norms, and other valuable data, as well as original scores both from the preliminary experimentation and the final experiment proper; a classified bibliography of 17 pages; and an excellent index, covering 25 pages.

The work on establishing criteria and percentile norms of mechanical abilities, on preparing objective tests and scales, on organizing the rating and the test-scales and results for practical use is of considerable value; and the tables of original data will enable other workers to check and evaluate the methods and interpretations. In view of the excellent coöperation under most favorable school conditions and of the technical assistants employed, the work done was so stupendous and excellent in details that future mechanical ability determinations will probably be dependent to a considerable extent on this publication for many years. It is therefore of practical importance that all the items of the assembly tests may be available to testers for some time, and that their uses be clear. Item 10 of Box A and items 8 and 10 of Box B (pp. 320 and 323) are marked "not included in the present series." Were scores on these tests included in the data on which norms were established? This isn't made clear.

One is struck by the enormous number of tests that had to be dropped out because of small value. Would a more careful survey of past work not have saved much time, or was this extent of "trial-and error" essential? The practical-minded reader would doubtless like to know more about the time for administration and the scoring time of the tests. Much of the scoring seems to be of a laborious nature. Certainly reliability coefficients listed as obtained from the combined estimates of ten judges (Table 20) are impracticable in actual use, and the coefficients obtained by the up-step Spearman-Brown formula by the odds-evens device will be found too high in actual practice, where the perfectly controlled conditions assumed by the formula cannot be realized. No data yet available indicate the relationships of success in the tests and success in practical mechanical work outside the schools.

It must be said, finally, that the work under review is a conspicuous example of elaborate and carefully executed work of high quality, carried out coöperatively by many workers of both scientific and practical interests. J. P.

Genetic Studies of Genius, Vol. III. The Promise of Youth. By BARBARA STODDARD BURKS, DOROTHA WILLIAMS JENSEN, and LEWIS M. TERMAN. Stanford University, Stanford University Press, 1930. Pp. xiv, 508.

This volume consists of the follow-up studies of a thousand gifted children first tested in 1921-22. The authors sought answers to two fundamental questions concerning the development of gifted children: (1) "How well does the mental ability of these gifted children hold up on the average? (2) How many of the children later show radical changes in intelligence rating, either positive or negative?" Methods and results are presented in Part I. The tests used comprise 5 standard intelligence tests, 5 achievement tests (besides school marks), 6 personality tests, including tests of emotional stability, femininity-masculinity, interest, etc., and 9 information blanks sent to parents and schools. The results of retesting in 1927-28 show a drop in IQ for both sexes of 9 points, 3 points for boys and 13 points for girls. Some drop in IQ is to be expected statistically as due to unreliability of measurement. The children who were first tested at an early age do not show a greater drop in IQ than those who were first tested at an older age.

No evidence was discovered to indicate that such factors as environment, personality, health, race and the nature of the Stanford-Binet test were significant in causing marked IQ changes in gifted children. No significant differences between early and late testing were apparent with respect to interest in school, achievement in school subjects, regularity in school attendance, personality traits (*e.g.* fair-mindedness and masculinity-femininity), and social intelligence.

While no direct comparison could be made with respect to the general health of gifted children between the follow-up group and the 1921-22 group, the reports obtained from information blanks tend to disprove the popular belief that gifted children are apt to be deficient in health and physical vigor. The reports from home and school show that from 77-90% of the gifted boys and girls have good general health and that from 1-5% have poor general health. However, the number of girls who show symptoms of general weakness is nearly twice as great in 1927-28 as in 1921-22. In the follow-up group the school blanks report 15% of the gifted boys and 18% of the gifted girls as showing symptoms of general weakness. It was found that gifted children sleep somewhat less daily in 1927-28 than in 1921-22, but they still sleep more than untested children.

All but two of the 1921-22 high school group who coöperated in the follow-up have graduated from high school, and more than 90% of both boys and girls have gone, or plan to go, to college. Approximately one-half of those graduating from college have taken postgraduate work.

Most of the boys and two-thirds of the girls who have graduated from college have decided upon their life vocations. Gifted boys who have graduated from college tend to choose the professions, while non-graduates in the gifted group are more likely to choose a business vocation.

Part II deals with case studies of youthful geniuses, gifted leaders, musicians and musical composers, artists and devotees of science. These individual studies reveal certain inconsistencies in development. A few cases of marked or moderate decrease in IQ and achievement are reported, but a large majority of the individual cases studied show consistent development during the period between 1921-22 and 1927-28.

Part III is a study of literary juvenilia, the object of which was "(1) to derive a scale with steps high enough to measure the best writings of the most talented children, (2) to compare the productions of California gifted children with the juvenilia of eminent writers, and (3) to study the environment and early development of those who have evidenced precocious literary ability." Fourteen California gifted children with IQ ranging from 140 to 188 (ages ranging from 6 yr. 9 mo. to 13 yr. 10 mo.) were found to possess outstanding literary ability. The literary productions of these children compare favorably with the juvenile productions of 28 English and American men of letters who were rated as having IQ of 140 or over.

Part IV gives a summary, and an appendix presents the protocols which were used in securing information from parents and schools.

The chief value of the follow-up studies is suggested by the title of the present volume, "The Promise of Youth." Although the reliability of some of the tests is still in doubt, the combined results point unmistakably to the conclusion that "the gifted individual whom intelligence tests may discover in childhood" will become "the extraordinary genius who achieves the highest eminence."

University of Montana FRANKLIN O. SMITH

The Treatment of Behavior Disorders Following Encephalitis: An Experiment in Re-Education. By E. D. BOND and K. E. APPEL. New York, The Commonwealth Fund, Division of Publications, 1931. Pp. 63.

In this small book, an account is given of an experiment in the treatment of the disorders of behavior which follow encephalitis. The first chapter contains a clear and non-technical account of the disease of encephalitis. Case histories are then presented which graphically illustrate the unfavorable changes in behavior which often develop as an aftermath of the disease. Previously normal, well-adjusted children after the disease frequently show marked behavior disorders and social retrogression.

The experiment concerning which the book is written, deals with the organization of a hospital-school under the direction of the chief psychiatrist of the Department of Mental and Nervous Diseases of the Pennsylvania Hospital. The work in the school consists in an ingenious effort to reëducate the total personalities of children who are in its charge. A balanced program of sleep, work, and directed play has been organized, which occupies the whole twenty-four hours of each child. This program, and its sympathetic and personal application, developed in the light of individual differences, may well be studied by anyone who is interested in modern methods of changing atypical behavior in children. The results of the experiment show that, in a substantial number of cases, the treatment has led to real improvement.

Approximately one page of the book is given to a section headed "Psychological Theories." Here the rather startling statement is made that, "In treating the children in the hospital school, if a theory appeared to throw light on a problem, that theory served as the basis of treatment, whether it implied the behaviorism of Watson, the inferiority feelings of Adler, the 'needs' of Thomas, the type-reactions of Jung, or the psychological processes emphasized by Freud." Is it too much to hope that the time may come when psychiatrists may recognize psychology as a scientific and useful body of fact and not as a welter of conflicting views in regard to personality?

Save for the criticism suggested above, the book seems well calculated to supply a significant program for those who are concerned in dealing with the distressing after-effects of encephalitis.

Brown University LEONARD CARMICHAEL

Hypnotic Poetry. By Edward D. Snyder. Foreword by James H. Leuba. Philadelphia, University of Pennsylvania Press, 1930. Pp. xi, 162.

The author, an associate professor of English at Haverford College, suggests that certain poems are largely "spellweaving" poems which influence the auditor in a fashion different from the influence of more "intellectualist" poems. The "spellweaving" poems are capable of stimulating the listeners to various states, from highly emotional to mildly trance-like, and this without the poems having a "rational" appeal. It is suggested that there is a close parallelism between the results of such poems and the phenomena of hypnosis, including post-hypnotic suggestion; and between the factors producing both the "spell" and the hypnosis. Among these factors are: monotonous rhythm of auditory stimuli; freedom from abruptness; vagueness of imagery; and slight fatigue due to minor complexities. The relationship between these states and phenomena of religious revivals, due largely to "hypnotic hymns," is pointed out. Certain implications bearing on literary criticism are mentioned.

The book is not a text on poetics or on hypnotism, but it is intelligently written, and suggests certain applications of psychological knowledge which are worth considering by those interested in the phenomena of hypnotism or in poetic literature.

University of Pittsburgh J. H. McFadden

Foundations for Human Engineering. By C. R. Gow. New York, Macmillan Co., 1930. Pp. xiii, 226.

This book presents briefs of lectures constituting a course in "Humanics" which is elected by seniors at the Massachusetts Institute of Technology. The general purpose seems to be that of formulating an applied science of self or character development. One obvious weakness of "Humanics," as presented, is that it seems to have much less basis in psychology than does mechanics in the science of physics. The foundation of "Humanics" is only the superior common-sense of the author, who is an experienced and successful business man with a frankly commercial point of view.

The text is essentially a discussion of the virtues from the point of view of a paternalistic capitalist. It contains a long list of *do's* and *dont's* for the guidance of the young man who desires, above all things else, to be "successful" under the sacrosanct sway of "business." Honesty and loyalty are among the chief virtues. Honesty is preferred, apparently because it is the best policy. Loyalty is likewise indispensable. "As long as one is supposed to serve an employer and accepts pay for that service, let him be *for* that employer in every honorable way,"etc. Other virtues are tact, patience, courage, cheerfulness and friendliness. On the other hand "one of the chief obstacles to progress and prosperity is anger." Other vices are conceit, egotism, envy and criticism. "By all means do not criticize others. Praise them if you can do so." Many striking illustrations of the advantages and disadvantages of these traits in business are given. Practice to develop traits, as specific entities, is urged. "Training in observation should begin early." Abstractions are interpreted in terms of other abstractions.

Credit must be given the author for his sensible and unspoiled, albeit rather naïve, attack on the problem of character development in a technical school.

Some excellent advice is given in regard to the discovery and cure of specific defects of personality, such as peculiarities of voice, gesture, and expression, but the book does not have a scientific point of view or background. Furthermore the accepted standards of good character are evidently specified by the same people who write the specifications for the other products of engineering, such as skyscrapers and automobiles. Too much standpat conformity to a fixed moral code may not be a good thing in the long run, even in business.

University of Colorado THOS. H. HOWELLS

Voice and Personality. By T. H. PEAR. London, Chapman and Hall, Ltd., 1931. Pp 247.

This book touches upon problems of the voice as an expression of personality. The first half is devoted to a discussion of 'dialect' and local differences in speech or accent, and the last half to a description of experiments carried on by the author and his assistants.

The experiments suggest many problems of interest to psychologists. Examples of these are as follows: How is the judgment concerning any particular attribute of the voice formed in the "wireless" listener's mind? How would a listener who does not understand our language be affected by these attributes? What effect will broadcasting have upon standardizing the speech of different localities and of the different professions? Wherein may psychological analysis aid in discovering and remedying the defects in the voice that fails to "get across." The volume on the whole is an excellent piece of popularizing work, and is extremely valuable as suggestive of fields for further investigation.

University of South Carolina W. F. SMITH

Psychology of Early Childhood up to the Sixth Year. By WILLIAM STERN. Translated by Anna Barwell from the 6th German edition. New York, Henry Holt & Co., 1930. Pp. 612, 11 pl.

The new English edition of this classic differs little from the earlier translation of 1924 (based on the enlarged 3rd German edition of 1923). The bibliography, however, is much extended, the text contains a number of references to recent research on Russian children, and the four plates of Kurt Lewin's film-photos of expression movements in children (which appeared first in the fourth German edition) have been appended to the illustrations.

The treatment throughout leans heavily on the psychographic studies of the Sterns, Scupins, Bühlers, Katzes and others; centering naturally about the socialized activities of drawing, speech, and the interpretation of pictures, with the investigation of which Stern and his wife have been so long identified. Ever since the third edition, however, the text has striven to take account of and to criticize the work and theories of Bühler, Koffka, Freud, Montessori, and Piaget as well.

The principle of convergence adopted by the author (*i.e.* the interaction of innate aptitudes and experience) absolves him from the painful necessity of disentangling hereditary and environmental influences. While the recurrent stress on the 'unity of personality' expressly implicates the total psychophysical unit, the usual sections on biology and neurology are conspicuous by their absence. References to the Watson-Blanton-Sherman type of observation, or to Russian 'reflexology' are rare. Much illuminating material, however, bearing both on the development of perception and on the emotional life of the child is made available in these pages. Aesthetic perception (in music, rhythm, and pictorial representation) and phantasy or imagination, are especially stressed. While the generalizations are based largely on children from social strata above the average, the work forms an invaluable supplement to the usual American text.

Cornell University ELSIE MURRAY

Child Psychology. By MARGARET WOOSTER CURTI. New York, Longmans, Green & Co., 1930. Pp. ix, 527.

This is a student's text, the fifth in the series edited by H. A. Carr. The language is for the most part non-technical, on the level of commonsense rather than of science (notably in the use of such words as 'idea,' 'thinking'). Hence the layman no less than the student will find it readable. Psychologists in general may also derive profit from its pages, since it offers a fairly impartial survey or digest of current and recent literature on the topics treated.

Writing from the standpoint of Carr's 'functionalism,' the author strives to avoid dualistic snags, and the extremes of a one-sided subjectivism or behaviorism. Mind is defined as the capacity for 'unified and effective adaptation to environment,' and space is assigned both to overt and to implicit (conscious or neural) response. The first hundred pages are largely biological, dealing with heredity, physical and mental growth, and native responses. The nervous system gets scant mention. The section on 'perceptual-motor learning' gives a detailed account of Russian work on children, including Krasnogorski's use of conditioning technique in the early diagnosis of individual types, as the author herself saw it demonstrated in Leningrad.

The term 'conditioning' is used consistently throughout the text, carrying often precisely the same connotation as the 'association'' of the older schools. Though rejecting some of Watson's concepts, the author fails to build up for us a picture of the child as an individual, as does Stern, presenting us rather with a human mechanism or robot. A mechanism, moreover, with a bent toward going awry; for although her theme is avowedly the normal, Mrs. Curti's slant, in consequence perhaps of her clinical experience, is more often toward the abnormal. In her suggestive chapter on motivation, though critical of the Freudian stress on sex, she retains the stereotyped old rubrics—'compensation,' 'rationalization,' 'conflict.' Not only do these affix a pathological stamp to perfectly normal trends, they presuppose a view of human nature only a step above the Calvinistic or Hebraic. 'Conflict,' especially, in emphasizing the antiquated 'either-or' dilemma of contradictory opposites, ignores the complexities of modern community life. Even in childhood, surely, ethical problems center less upon the collision of code and inclination than on the difficult harmonization of several slightly compatible aims—vocational, recreational, and domestic.

The discussion of the origin, growth, and organization of meaning (defined not as an abstract entity, but as the totality of potential responses to a given situation) occupies another section, one of the best. While paying her repects to Carr and Dewey, the author follows in the main Piaget's illuminating use of *syncretism, juxtaposition,* and *egocentrism,* intracing the development of child thought. *Gestalt* nterpretation—except when pressed to extremes—is shown to be compatible with current experimental findings; but reasoning is handled as essentially a trial-and-error process, a mental fumbling. The importance of *Einstellung* or set receives as little recognition as it does at the hands of configurationist or objectivist.

In the chapter on play, the author summarizes a number of recent American studies—mostly statistical—throwing them into perspective and relating them to standard theories. Finally, a hundred pages are devoted to discussion of the relative weight of defective intelligence, emotional instability, and social milieu in influencing delinquency and anti-social conduct. Burt, Healy, Slawson, Hartshorne and May are quoted. The book concludes with a short but sane chapter on the organic and environmental factors conditioning personality and traits (the latter defined as 'functional relationships distinguishable in a total of adaptive activity'). There are numerous footnote references and an index, but no adequately organized bibliography. A 'Work-Book' for students is said to be in preparation.

Cornell University ELSIE MURRAY

L'éducation fonctionnelle. By ÉD. CLAPARÈDE. Neuchatel, Delachaux & Nestlé, 1931. Pp. 267.

A ninety-seven page "Introduction" explains the theory and the sources of functional psychology traced historically from Locke and Herbart to the present day. Following this introductory material is a discussion of 10 laws of conduct to be employed in utilizing the child's activity, with special emphasis upon the laws of need and interest. Among the other laws there is some discursive overlapping. The next one hundred and forty pages include articles, many of which had already been published, on various functional aspects of education. The remaining twenty-four pages are devoted to a "Conclusion" answering the classic question, "Is education a preparation for life or life itself?" The author's final statement may be translated as follows: "In order to prepare for life education must be life. If education aims to be a preparation for life without being life itself it does not prepare for life."

Functional education is defined as that which considers the needs of the child and his interest in accomplishing his purposes. By the term interest is meant the factor which brings about adjustment in governing the individual's reactions and adapting them to those needs. To provide proper conditions for creating appropriate needs for which the child will seek satisfaction is the business of the school.

The volume contains no index beyond an alphabetical list of 114 psychologists and noted educators whose ideas are utilized in establishing and corroborating the author's theory of functional education. Of this number he quotes about thirty American writers.

Because the book is not arranged in conventionally numbered chapters it gives the impression of being fragmentary. Every section, however, reënforces the central idea of need and interest. Emphasis upon important points is obtained by well-chosen introductory statements and summaries. The style is non-technical.

The book contains little that is new. It presents, however, a definite point of view and brings to the attention of the reader most of the best that has been said concerning the aims of that much discussed and much defined process which we call education. The reader is left convinced that consideration of the child's real needs is fundamental.

Rhode Island College of Education GRACE E. BIRD

Diary of a Child's Life. By VILHELM RASMUSSEN. Translated from the Danish by Margery Blanchard. London, Glydendal, 1931. Pp. viii, 187.

This is a series of observations, kept systematically from birth to the fifteenth year, on the author's second daughter. It is presumably identical with *Et Barns Dagbog* printed in Denmark in 1922. The entries (made by either parent) range between 50-100 items yearly. They are set down in chronological order, usually dated, under such commonsense captions as attentiveness, catching hold, fear, recognition, temperament, standing, walking, language, sentences, animism, counting, time, rage, lies, bashfulness, sympathy, and religion, and are designed to stimulate the keeping of similar diaries by others. There are numerous reproductions of the child's drawings at different ages; also of photographs illustrating varied facial expressions and postures. The little volume affords a vivid picture of a developing child personality, and forms an interesting supplement to Rasmussen's earlier, more systematic series.

Cornell University ELSIE MURRAY

La personalité de William James. By MAURICE LE BRETON. Paris, Hatchette, 1929. Pp. 383.

This is a detailed, well documented, account of the hereditary and early environmental factors contributing to the personality and achievements of the famous psychologist and philosopher in whom began the third generation of the Jameses in America. The life of the psychologist's grandfather, also William James, who came to America in 1789, at the age of 18 years, with very little money and a Latin grammar, and who began in Albany as a clerk in a store and rapidly worked himself up to be a leading business man and banker, amply providing for his eleven children, is briefly sketched. There is also a good account of the father, Henry James, who left the Princeton Theological Seminary with a growing antipathy to all ecclesiasticisms and turned his back on the ministry to develop his own type of Swedenborgian philosophy; of his solicitude for the education in America and Europe of his five children; of William's uncertainty and vacillation in choosing his life's work, his experiences in Europe, his important contact with Agassiz on the trip to South America where he decided definitely to give his life to philosophy, but where he also learned to have respect for objective facts in his generalizations.

Le Breton makes a careful analysis and evaluation of James' contributions, indicating how they reflect the life and personality of a keen and serious-minded individual who had taken philosophy and psychology seriously, and had found it necessary to reconstruct them both in order to agree with the undeniable facts of his own varied and wide experiences. Recognizing James' originality and certain genuine contributions, Le Breton points out obstacles in the way of an objective evaluation and dangers of being misled by the persuasiveness of James' arguments and by the influence of his winning and resourceful personality. He credits James not only with a cosmopolitan view, but also with a keen sensitivity to, and a close contact with, the ups-and-downs of every-day life. He thinks that with the passing of years the specific contributions by James will become invisible but holds that his personal charm, seriousness of purpose, and great penetration have given direction and vitality to philosophy. James' philosophy was so personal and unstable that if he had lived longer he would never have been able to put a final touch to it but would have continued to get new insights requiring reconstructions and reinterpretations. J. P.

A Comparative Study of Those Who Accept as against Those Who Reject Religious Authority. By THOMAS H. HOWELLS. University of Iowa Studies: Studies in Character, Vol. II, No. 2. Iowa City, The University of Iowa, 1928. Pp. 80.

The object of this experiment was to compare religious radicals and conservatives on the basis of a number of quantitative psychological tests. From 542 University of Iowa students in elementary psychology, the author selected, by means of a self-rating point scale for measuring intensity of beliefs and attitudes (reliability 0.92), 51 as the most radical and 50 as the most conservative. There were six times as many Catholics in the conservative group (23% of all in this group) as in the radical and the few Unitarians present were all in the radical group (3% of this group). Other denominations were more evenly represented in both groups. These extreme groups were given for comparison approximately 30 different tests—sensory-motor, volitional (suggestibility and perseverance), or intellectual in character, including a high school content examination and university grades. What the subjects reported about themselves in the self-rating was also taken into consideration. Considering these latter self-reported data first, we may note that the mean of the radical group was 18.9 years, and of the conservatives it was 19.7 years. In both groups women were in the majority, but the proportion was greater in the conservatives. Allowances for slight sex differences in the tests did not alter appreciably the results. The conservatives were as a group somewhat less healthy and inclined toward pessimism; they came from somewhat larger families and had had more religious training and sympathetic relationships with parents and also more mystical experiences (conversions, sense of divine presence, etc.). These group-differences, however, were small and probably not very significant. The tests revealed only a few sensori-motor differences approaching reliability, the conservatives being poorer in weight discrimination and muscle coördination, but slightly more sensitive to (expected) electric shock than the radical. This latter difference seems from the data, however, to be due to greater suggestibility clearly revealed by reliable differences on several tests. The conservatives excelled slightly in accuracy of slow arm movements, but were less deliberate and more inclined to hasty action. None of the sensori-motor tests revealed a group difference more than 3.7 times its PE. The conservatives accepted more suggestions as to printed (impersonal questions) relating to pictures recently shown them than did the radicals. For example, to the question "Did you see the man (not present) holding the oar?" They were more apt to say "yes." While the group difference here was only 3.1 its PE, the conservatives were reliably more ready to accept suggestions as to increase of pain from electric shock (when there was no increase in the current) and less persistent against painful stimulation. Threats of punishment effected greater improvement in them. They were also reliably inferior to the radicals in tracing mazes, in the high school content examination, and in the intelligence tests. The group differences in ability to follow written directions, to make moral judgments of the multiple choice type (choosing the best and worst of four hypothetical responses in described situations), and in university grades all favored the radicals, but these differences were slightly below the point of statistical reliability.

It is certainly worth while to follow out more fully the causes of this intellectual and volitional inferiority of conservatives, and to determine the relative rôles of the lack of educational opportunities, the operation of various selective influences by religious groups for individuals of inferior ability and exclusion of superior individuals because of non-conformity, etc. The investigation indicates lines of approach to several important problems in social psychology and of intergroup and inter-national relations. There is a bibliography of 72 titles; and the self-rating tests, and additional laboratory test data are given in appendices.

J. P.

Abnormal Psychology: Its Concepts and Theories. By H. L. HOLLINGWORTH. New York, Ronald Press, 1930. Pp. 590.

This is in many ways a notable volume. In it Hollingworth discusses the facts of abnormal psychology as part of general psychological science, and not from any one partial standpoint, such as psychiatry, mental hygiene, clinical psychology, or psychoanalysis.

The book begins with a definition of the field of abnormal psychology and then turns, after a brief historical summary, to a consideration of the modern period in abnormal psychology. Among the contemporaneous viewpoints which are explained and evaluated are the anatomical, the physiological, and the psychological. A special chapter is also devoted to what the author ingeniously calls the *psychoanalogical* approach. This approach he says is "sometimes meaninglessly called the psychoanalytic."

The next general section of the book deals with the various classes of mental deviations from the normal. A brief summary of the material covered in this main body of the work may possibly best be given by listing certain of the main topics of the chapter. They are: feeblemindedness; origin of functional disorders; Janet's conception of psychoneuroses; typical psychoneurotic cases; the clinical views of Babinski, Hurst, Rosanoff, Freud, Adler, and Rivers; association and conditioning; objective study of the neurotic constitution; the psychology of the neurosis; stage-fright, dreams, stuttering, and aphasia; the psychological correlates of epilepsy; constitutional psychopathic states; personality types and the functional psychoses; and the relationship between drugs and mental disorders.

The fundamental theoretical concept of this volume may probably be said to consist in an interpretation of redintegration and integration. In developing the importance of redintegration the author points out that neurotics are as a group physically and psychologically inferior, and are affected by conditions which would not influence the ordinary individual. Typical in the history of each neurotic there appears a past environmentally determined episode from which the particular morbid symptom or symptoms under investigation date. There are, however, always discernible *present* stimuli, however reduced or attenuated, which redintegratively evoke the complaint, and in the absence of which the symptoms are abated and may, indeed, disappear. Particular pathological symptoms may therefore be changed by altering the environment or by changing the individual as in re-education. Such modification either of environment or organism, however, does not necessarily change the individual's psychological inferiority which shows itself in a proneness to develop new episodes and thus generate new symptoms.

Many other important practical observations and suggestions are given in the volume, but the reviewer believes that its major contribution is the explicit application as described above of the conception of redintegration to an understanding of mental abnormality. Redintegration is often spoken of as if it were a theory or hypothesis, but, at least in the reviewer's opinion, when correctly understood, it is merely a statement of facts which are attested by unimpeachable experiment and almost universal observation. Hollingworth in previous works has possibly done more than any contemporaneous psychologist to systematize the implications of redintegration in so far as they apply to normal mental life. In the present volume this concept has with equal success been applied to abnormal mental life. In the reviewer's opinion this whole body of work, both on normal and abnormal psychology, presents a system which is of great importance to one who wishes, with a minimum of speculation, to unite the psychological facts of observation and experiment into a coherent and explanatory psychological theory.

Brown University LEONARD CARMICHAEL

General Psychology for Professional Students. By A. R. GILLILAND, JOHN B. MORGAN, and S. N. STEVENS. New York, D. C. Heath & Co., 1930. Pp. vii, 439.

We are told in the preface that "this book was written to fill the specific need for a special text for professional students." This alleged need will be met, the authors announce, "by a text that emphasizes no particular point of view, stresses objective material and practical illustrations rather than theoretical discussions and selects the illustrations from the fields of industry and the professions rather than from those of art, literature, and science."

After the introductory chapter on the nature and purpose of psychology, in which the assumptions and techniques of psychology are balanced by some pedagogic advice to the student as to his attitude toward the study of psychology, the book deals in a simple, lucid style with chapters on the human organism, innate behavior, sensory processes, attention, perception, and the usual order of topics in orthodox textbooks of general psychology. The final chapter on mental health is the only significant departure from the usual choice of topics for discussion.

Although no distinctive point of view is made explicit, there is nevertheless interwoven with the discussion of facts and illustrations an implicit interpretation of facts and observations in terms of action psychology. Perception is defined as the "result of actual responses to stimuli." Memory is treated as "a form of the learning process in which the verbal components are more significant than are other types of response." The essentials of thinking are discriminating and generalizing which are described as "acts in response to a maladjusted situation." Insight, we are told, results from experimenting with a new situation in the light of previously experienced situations. Finally, it is held that the best definition of intelligence is the one that characterizes it as "the ability to make successful adjustments to the problems of life."

The text is amply illustrated and many of the figures and illustrations are original. To each chapter is appended a selected list of references representing every significant point of view in psychology. The title seems to be a misnomer. Neither the choice of material nor the method of treatment is distinctive enough to warrant the claim of a special type of general psychology.

University of Montana FRANKLIN O. SMITH

Chinin in der Allgemeinpraxis unter Berucksichtigung pharmakologischer Befunde. By FRITZ JOHANNESSOHN. Amsterdam, Bureau tot Bevordering van het Kinine-Gebruik, 1930. Pp. 232.

This book is essentially a review of 652 articles or works which throw light upon the problem of the physiological and therapeutic effects of quinine (Chinin), and of 157 articles which have to do with the effects and applications of "Chinidin," a less completely extracted form of quinine. All of the articles reviewed have been published since the close of the World War.

The opening sections of the book deal with the topic of the occurrence of quinine. The greatest part, however, deals with the physiological and therapeutic effects of the drug. The book will, therefore, claim the attention of the medical man to a much greater extent than that of the psychologist.

The author has rendered an important service in giving a clear and concise summary of the widely scattered literature upon quinine. The work is perhaps defective as regards the touch of originality; the author's name appears once only in the two long bibliographies. The material is well organized and the style is direct.

Wellesley College MICHAEL J. ZIGLER

Psychology for Students of Education. By ARTHUR I. GATES. Revised Ed. New York, Macmillan Co., 1930. Pp. xv, 612.

The revisions fall under two general heads: those which are made for the purpose of presenting material which has a maximum value in the work of the teacher, and those made to bring the text in line with the developments made in the field of psychology since 1922. One change of the first type has to do with the problems of physiological psychology. In the old text three chapters are devoted to such problems while in the revision this material has been reduced to one. The omitted material is concerned almost entirely with sense organs and localization of functions. The omission of the chapter on conscious states and processes is also to be noted. The most fundamental revision due to new developments is in the treatment of instincts. In the new text emphasis is placed on the theoretical aspects of the problem, along with the implications for education. A considerable amount of this material is put under the heading of motivation. Greater emphasis is now placed on personality and personality adjustments, and on researches in growth. Gestalt psychology, emotions and feelings in the life of the child with consideration of some individual cases, and more specific material on the general laws of learning—particularly from the field of psychology of reading and of other school subjects—might well have received more space and attention; and the chapter on the nervous system could have been made more up to date and useful. Our teachers of psychology who use the book have supplemented and modified the materials presented as seemed necessary.

University of Texas C. T. GRAY

The Behavior of the Newborn Infant. Ohio State University Studies, Graduate School Series: Contributions to Psychology. No. 10. By Karl C. Pratt, Amalie Kraushaar Nelson, and Kuo Hua Sun. Columbus, Ohio State University Press, 1930. Pp. xiii, 327.

This investigation was carried out with commendable objectivity under the direction of the late A. P. Weiss and Andrews Rogers (1) to determine the reactions of infants to certain stimuli at birth and the development of these reactions with age and (2) to note the appearance of 'new reactions.' The investigators carried out their studies on from 30 to 70 infants beginning one to four hours after birth and continuing for about two weeks. Every infant was put into a specially constructed experimental cabinet where temperature, light, sound, etc., could be rather accurately controlled. Provision was made for the automatic recording of responses in the head-foot and the right-left direction of each infant to both general and specific stimuli during the daily observation period, and two special windows were provided through which the experimenter and the recorder could make observations and records in order to get more additional data than either of these could record by himself and also to serve as special checks on each other's accuracy. By means of easily-marked symbols devised to designate different, carefully classified objective responses, the most commonly observed specific movements, postures, and conditions of the infants were recorded while the latter were in the cabinet. The experimental session was divided into 'control periods' and an 'experimental period.' The former were periods of no specially applied stimuli, intended to serve as a check on responses obtained in the experimental periods. During any 'control period' the recorder observed the infant through the small window at his end of the cabinet, noting down on the tape of the Renshaw polygraph various data as to the time of day, hygrometer readings, position and condition of the infant, etc. The polygraph was then started and a paper strip three and one-half inches wide was unreeled. Upon this the activity pens and the time-marker inscribed permanent records of the application of the various stimuli and the resulting movements as indicated above. Upon this strip the recorder also noted by symbols certain other specific movements of the infant. Thus both automatic records and observed records were made in their true time relation During the experimental periods stimuli of different kinds were carefully applied, and reactions to light, sound, taste, smell, temperature, and to the holding of the nose and also of the arms were recorded as already described, the time and duration of each stimulus showing on the time line.

The methods used were admittedly somewhat crude and mechanical and of a preliminary nature. The authors find that "the infant is not a man in miniature." It is equipped soon after birth "with quite a number of reflexes, but the degree of their specificity and their significance seem to have been unduly exaggerated" (p. 208)—a natural consequence, one may infer, of the earlier merely observational method. The child sneezes, coughs, hiccoughs, cries, etc., and does in a general way adjust itself to its environment. Movements which have to do with swallowing are fairly well coördinated: those movements which have to do with getting food, such as 'piercing cries, high and almost convulsive strampling activity, while not coördinated in the muscular sense, do act as strong stimuli on the social environment so that something is done about it," *e.g.* the child is fed. Similar reactions are given to such external conditions as decreasing temperature. "It seems certain that such well coördinated reactions as those implied by the terms love, rage, and fear do not exist at this early age" (p. 211). The authors found that the double-check method of observation was valuable. The reactions associated with specific stimuli which also occurred during the non-stimulus periods could certainly not be attributed to such stimuli although many qualitative observers have so attributed them. In the responses to external stimuli, the infant's behavior is generalized so that a stimulus affecting any group of receptors will produce a response in almost any part of the organism; but the reactions tend to manifest themselves most strongly in that part of the organism which is stimulated, and from there it spreads out in decreasing intensity to other segments of the body. The authors say that "even within the first two weeks one may observe a restriction of the generalized reactions as indicated by a decrease in the number of effector segments involved and an increasing specificity in their reactions to stimuli. We are not in a position to ascribe this increasing specificity of function as primarily due to either maturity or environmental factors" (p. 208). It is obvious that earlier observations cannot be relied upon in the light of these results by more accurate and objective methods. J. P.

Intellectual Growth in Young Children. By Susan Isaacs. With an appendix on children's 'Why' questions by Nathan Isaacs. New York, Harcourt, Brace & Co., 1930. Pp. xi, 370.

This is the first of a series of three volumes on child development. The data are based upon verbatim records of the activities and conversation of a small group of children in a private school at Cambridge, England, during a three-year period. All the children were from the professional classes, "many of highly distinguished parents." The average mental ratio on the Burt revision of the Binet scale was 1.31. The superior endowment of the subjects and their stimulating intellectual environment must be kept in mind in interpreting the findings.

Isaacs points out that the verbal responses of children to a series of standardized questions and exercises in a mental test given by an adult do not always afford a true picture of their actual ability to make rational judgments or to plan resourcefully when motivated by the needs of a concrete situation. She takes issue with Piaget's view that interest in mechanical causality and ability to appreciate it do not normally develop until the middle years of childhood, and cities a number of spontaneous comments and questions put by children of 3-5 yr. of age which appear to show a level of understanding far in advance of that reported by Piaget, whose conditions of observation and experiment were not, in her opinion, favorable to the manifestation of logical thinking on the part of the child. As a result of her observations she comes to the conclusion (p. 84) that "the difference between the younger child and the older, between the child and the adult, is thus *not* that the former do not reason or reason *only* in the form of the perceptual judgment and practical manipulation. It is rather the extent to which, with the younger children, the higher forms of noetic synthesis rest directly upon and grow immediately out of the simpler. Verbal reasoning and the clear formulation of judgments are no more than wavecrests upon the flow of young children's thought. They spring up from a practical or personal situation, and die down again to it. Verbal thinking can hardly yet be *sustained in its own* *right*, in the earlier years. It draws its vitality from the actual problems of concrete understanding and manipulation in which it takes its rise, and the solution of which it furthers. One of the marks of growth is thus the increasing independence of cumulative thought from immediate concrete and personal situations."

The major portion of the book is given up to accounts of individual incidents and conversations among the children illustrating the author's point of view. These reports are often highly suggestive, but the failure to arrange the material by age-levels or to group the data for the separate children in an orderly sequence makes it difficult to interpret the findings in terms of the growth trends suggested by the title. The treatment throughout is descriptive and philosophical rather than experimental.

University of Minnesota FLORENCE L. GOODENOUGH

The Social and Emotional Development of the Pre-School Child. By K. M. B. BRIDGES. London, Kegan Paul, Trench, Trubner and Co., Ltd., 1931. Pp. x, 277.

The greater part of this book consists of a detailed description of two scales, one for scoring the social development, and the other the emotional development, of the pre-school child, as they were formulated and applied to a group of 28 children in the McGill University Nursery School. The author observed the children for a period of a month, with respect to the items to be scored, before the actual scores were made on the basis of notes taken during the month. Much of the subjectivity of the usual rating scale was avoided by the method of scoring the occurrence of specific types of behavior under given situations. For example, a child was given a point on the social development scale if he had "sought another child's approval" or "comforted another in distress." The careful description of the types of behavior considered under each item, makes possible duplication by others of the method of scoring, and, in addition, gives an accurate picture of the behavior of pre-school children.

The Social Development scale is made up of two sections—(I) Relations with Children; and (II) Relations with Adults,—each of which is subdivided into socially desirable and undesirable forms of behavior. These are so worded that a point score is always given to the desirable reaction, and the final score is the percentage of observed responses which are desirable.

The scores obtained by the nursery school children show that, although there are individual variations, "children between the ages of two and five years pass through" first a "dependent stage" where "the child is somewhat passive and relies on the adult for assistance and attention"; secondly, between two and a half and three years, a stage of "resistance against adult influence and striving for power and independence." This attitude gradually changes to one of coöperation and friendliness, after the fourth year.

The Emotional Development scale, which is scored in the same way as the other, is divided into six sections—(I) Distress and Tears, (II) Fear and Caution, (III) Anger and Annoyance, (IV) Delight and Affection, (V) Excitement and Enuresis, and (VI) Mannerisms and Speech Anomalies. The scores on this scale showed very slight age differences. Far more noticeable was the increase of socially desirable emotional behavior during the first few months of nursery school attendance—evidence of the importance of experience in determining emotional behavior "and therefore the emotions." Wide individual variations were found, regardless of age. The kinds of stimuli which elicit the emotional behavior indicated under the section headings, are discussed.

A chapter on A Genetic Theory of the Emotions describes the increasing variability of emotional behavior in the growing child, from the undifferentiated excitement of the new-born infant, to some 15 readily discriminable forms of emotion in the 5-yr.-old. There are additional chapters on a character rating scale, and comparisons of the scores of individual children on the three scales. A large number of photographs of the children in the nursery school illustrate characteristic responses on which the scores were based. There is a bibliography and an index.

University of California NANCY BAYLEY

An Experimental Study of the Iowa Placement Examinations. By LAWRENCE WILLIAM MILLER. University of Iowa Studies, Studies in Education, Vol. 5, No. 6. Iowa City, University of Iowa Press, 1930. Pp. 116.

On the basis of careful analyses of new and extensive data and of weightings by multiple correlation new materials were devised and tested with a view of improving the placement examinations.

Duke University W. A. BROWNELL

Biblical Information in Relation to Character and Conduct. By PLEASANT ROSCOE HIGHTOWER. University of Iowa Studies, Studies in Character, Vol. 3, No. 2. Iowa City, University of Iowa Press, 1930. Pp. 72.

Hightower attempts in this monograph to learn whether mere knowledge of the Bible affects the development of character in children. He wishes specifically to discover how the child's conduct is affected when exposed to the temptation to cheat.

The methods of investigation conformed to accepted standards of research. The results indicate no significant relationship between Biblical information and conduct. The author concludes, therefore, that the methods of instruction and the "curricular arrangement of Biblical material" must be altered if desired results are obtained.

Newcomb College, Tulane University DAGNY SUNNE

La science du caractère. By W. BOVEN. Neuchatel, Delachaux et Niestlé, 1931. Pp. 345.

This work, an *essai de caractérologie générale*, is an instructive sample of a type of psychological writing common in Europe, but not represented in America. The author draws freely from both the German and the French tradition in characterology. His method, however, resembles less the dialectical attacks of Utitz, Künkel, Ziehen, and Hoffman than the pure schematization of Paulhan, Malapert, Achille-Delmas, and Boll.

According to Boven the structure of character consists basically of three systems, the vegetative, the motor, and the sensorial, through which operate seven major dispositions (desire, domination, pain, pleasure, fear, anger, and sympathy) that constitute the first *assise* of character The second *assise* comprises the level of traits which are "attitudes habitually adopted" toward the self, others, things, and abstractions. The third *assise* is composed of *linéaments* representing higher integrations or "prolongations" of traits into values, intentions, or concepts. The higher levels are more educable than the lower.

This doctrine of the constitution of character is overlaid and obscured by the bulk of the book which presents a spacious view of certain general topics. The author hopefully undertakes to illuminate the field of character through a survey of heredity, embryology, intelligence, race, sex, age, disease, and education. The reader is likely to feel that the treatment is indistinct, and that the scientific study of personality demands a sharper focus.

Harvard University GORDON W. ALLPORT

Legal Psychology. By H. E. BURTT. New York, Prentice-Hall, 1931. Pp. xiv, 467.

The author wants to show lawyers what possibilities there are at the present time for applying the modern technique of psychological measurement to problems which may occur in the practice of law. After a somewhat popularizing introduction, a long chapter (42 pages) summarizes the most common forms of illusions of sensation and perception. Two briefer chapters then point out the difficulties most commonly obstructing attention and memory. A further chapter discusses hypnotism from its mildest aspect in "the leading question" to the probabilities of post-hypnotic influences. The value of testimony and confession and problems of the jury are then taken up. The detector methods of word association, breath recording, and pulse recording are treated in three chapters, together covering 80 pages. Two chapters of 74 pages are then given to the mentally disordered and the mentally defective criminal. The possibility of discovering in childhood and early youth symptoms of later criminal tendencies is then discussed in a brief chapter, somewhat too optimistically as it appears to the reviewer; but the author could hardly take here a pessimistic attitude.

The reviewer does not understand how the next chapter, the one on eugenics (31 pages), came to be fitted into a book on legal psychology, as it has no relation to the practice of law.

The chapter on punishment, which follows the one on eugenics, reiterates the arguments in favor of the possibility of making mild punishments, through certainty of application, as deterrent as severe, but rather uncertain, punishments. In a chapter on drugs the author joins those psychologists who from experiments on extremely simple reflexes conclude that the belief of having taken a drug might be equivalent to an actual drug effect. The reviewer is peeved because the author is unacquainted with his method of studying the effects of drugs on highly intellectual processes, which seemed to show that the expectation of a drug effect does not produce a result similar to that of a drug.

Two chapters on crime suggestion and crime prevention, being purely sociological, do not seem to fit in the book any more than that on eugenics. The last chapter on trade-mark infringement points out to the legal profession that "the degree of confusion" can be measured more exactly than various court decisions seem to suppose, but admits that "many other things enter into a decision."

There was a need for a book like this and the author deserves the gratitude, not only of the lawyer, but also of the psychologist, for having gathered whatever seemed to him to belong together, although opinions may differ slightly in this respect.

University of Missouri MAX F. MEYER

Psychology for the Lawyer. By D. G. McCARTY. New York. Prentice-Hall, 1929. Pp. xiii, 723.

If the matter directly quoted were to be removed from this book, it would cover a little more than 300 pages. That, however, is no fault for the lawyer, since lawyers are accustomed to books made up of quotations. In reviewing the book for psychologists, the psychologist can hardly abstain from expressing the judgment that it is of very mixed quality. There is at least one chapter which to the scientific thinker is trash. There are other chapters, and not a few, which the psychologist will read with high appreciation of the author's keenness in analyzing problems of the court room and of legal practice. Such are the chapters on Bias, Intention in the Law, Psychology and the Law of Evidence, Responsibility and the Criminal Law, and Knowing the Client.

The introductory chapters, which give the lawyer an exposition of general psychology, are full of contradictions. For this the psychologists whom he has read are perhaps more to be blamed than the author. He has taken the quibbling of the 'schools' too seriously, as most psychological laymen are apt to do. But how a whole chapter on "phrenology with the bumps moved on the face" (so the author, quoting Allport, contradicts himself) could have gotten into the book, is difficult to understand. The chapter on Strategy in a Lawsuit is extremely interesting because it shows that a successful lawyer's ethics must by necessity be of the same quality as that of the successful warrior; that is, brutal and unscrupulous. The professional ethics of the practice of law has about the same meaning as that which the international law has in actual warfare. The psychologist who is looking for problems in social psychology to the solution of which he might apply some of the methods which social psychology is developing, will find in the book a rich source of suggestions, sometimes contrary to the author's intentions, but often set forth with clear intention, especially in the chapters which the reviewer has above enumerated as the best of the book.

University of Missouri MAX F. MEYER

Psychology in Modern Business. By HARRY WALKER HEPNER. New York City, Prentice-Hall, 1930. Pp. xv, 728.

This is in many respects a useful book in spite of the fact that it falls into the error of most books on applied psychology; namely, it covers too much ground on the psychological side and, on the side of application it includes too much from fields other than psychology. The first 150 pages of the book are to give the business man insight into the "Nature of the Mental Life." But the reviewer doubts the possibility of realizing such an ambitious scheme. The author wants to protect the business man against quack methods. But can that be done by dubitable bits of information like the following? "To understand why people behave . . . it is necessary for the business man to have an acquaintance with glands." "Hysteria is more common among . . . low . . . than . . . high intelligence." In the opinion of the reviewer nothing would have been lost if the book had started on page 151 with "the pseudo-scientific methods of analyzing people." One could apply, with adaptation, to the first part of the book what the author says on page 535: "We attend the theater (the business man reads these pages), not to see life (psychology) as it is, but to see it in a romantic situation." More or less this applies to the whole book. It is a readable book, a good seller.

The bulk of the book is divided into four parts, the second and third having the individual in view, the fourth and fifth the group; but it is clear that the distinction is more the result of book-makers practice in pleasing the reader by a conspicuous classification than the result of psychological science. So we find Credit, Hiring, Vocational Choice, and Salesmanship, treated as individual problems, but reappearing as group problems with little change of "psychology" under the titles of Good Will, Social Services, and Advertising Devices. But this statement is meant only as a criticism of the scientific value to the psychologist, not as a criticism of the practical usefulness of the book for the business man. The material has been largely collected from trade publications and commercial magazines.

The professional psychologist who is not a business technician may be most interested in the last sixty pages, the sixth part of the book, which gives rules for undertaking any business psychology research. The limitations of the laboratory method are pointed out. The questionary is critically discussed. A very brief account is given of the statistical treatment of rankings. A few interesting tests are printed. The meagerness of the sixth part compared with the four parts preceding it suggests the question: How far has applied psychology advanced beyond being a mere study by the descriptive "case method"? The author (p. 549) is right: "Abstractions do not impress the reader." Therefore he has reduced them to a minimum.

University of Missouri MAX F. MEYER

The Remaking of Marriage: A Contribution to the Psychology of Sex Relationship. By POUL BJERRE. Translated by T. H. Winlow. New York, Macmillan Co., 1931. Pp. viii, 257.

Two very brief quotations indicate the nature of this work. "In the series of chapters beginning with Love and Anxiety and closing with Heroism and Tragedy I present various views and join them loosely together without attempting a systematization" (p. 11). "First of all, as far as the psychological standpoint is concerned, we reject all systems as human inventions and proceed directly to nature with that will to lucidity which has been given to the mind generally, and to the subconscious especially" (p. 19). The book is more of the nature of the memoirs of a practicing psychiatrist than it is a scientific treatise. The author believes the chief functions of sexual relationships are to ward off loneliness, to smooth out life's difficulties, and to secure a 'renewal of the spirit.' Marriage is an attempt to find one's self, and loneliness is the only valid cause for divorce. He recommends (p. 216) a period of betrothal which is equivalent to legalized free love, but suggests that legitimization should not include publication. On the other hand, he considers best of all what he terms "the sacramental union," a joint struggle through all life's difficulties. This, apparently, is to be entered upon if the experiences of the betrothal period justify it.

Southwestern University W. R. Atkinson

The Influence of Training on Changes in Variability in Achievement. By Homer B. Reed. Psychological Monographs, No. 185. Princeton, The Psychological Review Co., 1931. Pp. 59.

Reed gives a critical evaluation of eight different statistical techniques that have been used in studying changes in group variability with training. He selects three of these, viz., (1) coefficient of variability, (2) correlation between initial score and percent gain, and (3) ratio of best to worst before practice compared with ratio of same individuals after practice, as being best adapted to the problem. These three measures are reported for the data of 25 published investigations, Reed computing the measures himself whenever not given by the authors. It is pointed out that when the same measures are used, the apparent discrepancy between the results of different investigators disappears, the great majority of results showing a decrease in variability with equal training.

A statement frequently repeated by Reed throughout the monograph is that the S.D. takes into account only the middle two-thirds of the cases. This seems to be based on a misinterpretation of the fact that a distance of ± 1 S.D. from the mean will, in a normal distribution, include roughly two-thirds (actually 68%) of the total number of cases. This does not imply that only those cases are taken into account by the S.D.; the extreme deviations have, in fact, more weight than those near the mean in determining the size of S.D., since each deviation is squared.

In addition to his analysis of published data, which takes up the larger part of the monograph, Reed reports two new researches on the problem. The first is on practice in a motor task, viz., throwing the yo-yo top. The subjects were 37 college sophomores. They had a total of eighteen 10-min. practice periods, the score being the number of successful 'catches' during each period. In the second study, the Stanford Achievement Test and the Compass Survey Arithmetic Tests were given in October, February, and May to 137 school children in grades 2 to 8 inclusive. The 'practice' consisted only of the ordinary classroom work intervening between the three testings. The various measures of variability computed in the two studies again show a decrease in individual differences after training. In addition, the probability of a decrease in variability seems to be greater for the extremes of the distribution than for the individuals closer to the mean. Reed writes about these results that "the greater the difference between individuals in achievement is the greater the probability that training will reduce this difference." (p. 53). His data suggest only that this is true *within* any one distribution; no evidence is given that it applies to comparisons *between* distributions. Finally, Reed suggests the possibility that the amount of reduction in variability may be a function of the amount of gain during the training period. His evidence is that in the study on school-children, greater gain as well as greater reduction in variability occurred from October to February than from February to May. This raises an interesting problem which should be investigated, and we wonder why similar results are not reported for the data on the yo-yo top. In that study, practice was more carefully controlled than in the study on school children, and furthermore, there were 18 rather than 3 records on each subject. Such data would have been much better adapted than those used to studying the course of decrease in variability and its relation to the course of improvement in performance.

The criticisms suggested above do not in any way detract from the fundamental contributions of Reed's study, which seem to be, first, the demonstration of the essential agreement among published results on the effect of practice on variability, and secondly, the suggestion of a number of questions which require further investigation.

Columbia University ANNE ANASTASI

Das Genie-Problem. By W. LANGE-EICHBAUM. Munich, Ernst Reinhardt, 1931. Pp. 128.

This little book is nineteenth century in spirit, in method, and in such few allusions as are made to the work of scholars other than the author. Gravely assuring his readers that an analytical study of genius should no more destroy its enchantment than should the study of botany and astronomy detract from the beauties of nature, the author proceeds to outline his field, and succeeds in making it appear very complicated indeed. The necessary and sufficient condition of genius are discussed in Chapter 1. In succeeding chapters are considered (1) sociological aspects—fame and milieu; (2) endowment of mind and body; (3) genius and insanity. Problems (1) and (2) are handled as meticulous expansions of the definition of genius formulated in Chapter 1: "a bearer of spiritual values who is revered by many." The bearer, the values found in his accomplishment, the recognition accorded him, and his admiring public, are methodically discussed under such headings as "disposition and aptitude;" "inborn talent;" and "the origin of fame."

The section on problem (3), genius and insanity, is not closely related to the main discourse, but deals with the incidence of psychoses and psychotic traits among men of genius, and seeks to establish an intimate functional relationship between genius and mental disorders. In this field, where vigor of thought and positiveness of statement cannot substitute for research data scientifically collected and interpreted, the author is most inadequate. Disregarding all modern studies of the much discussed subject, he builds up from within a remarkable series of statements and inferences. Of these, the climax is the conclusion that while genius and madness may not be synonymous, the insane are more liable than others to become eminent!

Pasadena City Schools BARBARA S. BURKS

Animal Motivation: Experimental Studies on the Albino Rat. By C. J. WARDEN, with the Collaboration of T. N. Jenkins, L. H. Warner, Marion Jenkins, E. L. Hamilton, and H. W. Nissen. New York, Columbia University Press, 1931. Pp. xii, 502.

This volume brings together 13 reprinted papers and one new article reporting researches on animal drives. The entire project is an attempt to measure and compare the maximal strengths of the maternal, thirst, hunger, sex, and exploratory drives under definitely standardized conditions. The reviewer feels that it is probably the best work that has been done in this field. It is superior to previous studies in its methods and apparatus, which were definitely standardized after much preliminary experimentation, in its detailed statistical analysis of the results, and in the intrinsic value of the results and conclusions themselves.

The method used in all the studies, except the four in the appendices, is the well-known obstruction method which involves the placing of an obstruction (electrical grill) between the animal and the incentive. Indices of the relative strengths of the drives are in terms of the number of times the animal passes from the entrance compartment through the obstruction compartment, into the incentive compartment in a certain period of time (here 20 min.).

Since all of the papers making up the present volume, except the one which compares the maximal strengths of the drives, have appeared elsewhere, we shall deal principally with the latter.

The different drives are considered to be maximal after deprivation periods of one and two days for the females and males, respectively, for thirst, and two and four days for hunger. Sex deprivation for one day for the males and the female in oestrum produce the peaks of sex activity. For the maternal drive females who had given birth to their litters 15 hours before testing were used. For the exploratory drive no special arrangements were made to control the little-known intra-organic conditions influencing the tendency. There is, of course, no assurance that the last two drives were maximal. The incentive used for the maternal drive was the living cage containing the litter of young. This means that the maternal drive plus the tendency to return to the home nest were both involved. This possibly accounts in part for the great motivation under these conditions. The authors recognize these limitations. Under these conditions the drives are ranked in strength as follows: maternal, thirst, hunger, sex, exploratory.

Besides these limitations, there is another objection: when comparisons between the drives are made on the basis of 5-min. periods instead on that of total scores, significant differences in persistence are found. This means that the ranking of the drives is in part a function of the length of the test period. If the drives are ranked on the basis of every 5-min. period, thirst decreases in rank from first in the first two periods to fourth in the last. On the basis of totals it is placed second. The exploratory drive is the only one that maintains a constant rank throughout.

Since we are comparing the maximal conditions of the different drives perhaps we should use the period showing the *greatest* number of crossings. These we find pp. 385, 395, to be thirst in the first 5-min. period (153 times), maternal in the first (126), hunger in the third (100), sex in the fourth (94 both sexes), and exploratory in the first (48). These comparisons lessen somewhat the significance of the final rankings given by the authors. If the test period were 10 instead of 20 min. in length, the peak of activity would be after a deprivation period of two instead of three days for hunger. Table 5 (p. 205) shows that the 35-days-segregated males, after 28 days of sex deprivation, cross the electric grill to get to a cornified female 16, 40, 54, and 101 times, respectively, during each of the four successive 5-min. intervals. After only one day of sex deprivation they cross 76, 63, 60, and 70 times during the corresponding periods. The totals are 211 and 269, respectively, and the authors conclude that "after one day [of deprivation] the sex drive seems to diminish slowly in strength as the segregation period lengthens" (p. 212). The results suggest that if a longer text period had been used a somewhat different conclusion would be justified. And, after all, a 20-min. test period is rather arbitrary! The authors admit that the meaning of the "loose concept of persistence of drive is not at all clear" (p. 384), but their failure to take it into consideration lessens somewhat the significance of some of their comparisons.

A few errors occur. The "22" in the column at the extreme right of Table 3, p. 385, opposite "Sex-female, Oestrum," should be "122." On pages 78 and 80 a one-day starvation group is mentioned, but so far as the reviewer is able to discover there is no such group in the study. There is much unnecessary repetition in the details of apparatus and procedure, and in the bibliographies. Every oneof the seven parts and the separate appendices has an introduction by the senior author.

University of North Dakota C. W. TELFORD

Child Psychology. By JOHN J. B. MORGAN. New York, Richard R. Smith, Inc., 1930. Pp. viii, 474.

After an introductory chapter on Methods and Objectives of Child Study, the author lays the foundations for his later discussion in a chapter on Infant Behavior. In this part, he makes effective use of recent experimental data gathered from studies on infants, in order to show the genesis of later attitudes and more fully developed behavior. The subsequent chapters are written chiefly from the point of view of the clinical psychologist. All in all, the reader may get from them a fair notion of the possibilities of education and control, as well as the best means by which desired ends may be accomplished. Advice for guidance and for treatment of problem children is concretely given with examples drawn from the author's experience. A list of the additional chapter headings will indicate the subjects covered. The Child's Adjustment to His Family, Motor Development, Emotional Development, Motivation, Development of Meanings, Imagination, Language, Verbal Learning, Thinking and Working, Play, Social Development, Intelligence, and The Integrated Child.

In the preface, the author expresses the belief that the book will be of value to teachers and to parents, and that it may serve as a text in child psychology. For the latter purpose, it seems to the reviewer somewhat elementary, although on the whole, it may be recommended to such of the former as do not possess any special knowledge of the subject, and who will not be too much annoyed by the trivial platitudes which so frequently fill up the spaces between useful facts and wise clinical insights.

University of California NANCY BAYLEY

Cope: Master Naturalist. By HENRY FAIRFIELD OSBORN and others. Princeton, Princeton University Press, 1931. Pp. 740.

This is a life story of a great naturalist and it is well worth the perusal of all students of natural science. Many of Cope's letters are reproduced which not only make pleasant reading but which throw important sidelights on his scientific ideas.

Cope was superlatively able and productive in comparative anatomy and vertebrate faunal zoölogy and paleontology. His personal experiences and human contacts, his travels and dealings with all sorts of people, his general reading and philosophical speculations, his interest in comparative ethnology, in human and anthropoid variability, in phylogeny and evolution, in archeology and pre-history, and his special work in osteology, craniology, and comparative odontology connect him with the anthropological and psychological sciences.

A classified bibliography of 1395 titles lists a number of interest to the psychologist—in particular the genetic observations and discussions on the habits of many types of animals.

New York City WILLIAM HARPER DAVIS

Social Psychology. By JOSEPH K. FOLSOM. New York, Harpers, 1931. Pp. xviii, 701.

The expressed wish of the author is that his book will prove equally acceptable to psychologists, sociologists, and cultural anthropologists. Basic to this hope is the treatment of social psychology from three different angles; namely, individual behavior approach, social interaction approach, and cultural approach. These three approaches to the field of social psychology are treated in the order indicated and, if the author's judgment is our criterion, they have been integrated into a well organized whole. Before questioning the validity of this judgment, it should be admitted that Folsom has drawn intensively upon many inductive studies of social behavior, although his claim to have covered all of the important discoveries and conclusions of research in social psychology need not be taken seriously.

Undoubtedly sociologists and cultural anthropologists are capable of evaluating Folsom's contribution to their respective fields. In the reviewer's opinion, psychologists will easily detect the absence of integration within the "approach trinity" and, in addition, they will question the pertinence to social psychology of the greater part of the material arrayed under the individual behavior approach. Lengthy descriptions of the nervous system and discussions of theories of learning, occupying as they do approximately one-sixth of the total text, immediately confront the student of social psychology; yet the author kindly informs the persistent reader, "we are now to take the elementary processes for granted and to study the higher forms of organization" (p. 135). In similar manner chapters V and VI, entitled Wish Frustration and Personality Readjustment and Individual Personality Differences and Their Measurement (pp. 167-295), which conclude the individual approach, are not utilized appreciably in succeeding discussions. These two chapters contain excellent material for students of abnormal psychology and of personality measurement; but a reader ignorant of the book title could not be expected to guess it correctly from the contents of these chapters. The section of the text presumably of interest to psychologists is not an organized unit but a series of discrete topics. Had these been made to yield service to the two remaining approaches the book would command the interest of psychologists, but the lack of integration forces attention primarily to a few meritorious chapters.

University of Minnesota CHARLES BIRD

Academic Progress in the University. By HAROLD A. EDGERTON. Baltimore, Warwick & York, 1930. Pp. 83.

"This study has as its purpose the investigation of some of the possibilities of a system of cumulative academic prognosis." The sample consists of those students who entered the five largest colleges of the Ohio State University in 1924 and 1925. The criterion of academic success is the point-hour ratio during the student's first six semesters in the University (points being determined as follows: each hour of A, 4 points; B, 3 points; C, 2 points; D, 1 point; E, 0 points). Results are presented separately for each sex and for each of the five colleges. Briefly, it is found that the correlation between academic success and percentile score in the Ohio State University Psychological Examination (Intelligence Test) is, for the 1924 group, 0.517; for the 1925 group, 0.490. Between academic success, and a combination of intelligence test and point-hour ratio at the end of the *first* semester, the multiple correlations are 0.882 and 0.868 for the 1924 and the 1925 groups respectively; between academic success and a combination of intelligence test and point-hour ratio at the end of the *fifth* semester, the correlations are 0.979 and 0.983. Intelligence-test score contributes practically nothing to these last four correlations which is not already included in the point-hour ratio. It is concluded that academic success (*i.e.* point-hour ratio at the end of the first six semesters of work) can be reliably predicted from previous academic performance.

Edgerton's monograph supplies a careful, thoroughly competent study of an important problem. The statistical technique, in particular, merits commendation, among other things, for the use of Fisher's z—function of r. We are less certain as to the theoretical validity of the use of percentile scores in Pearsonian correlation. The defense to this, doubtlessly, is the practical fact that percentile scores are what the Ohio State University actually employs in its personnel work. The reviewer fails to understand why no attempt was made to determine, by some split-half technique, the reliability of the point-hour ratio; the fact that *ratios* are involved is not the statistical *kobold* that Edgerton (p. 17) would appear to believe.

The weakest point of the monograph doubtless lies in its serious omission. What is the correlation between academic performance in the first three semesters and in the *next* three? An answer to this question would constitute real prediction. Edgerton, *per contra*, presents only the correlation between performance in a certain number of semesters, and performance in a longer period which *includes all the past semesters but one.* This is not genuine prediction at all, even if it be "prognosis."

University of California HERBERT S. CONRAD

Mental Development of the Child; A Summary of Modern Psychological Theory. By KARL BÜHLER. Translated from the 5th German ed. by Oscar Oeser. New York, Harcourt Brace & Co.; London, Kegan Paul, Trench, Trubner & Co., 1930. Pp. xi, 170.

This volume is chiefly a summary and discussion of the early German contributions to the field of child psychology supplemented by considerations of the experiments of Köhler and those of C. Bühler, Hetzer, and others in the Vienna laboratories. The plan of organization does not seem to the reviewer to be entirely consistent. There is an excellent and brief summary of comparative psychology in so far as it contributes to the study of the young child, and the treatment of physical development is clear and concise and has excellent illustrations. The author distinguishes the three factors operating in successive stages of behavior—instinct, training, and intellect. Instincts are considered to be "a ready-for-use inheritance of modes of behavior set going in a definite way according to a preformed natural plan." Training works by suppressing some, accentuating others and forming new combinations of the instinctive modes. Intellect is defined as "the ability to discover and invent." It is used almost synonomously with the Gestalt "insight."

The later chapters are highly theoretical and are chiefly expressions of opinion. There is little evidence that the author is familiar with the outstanding experimental studies by French and American investigators in recent years. His chapter on children's drawings does not include a mention of the Goodenough "Drawing Test" nor does his chapter on language reveal any knowledge of the work of Descoeudres, Piaget, Smith, and many others of importance. However, his approach to the development of concepts, judgments and reasoning in the child is strikingly similar in many respects to Piaget's. Although all the books of Piaget are listed as references at the end of the chapter on the Origin of Concepts, there is not a single mention of these works in the text. Inasmuch as this volume is a summary of the author's *Die geistige Entwicklung des Kindes* it probably should not be censured for incompleteness. However, any adequate "summary of *modern* psychological theory" (italics mine) should include at least some comments upon the outstanding recent experimental studies in the field. The revision thus leaves much to be desired. The first edition was published in 1919.

University of Georgia DOROTHEA McCARTHY

A Syllabus for the First Course in Experimental Psychology. By S. L. CRAWLEY. New York, Henry Holt & Co., 1930. Pp. 177.

This is a practicable manual for use in schools with no or very limited laboratory facilities. Most of the experiments, of which there are 24, are of a type that makes possible their use with an entire class as subjects and the instructor as experimenter. The directions are complete, although sufficient provision is made for adaptation of technique to local conditions. The experiments outlined are of the usual sort, dealing with individual differences, observation and report, suggestion, sensory acuity, feelings, intelligence, learning, memory, etc. A bibliography of from 10 to 41 references for each experiment, consisting chiefly of citations in elementary texts, is quite complete for its type. It could have been improved, however, had certain of the best treatments been starred, or those books indicated which give practically the same summary of any one topic. Detachable sheets for experimental write-ups are included in the manual, as are also forms for that material which needs to be mimeographed. A helpful list of firms which deal in laboratory equipment is appended. The syllabus should supply a real need as a bit of enlivenment for the all too customary textbook course in psychology.

DePauw University STEPHEN M. COREY

Educational Measurement in the Elementary Grades. By I. N. Madsen. Yonkers, World Book Co., 1930. Pp. x, 294.

This is a useful rather than a highly important or significant book. Like certain men, it is adequate without being brilliant—and adequacy, though less exciting, is often more satisfying.

Recognizing the needs and limitations of beginning students in normal schools and colleges, the author has attempted to meet these needs without putting a strain upon the limitations. Assuming a complete lack of information on the part of the student in the field of tests and measurements, he devotes the first part of his book to a simple presentation of the facts of individual differences and of elementary statistical procedures.

A chapter each is given to individual intelligence tests and to group tests. This material is followed by the most distinctive section of the book—a presentation and brief discussion of all the worth-while achievement tests on the elementary school level. Any teacher, beginner or otherwise, would find this material of great practical value. As Terman says in the Editor's Introduction, "Dr. Madsen's book gives about all the information on this subject that can reasonably be regarded as essential for the rank and file of teachers to have."

University of Minnesota Herbert A. Carroll

"Insight" in Rats. By E. C. Tolman and C. H. Honzik. University of California Publications in Psychology, Vol. 4, No. 14. Berkeley, University of California Press, 1930. Pp. 215-232.

This brief paper reports three attempts to prove "insight" in the white rat in various types of maze situation. The general procedure involved, first, a preliminary training on the apparatus in which a strong preference for one pathway to food was developed, a less strong preference for a second pathway, and no preference for a third pathway; secondly, the "insight" runs in which only the latter pathway is open. If this pathway is taken directly by the rat, then insight is supposedly indicated. Tests were made on three types of mazes, embodying this essential feature but different in minor details. In only one of the three set-ups were positive results secured. The authors acknowledge that we do not know under what conditions insight appears, and they might have added that we do not know what insight is, either in rat or man. Interpretations aside, the authors have shown how the white rat behaves in three situations in which it looked as if "insight" should appear if the rat possessed the same sort of insight as the authors exhibited in arranging the problem for the rats.

Columbia University C. J. Warden

Coöperation and Competition: An Experimental Study in Motivation. By Julius Bernard Maller. New York, Bureau of Publications, Columbia University, 1929. Pp. x, 176.

Eight hundred and fourteen pupils of grades V to VIII were tested for speed of simple addition (1) when merely practicing, (2) when attempting to attain high individual ranks, and (3) when the scores of one school class were to be compared with those of another class. The difference between the pupil's score when working for himself and his score when working for the group was taken as a measure of his coöperativeness. A second measure of coöperativeness was obtained by allowing the pupils to choose at the beginning of seven tests whether they would work for themselves or for the group. Competition (work for self) was found to result in greater speed than coöperation, 32.4 examples more on the average in 12 minutes work a difference 13.2 times the S.D. Also work for self was chosen 74% of the time. The two measures of coöperativeness were found to have a correlation of 0.23, although the reliability of each measure was found to be high.

Coöperativeness was found to differ with the homogeneity of the group (work in a specially organized team was more efficient even than work for self), with sex (girls more often choose to work for the group, but in general they do not work as near their maximum speed when working for the group), with nationality, and with the amount of discussion about the significance of coöperation (Boy Scouts, Girl Scouts, etc. were more coöperative than their classmates, and after a series of three discussions the work for the group was more efficient). Siblings and mutual friends were found to resemble each other in coöperativeness.

The measures of coöperation were correlated with a great number of other measures and ratings: (1) rating in coöperativeness by classmates and teachers—.35, (2) rating in coöperativeness by a psychologist—.80, (3) other tests of helpfulness—.42, (4) intelligence—positive but curvilinear, (5) chronological age, mental age, moral knowledge, resistence to suggestibility, deportment in school, character ratings, tests of honesty and inhibition, neurotic index—positive, (6) speed of work, variability in speed, health condition, vision, condition of teeth, nutrition, socio-economic status, frequency of attendance at movies, weight-height ratio—negative, (7) number of friends and number of siblings—positive but curvilinear.

Those who were in the upper quartile of coöperativeness as measured by the difference in speed of work for self and for the group were compared on 66 variables with those in the lower quartile.

This study is, in the reviewer's opinion, an important contribution in the field of character education and trait measurement. The low correlation between the two measures of coöperativeness and between these and other tests of helpfulness raises the question, pertaining to most tests. What is measured? But testing and correlating test results with other variables seems a promising method of attacking certain very real and practical problems.

Stephens College CARL N. REXROAD

Sangren Information Tests for Young Children. By PAUL V. SANGREN. Yonkers, World Book Co., 1930. Pp. iii, 28.

These are individual tests intended (1) "to call attention to the need for greater definiteness and certainty in nursery and kindergarten instruction," (2) to determine what young children at various stages of maturity and of instruction actually know, (3) to fix certain standards of promotion and placement, and (4) "to provide a basis for instructional practices and classroom management at the beginning of the first grade" (p. 1). Six types of material are used: Nature Study (28 items, maximum score 35); Numbers (17 items, maximum score 35); Vocabulary (35 words from the Gates Primary Reading Vocabulary, maximum score 35); Social and Civic Information (30 items, maximum score 31); House-hold Utensils (16 items, maximum score 22); Language and Literature (eight divisions of items, maximum score 35). Children are examined individually, and the test scores are designed to supplement rather than to supplant intelligence measures. Norms (median scores) for the test as a whole and for the various parts are given for ages 5, 6, and 7-yr., and are based on the results secured with 91, 127, and 32 children, respectively. In the absence of more desirable criteria of validity, test scores were correlated (1) with chronological age—r ranged from 0.02 to 0.25; (2) with Stanford-Binet MA—r ranged from 0.46 to 0.73; and (3) with Stanford-Binet IQ—r ranged from 0.36 to 0.66. Reliability of the test obtained by correlating test-halves and using the Spearman up-step formula was 0.96.

A carefully planned and easily understood manual gives all instructions for scoring and recording. Publication, however, seems to have been premature as age-norms are based on data secured from only 250 children representing "not unselected groups" (p. 4), and standards of grade placement and promotion are derived from data for only 62 kindergarten and 63 first-grade children. One may question, moreover, whether the six types of material which comprise the tests furnish an adequate sample of a child's general information. The value of the tests for determining promotion and placement in the kindergarten and first grade is probably limited since the information tested is of little importance for the educational objective at this level of instruction.

Duke University W. A. Brownell

Reëducation psychologique par le travail. By Maurice Dide. Toulouse, Lyon & Bros., 1930. Pp. 22, four plates.

This is a pamphlet presenting from extensive successful experience a plea for the value of absorbing, constructive work in the psychological reëducation of mentally deranged individuals who have a certain degree of clearness. The author shows a number of full tone photographs of beautiful furniture, of decorative work forged in iron made in his own institution by the inmates, and of buildings constructed with their aid. The idea seems to be to get the inmates so interested in some real productive work of any kind whatever—including gardening, breeding domestic animals, work in nurseries, etc.—as a mode of self-expression that they lose the abnormal inhibitions and thoughts of self, and emerge from the monotony of routine existence and conflict into self-functioning individuals able to resume normal social habits. He admits that the surprise provoked by such tendencies to industrialism in mental hospitals indicates how such an innovation affects traditional opinions and conflicts with administrative dogmas. J. P.

Mind and the World-Order: Outline of a Theory of Knowledge. By Clarence Irving Lewis. New York, Charles Scribner's Sons, 1929. Pp. xiv, 440.

This is an approach to the problem of knowledge from the field of exact logic, from a point of view labelled by the author "conceptualistic pragmatism." The method is reflective; "it accords with the Socratic presumption that the truth which is sought is already implicit in the mind which seeks it, and needs only to be elicited and brought to clear expression. It accords, further, in the recognition that it is definitions or 'essences' which are the philosophic goal." Lewis suggests that Reality (with a capital R) is a sterile concept. Instead, the term "real" can be compared to the term "useful," and as a thing may be useful for one purpose and useless for another, so it is real from one point of view and unreal from another. "Nothing is real in all categories; everything is real in some category." The categories are independent of experience in that they are derived by definition, and a thing may be unreal physically by definition, while being real experimentally. The proper task of philosophy is the consideration of these categories and the explicit definition of them.

A logical analysis is made of the knowledge process, and the conclusion is reached that "there is no knowledge of external reality without the anticipation of future experience." Knowledge, when valid, is always *verifiable;* verification may be actual or hypothetical.

The author criticizes, in passing, those who deny the existence of consciousness because the "mental" must always be analyzed in terms of bodily behavior. An analysis must interpret the thing analyzed in terms of its constant relation to other things, and a term *is not* another term with which it has a constant correlation. The book is primarily an essay in epistemology and metaphysic, and is not directly relevant to psychology. It demands and will repay careful study by those who are interested in the problems considered.

Cornell University. MERRILL F. ROFF

La répression mentale. By PAUL SOLLIER. Paris, Félix Alcan, 1930. Pp. ii, 218.

Sollier is interested primarily in repression as a dynamic phenomenon, considering that the process of repression is more important than the thing repressed. He objects to the Freudian doctrine on two grounds: first, that there are no instincts, properly speaking, but imbalances in the organism; and secondly, that anything, good or bad, may be repressed if there is a contrary force working against it. "We may say, in effect, that there exists no psychological phenomenon having any more than another phenomenon the privilege of provoking mental repression."

Repression, for him, is a "phenomenon constituted by three essential and contemporaneous elements: a *tendency* arrested in its course; a *resistance* which not only arrests but also represses the tendency; and *affective reactions* or painful affective forces which result therefrom." The tendency is the activity of the organism in disequilibrium; the resistance may be internal or external, but is always active; the affective reactions are emotional disturbances. "Mental repression is essentially an active, dynamic process, but one in which the energy expended is employed in negative rather than positive work." Thus, the repressed individual may appear superficially inactive, but show definite fatigue; or he may even break under the strain, just as a ship may seem stationary but be strained by being impelled in one direction by the current and in the opposite direction by the wind. "Between the physico-chemical world and the psychic world there is no heterogeneity, and the same laws govern both."

Sollier finds the phenomena of repression in various aspects of life. All repression, of course, is not to be considered harmful, for our social life would be impossible unless each individual were taught to give up certain personal liberties to ensure liberty to the group. The author views individuals as dynamic mechanisms amenable to the laws of energy-changes and the resultants of opposing forces just as are objects in the physical world. While such analogies are probably more helpful than to consider individuals as mystic compounds affected by such intangibles as "will," "subconscious memories," "the censor," and so on, no data are offered to support these analogies. *La répression mentale* is essentially a book of philosophical speculations.

University of Pittsburgh J. H. McFadden

The Principles of Reasoning. By Daniel S. Robinson. Second Edition. New York, D. Appleton & Co., 1930. Pp. xviii, 393.

The modifications in the second revision consist in the addition of but little new material here and there and in minor corrections. References are brought up to date and new sets of exercises replace those in the first edition. The addition most interesting to the psychologist is a three-page discussion in which the author's 'implicative system' is identified with the pattern concept of *Gestalt* psychology. Three of Köhler's experiments "to determine the extent to which chimpanzees can reason" are considered. In what the author calls the *integrative experiment*, the problem for the chimpanzee was to complete a pattern which lacks but one link, as taking a stick or other long object to pull a banana within reach; in the *analysis experiment* the animal was to pull a banana within reach by one of two available strings so arranged that the objective would have to be pushed back and to the side behind a wooden frame and then pulled in, rather than being pulled in directly by the other string; and in the *synthesis experiment* two sticks put inside the cage were to be joined together like a jointed fishing pole so that the food could be reached. Robinson contends that in these experiments the solution depended on the animal's "comprehending the way the pattern is to be completed before he performs the action to complete it" (p. 11). This method of simply reasoning from examples of problems solved to the conclusion that certain pattern (or other) relations were apprehended now appears naïve to the student of animal behaviour. It is essentially the method that was early discarded in experiments with animals, for good empirical reasons. The author is not unaware that explanations in terms of purpose are objectionable to science, but holds rather arbitrarily to the need of such explanations in "ethics, legal science, and other social sciences" (p. 322). This persistence in old methods (which have proved inadequate *per se* in experimental research) is worth serious consideration in relation to the relative lack of progress in ethics and 'legal science.' Action with purpose is certainly not characterized by non-predictability or lack of relation to external and bodily conditions. J. P.

The Meaning of Sacrifice. By R. Money-Kyrle. The International Psycho-Analytical Library, No. 16. London, Hogarth Press, 1930. Pp. 273.

This treatise is a contribution to the Freudian literature, attempting "to apply the findings of psychoanalysis in the social field" (p. 9). Its primary thesis is "that sacrifice is a symbolic parricide" (p. 207), a "neurotic symptom" (p. 71).

Incestuous and parricidal desires are assumed, numerous inhibitions compel repression of them, and there result an ambivalence of love and unconscious hate producing a sense of impotence, until by a process of projection and substitution the original parricidal desires are directed toward a chief, a totem, or a god, "the symbolic expression of an unconscious desire for parricide which each individual has acquired for himself" (p. 260). Distribution, modes, and theories of sacrifice are discussed. An exhaustive index is provided, but no bibliography although frequent references are given in footnotes.

This book presents an excellent illustration of the extent to which a favorite theory may be applied by an uncritical enthusiast. It abounds in assumptions. Probably its chief value is that it contributes an elaboration of a single Freudian principle, thus furthering its ultimate acceptance or rejection.

Syracuse University — Ernst Thelin

Value of Marking Hard Spots in Spelling. University of Iowa Studies in Education, Vol V, No. 4. By L. S. Tireman. Iowa City, The University Press, 1930. Pp. 48.

For the study of the influence of marking hard spots in learning to spell, two lists of words were selected from Master's lists of words. The hard spots were indicated to the experimental group by (1) underlining, (2) using capital letters, (3) using bold faced type, or (4) having the child mark his own hard spots. The only other difference in method was this statement to the group mentioned: "Note that a part of each word on the study is marked in a special way. This indicates that it is a hard spot in the word—one that you are apt to misspell." The conclusions are: "After a study of over 4000 pupils of the 4th, 6th, and 8th grades, involving a half million spellings, one is impressed with the consistency with which the data show that marking hard spots is of little or no value. . . . The fact stands out that the pupils who studied words with the hard spots marked made poorer scores than those who studied lists with words unmarked."

University of North Carolina — A. M. Jordan

Non-Language Primary Mental Test for Kindergarten and Grades I and II. By Rudolf Pintner. New York, Teachers College Bureau of Publications, Test Booklet, 1929, pp. 20. Manual of Directions, 1930, pp. 11.

This test is devised for young children in the kindergarten and first two grades. All the directions are to be given in pantomime in the form of a game. Though the author says that the test is difficult to give, the procedure is outlined in clear detail and the test material is simple, easily handled, and interesting.

The test, which might more accurately be called a scale, is made up of four sub-tests including a total of 83 separate test-items. Directions are given for weighting scores on the separate sub-tests, and a table presents norms for changing total weighted scores into years and months of mental age. Norms are based upon responses from 157 children at kindergarten level, 123 children from grade I, and 168 children from grade II.

The reliability of the test, calculated by correlating odd and even items, is given as 0.90 for the first grade (N = 111), and 0.86 for the second grade (N = 129). It is not given for the kindergarten children. Kindergarten scores correlated with Stanford Binet scores (on 80 *Ss*) gave a coefficient of 0.61, and 154 kindergarten and first and second grade scores yielded a correlation of 0.51 with the Pintner-Cunningham test.

Child Guidance Clinic, St. Paul, Minn. GRACE ARTHUR

Test Method Helps, No. 2. Manual for Determining the Equivalence of Mental Ages Obtained from Group Intelligence Tests. By ROSS O. RUNNELS. Yonkers, World Book Co., 1930. Pp. 14.

It is well known that there are enormous variations in mental ages as determined by different group tests. This *Manual* gives "equivalent mental-age norms" worked out for the following group tests: National Intelligence Tests; Haggerty Intelligence Examination, Delta 2; Otis Group Intelligence Scale, Advanced; Terman Group Test of Mental Ability; and Otis Self-Administering Test, Higher. The first forms of these tests were used, but with exceptions noted, the forms may be disregarded. These norms were obtained from the scores of one application of each of the tests to the same 1422 5th to 9th grade pupils within a single school year, practice effects of the several tests being experimentally controlled. Percentile scores were found and graphs made for each test by interpolation, and the average of the mental ages in the five tests for corresponding percentile scores were called composite mental ages. For example, the composite mental age of the 50 percentile (median) was found by averaging the mental age scores of the several tests as found from their own norms in order as named above. Thus 14-7, 15-0, 15-9, etc., gave a mean of 14 years 10 months. This mental age is therefore given in the table opposite the 50 percentile scores of the several tests; and so on, for the other percentile scores. If the IQ is to be determined it is suggested that the formula IQ = MA/CA be used up to the age of 14 years, at which point Runnels supposes mental growth begins to slow up. After 14, a youth's IQ is to be determined by dividing the composite mental age by a purely hypothetical mental age given in the author's Table 3, which does not increase after the chronological age of 16 years. Suggestions for similar procedures with other tests than those used are given.

It may be noted that since the children were selected by grades, rather than by ages, those in the upper grades were probably superior samplings. An important research problem on mental growth by slightly improved but similar methods might be approached from an attitude that is more experimental than the practical one manifested in the construction of the *Manual*. Mental ages above the periods of retarded mental growth might be checked against scores by groups of mature superior children at different IQ levels as determined by earlier tests at 12 years of age. In such a study reliability coefficients (unfortunately neglected here) should be carefully brought up to a very high point. Low reliabilities greatly and erroneously increase individual variations. J. P.

Mental Measurement of Preschool Children. By Rachel Stutsman. Yonkers, World Book Co., 1931. Pp. x, 368.

The book is in four parts: (1) Problems and History of the Mental Testing of Preschool Children; (2) The Merrill-Palmer Research; (3) Guide for Administering the Merrill-Palmer Research; (4) Illustrative Case Studies. Of these, perhaps part 4 will be of more general interest to psychologists; while parts 2 and 3 will be of practical use to the clinical worker.

After summarizing the earlier work in the field, the author describes the Merrill-Palmer scale which is designed to utilize the successful features of other tests, and to avoid other features which have not proven valid. Special care was given, in selecting material, to the factors of inherent interest, variety, variation in difficulty, freedom from environmental influence, simplicity, ease of administering, objectivity, and the usual statistical evidences of validity and reliability. The tests are largely of the performance type, and the statistical work is unusually thorough.

The scale itself consists of 93 test items, ranked according to difficulty, representing a range of 18-71 mo. in mental development. One obtains the impression that clinical workers will have some difficulty in applying the directions for scoring and interpreting the tests although the procedure is explained in detail. While there has been marked improvement on Gesell's method, of which the author is properly critical, the scale would seem to be capable of much greater usefulness if it could be improved in this important respect. The statistical data might more appropriately have been issued as a technical monograph.

Notwithstanding the difficulties pointed out, the author has in the opinion of the reviewer the best available method for measuring the mental development of young children.

University of California at Los Angeles J. Harold Williams

The Field of Philosophy: An Introduction to the Study of Philosophy. By Joseph A. Leighton. 4th Edition. New York, D. Appleton & Co., 1930. Pp. xii, 639.

The chief additions and modifications in this revision bearing on psychology are the new chapter 17, Theories of the Soul or Mind, and some new material on dualism and mind-body relations in chapters 13, 14, and 16. J. P.

An Anthology of Modern Philosophy: Selections for Beginners from the Writings of the Greatest Philosophers from 1500 to 1900. By Daniel S. Robinson. New York, Thomas Y. Crowell Co., 1931. Pp. xiii, 836.

As in the author's *Anthology of Recent Philosophy,* this volume includes analyses before each selection, and at the end of each chapter it gives suggestions and questions for discussion. A biographical sketch of each author quoted, together with references, is given in the appendix. The present volume begins with selections from Paracelsus and ends with T. H. Green. There appear for the first time a number of translations of these masters of thought, and certain rare selections now out of print and mostly inaccessible to students are included. The book will be appreciated by persons who are interested in historical phases of psychology as well as by philosophers and general readers. J. P.

Mahatma Gandhi's Ideas: Including Selections from His Writings. By C. F. ANDREWS. New York, Macmillan Co., 1930. Pp. 382.

This is not a scientific book. While it attempts to present and evaluate Gandhi's views and something of the complicated religious and race difficulties in India, it is inclined to evaluate them from a particular (the Christian) point of view, even though it represents Gandhi as having a humanitarian viewpoint that is more comprehensive than that of any religion. J. P.

Personality: The Crux of Social Intercourse. By A. A. ROBACK. Cambridge, Mass., Sci-Art Publishers, 1931. Pp. 144.

This little volume is an expansion, for elementary students in psychology and intelligent lay readers, of the author's article of the same title published in the *Century*, 1929. It is intended as "a summary of the work and a description of the methods employed recently in the study of personality" (p. 7), but as a matter of fact very little is said anywhere of scientific method. The book is wholly qualitative and contains nothing new to psychologists, either as to content or as to point of view; and no such contributions are intended. More advanced students are referred by the author to his larger volume, *The Psychology of Character*, 1927. There is a bibliography of 128 titles. J. P.

Psychological Service for School Problems. By GERTRUDE H. HILDRETH. Yonkers, World Book Co., 1930. Pp. xiii, 317.

This book describes the applications of educational psychology to the problems of the school. It is more concerned with the situations in which psychological service is demanded than with the fundamental principles of educational psychology. The principles and technique of psychological measurement are, however, described in some detail. The study, classification, and treatment of exceptional pupils are described primarily as problems of the school, but in the light of psychological principles.

The chief value of the book lies not so much in the information it gives to the students of psychology, as in the psychological approach it gives to the student of school problems.

Duke University HOWARD EASLEY

Exploration psychologique infantile par l'expression graphique du mouvement. By MAURICE DIDE and MATHILDE GUILHEM. Extrait des Memoires de L'Academie des Sciences, Inscriptions et Belles-Lettres de Toulouse, Douzieme Serie, Tome 7, pp. 247-259. Toulouse, J. Bonnet, 1929.

The authors report here their investigation of the development of the ability of graphic representation in the young child. The investigation includes their observations of some 50 children over a period of several years. A number of situations, and some typical reactions to them, are described, such as: (1) Pseudo-schematization of attitudes. Here the child is asked to assume a position, *e.g.* arms extended, and then to draw immediately what he has done. Representations in this class include merely direction and approximate spatial relation of gross features. (2) Pseudo-schematization of movement. Here the child is asked to execute some gross movement and then draw it. The responses indicate simply direction of movement, and do not represent parts of the body as moving. (3) Cinematographic representation of movement. Here the drawings illustrate movements by drawing the moving members in different positions. These three stages are illustrated with description of typical cases, and also with some original drawings of the children.

The conclusions concerning the developmental relation of these processes to various mental processes seem to be based upon the whole of the observations, only a few of which are reported. Enough are reported, however, to indicate the significance of the method of investigation.

Duke University HOWARD EASLEY

Statistical Tables for Students of Education and Psychology. By CARL J. HOLZINGER. Third Edition. Chicago, University of Chicago Press, 1931. Pp. viii, 101.

In the present edition Table XX is added to the tables found in the second revision (1928). This new table covers pages 82-101, and facilitates the calculation of partial correlations by giving the values of the denominator $[(1-r^2_{13})(1-r^2_{23})]^{-\frac{1}{2}}$. Numerator values like $(r_{12}-r_{13}r_{23})$ are worked out arithmetically to three decimal places and are then multipled by the denominator value which is read off directly from the table. Table XX covers all values for the two correlations indicated from 0.00 to 0.99. By using r values that are correct to three decimal places similar values for the partial correlations may be interpolated from Table XX. With the additional use of the Crelle *Calculating Tables*, partial correlations may be worked out with considerable speed. J. P.

The Book of My Life (De vita propria liber). By GIROLAMA CARDANO. Translated from the Latin by Jean Stoner. New York, E. P. Dutton & Co., 1930. Pp. xvi, 331.

This book is an English version of the autobiography of the celebrated Milanese physician, mathematician, and scientist. Cardan was a contemporary of Cellini and a friend of Vesalius. However he knew very little about anatomy and surgery, but was sympathetic with the struggles of the great pioneer of race craniology (Vesalius) and once refused an offer to work with him.

The volume is of special interest to psychologists in that we find in the author such an unusual combination of personality traits—a scientist, who is at the same time morbid and superstitious, extremely sensitive and famished for fame, yet withal remarkably honest and self searching. At times he was something of a paranoiac, frequently having persecutory and grandiose delusions of a more or less mild degree. But in spite of these eccentricities (and his ignorance of anatomy), Cardan was deeply interested in the brain and nervous system, and their relation to physical and intellectual life. He was perhaps the first man to sit in clinic and ponder his own morbid experiences—he was psychiatrist and patient in one.

The book will probably rank as one of the few great autobiographies in literature. The translation is excellent and the notes very helpful. A bibliography of thirty-two references is given.

University of South Carolina W. F. SMITH

Les trois ages de la science. By BARON DESCAMPS. Extrait des Bulletins de la Classe des Letters et des Sciences morales et politiques. 5e serie, 1. xvi, No. 5. Bruxelles, Marcel Hayez, 1930. Pp. 257-273.

This is a lecture given by the author before the Royal Academy of Belgium. The first half of the discussion is devoted to a critical evaluation of the concepts of Descartes, Kant, and Auguste Comte, and the effects of these concepts on scientific progress. The three ages of science are designated as (1) the age of hegemonic concentration, (2) the age of autonomous separation, and (3) the age of harmonious expansion. The first age is characteristic of the past, the second of the present, and the third of the future. As a psychological inquiry the paper has relatively little value from an empirical point of view. It is of value, however, as a philosophical orientation to Bergsonian metaphysics.

University of South Carolina W. F. SMITH

L'ologénèse: Nouvelle théorie de l'évolution et de la distribution géographique des êtres vivants. By DANIEL ROSA. Paris, Félix Alcan, 1931. Pp. ii, 368.

In this volume Rosa, of Florence, proposes a new theory of evolution which is also suggested to account for the geographical distribution of living forms. According to the view proposed in the book every species is predetermined in preceding species, as each individual is predetermined in the egg from which it develops. The book deals successively with an explicit and detailed statement of this theory and of the relationship of the theory to other theories of evolution and also to various evolutionary concepts such as that of regression.

The reviewer finds in this volume little of direct or inferential value to the study of the inheritance of psychological characteristics.

Brown University LEONARD CARMICHAEL

Test Method Helps No. 3. Expressing Educational Measures as Percentile Ranks. By FRANCIS C. BUROS and OSCAR K. BUROS. Yonkers, World Book Co., 1930. Pp. 30.

In obtaining the percentile rank this method requires that all the cases be ranked and then the ranks looked up in a table under the appropriate number of cases. The percentile rank of an individual is taken to be the mid-point of the interval in which he appears, thus with ten cases the highest percentile rank is taken at 95. For practical purposes a simpler way to compute percentile ranks is to construct an accumulative frequency table, obtain the reciprocal of one case to three decimals, set Crelle's Rechentafeln at this number, and read off the percentages for each interval.

University of North Carolina A. M. JORDAN

Instructional Tests in Chemistry. By EARL R. GLENN and LOUIS E. WELTON. Yonkers, World Book Co., 1930. Pp. iv, 76.

A comprehensive set of questions has been prepared on 36 different topics in Elementary Chemistry. The items under each topic vary in number from 18, which consist largely of problems, to 80 which consist of writing symbols and formulas. Reliabilities furnished for each topic range from 0.70 to 0.88. Norms for each fifth percentile are published for each of the 36 tests.

University of North Carolina A. M. JORDAN

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By J. P. GUILFORD, University of Nebraska

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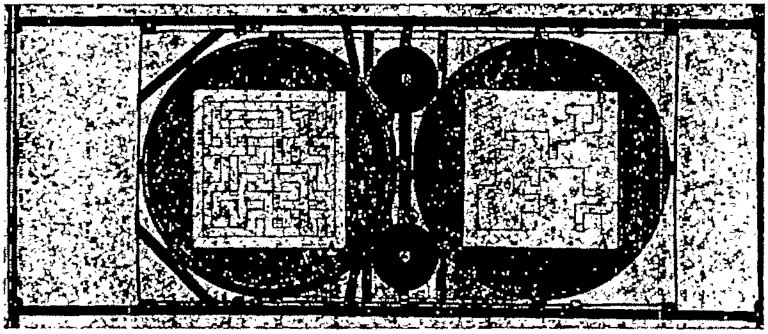

FIG. 1. SELF-RECORDING HUMAN MAZE